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SOME RECENT EXPERIMENTS ON THE USE OF HIGH EXPLOSIVES FOR WAR PURPOSES.

By Prof. CHAS. E. MUNROE, U. S. N. A.

Contributed to Van Nostrand's Engineering Magazine.

Since the processes have been perfected by which nitroglycerine and gun-cotton have been rendered comparatively safe during handling and storage, active efforts have been made to devise means for employing them as offensive and defensive agents. Heretofore these efforts have been confined to the use of these explosives in torpedoes and submarine mines, but while these have proved in some measure efficient, yet they have been found to be costly and difficult of application, especially when the enemy was in motion, while the apparatus could be used but once, since it was destroyed by its own explosion. Hence, attention is now directed towards the invention of methods which will combine accuracy and readiness of application to a desired point while admitting of rapid and repeated use. As gun-powder has been successfully used in shells, this method of projecting the high explosives has naturally suggested itself; but as ,these bodies have been found to be liable to undergo an explosive reaction if, when confined, they are exposed to a violent shock or to the influence of an explosive wave, and as the pressure exerted by the powder-gas, in the chamber of a gun, becomes very great before the projectile is moved from its seat, it has been generally feared that a shell charged with a high explosive would be exploded before it left the gun.

For this reason other means for propelling the projectile have been sought, and as it has been shown by the Perkins steam gun and the common air gun that the expansive force of high pressure steam, or of compressed gases, possesses for this purpose a considerable degree of efficiency, while at the same time the pressure can be so regulated that the projectile can be started without being subjected to any considerable initial shock, this form of gun has been recently adopted for experimental purposes. The velocity is attained by lengthening the gun so that the projectile is exposed for some time to the action of the expanding gases.

In Fig. 1,* the air gun recently under trial at Fort Lafayette, New York Harbor, is represented. It consists of a seamless brass tube forty feet long and one quarter inch in thickness, mounted upon a light steel girder. This is trunnioned and pivoted on to a cast iron base, thus enabling it to be swung in any desired position. To assist in this operation guys are placed on either side of the base and their length can be changed by turning the hand wheels. Compressed air is introduced to the gun from below and

* We are indebted to the *Scientific American* of April 8, 1884, for Figs. 1 and 2.

passes up through the center of the base to a pipe connecting with one of the trunnions, which is hollow. The air is thence introduced into the pipe on the side of the gun which leads to the valve. This valve is a continuation of the breech of the gun with which it is connected by a pipe leading upward. The valve is automatic in its action, and so regulates the supply of air that the pressure increases until the projectile leaves the gun, when the value closes.

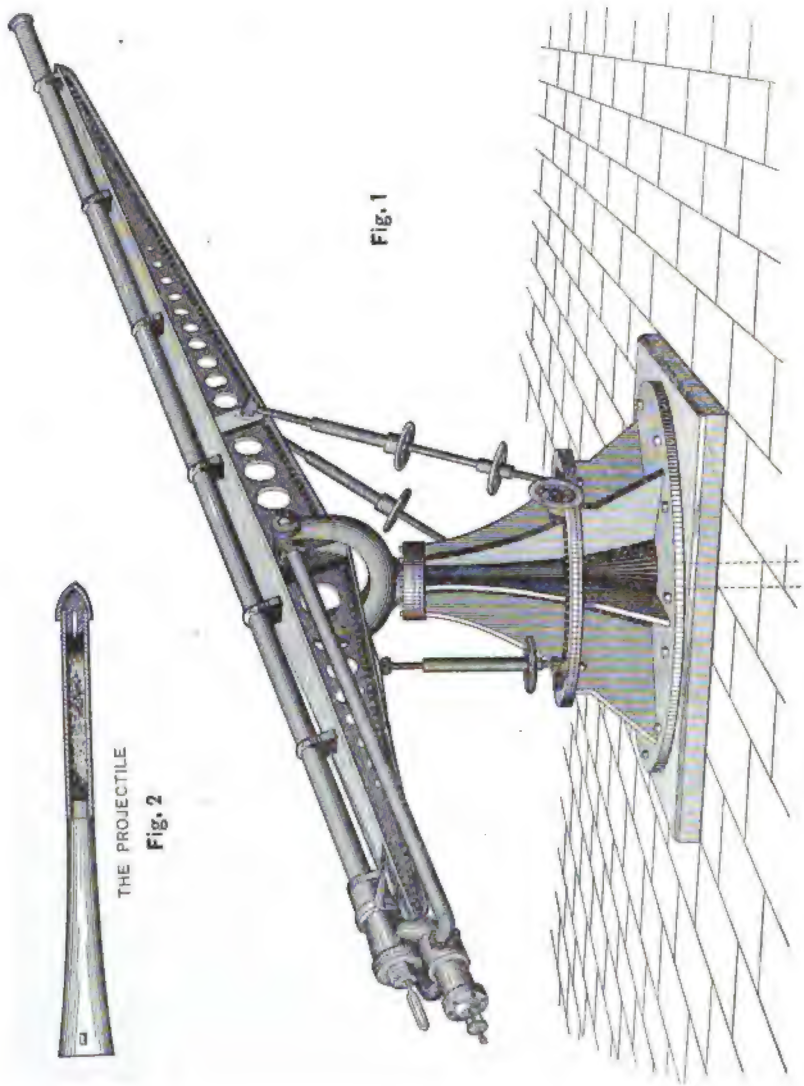

Fig. 1

THE PROJECTILE

Fig. 2

The peculiar projectile used is shown in Fig. 2. It consists of a thin brass tube in which the dynamite charge is placed, an airspace is left at the rear to act as a cushion and then a wooden sabot is inserted. This sabot flares out until its diameter equals the internal diameter of the gun. The forward end of the brass tube is pointed with some soft material and the firing pin with its detonator is embedded in this, so that it cannot be fired except upon impact. The projectile is so constructed that its center of gravity is some distance forward of its center of figure. Hence a side wind would tend to turn its head into the wind and keep it

in the line of its trajectory. The gun is loaded by inserting the projectile in the breech and closing the gas check. The lever is then moved which opens the valve, admits the compressed air and discharges the gun.

The first gun tested by Lt. Zalinski was two inches in diameter, and with this a range of 1¼ mile was attained when using a pressure of 420 pounds to the sq. inch. The four-inch gun is made to withstand a pressure of 2000 pounds, but as thus far reported the pressure used but slightly exceeded 500 pounds. With a projectile weighing 24 pounds and a pressure of 500 pounds, a range of 2100 yards was attained when the elevation was 22 degrees. Numerous experiments have been made with projectiles containing dynamite, in some instances 15 pounds being used for one charge, and they have been fired without any premature explosions. The gun, however, yet appears to lack in accuracy of fire. This can probably be corrected after further experimentation, and the progress thus far made has shown that a gun can be cheaply and quickly constructed which will safely and rapidly throw considerable masses of the high explosives to a distance of over 1¼ miles, where they will be discharged on impact. These guns are also so light that they can easily be used on small launches, and the absence of all report and of a flash of light during the discharge, commends them for night attacks. It is proposed now to construct an eight-inch gun to carry from 100 to 125 pounds of explosive gelatine.

In determining the efficiency of these projectiles against an armor clad vessel, we may assume that one of four effects may be produced, depending on the resistance of the armor to penetration and on the material, thickness of wall, profile, weight and velocity of the projectile.

(1). The projectile may either penetrate the armor partially and explode in place, or pierce it completely and burst inside of the ship. This is the condition of greatest efficiency.

(2). It may explode immediately upon impact and before breaking up. Then the explosive will exert the energy which it develops through explosion in a resisting receptacle.

(3). It may rebound before exploding. Then the effect will be reduced by the interposed cushion of air.

(4). It may break up on impact before the explosion takes place. Then the energy of the explosive will be simply that which it develops when exploded unconfined.

The resistance of an armor to penetration depends upon its hardness, its tensile strength (that due to bolting as well as that inherent in the metal itself) and its inertia. The latter is augmented by the thickness and weight of the armor, and by the rigid system of bracing which now obtains in practice. How great this resistance is can best be illustrated by an example. While, from the fact that very rapid progress is being made in the improvement of armor plates, we may not have chosen the best example, let us take the steel plates designed for the *Furieux*. One of these, weighing 23 tons, 9 cwt, and averaging over 17 inches in thickness, was tested at Gâvre, July 13, 1883. Three shots were fired against this plate from a 12.6 inch rifle using chilled iron projectiles, weighing 759 pounds each. The first and second shot struck with a velocity of 1403 feet each. The third struck with a velocity of 1438 feet. The projectiles were all broken up, all of the twenty bolts through the plate remained intact, and no portion of the plate fell from the backing, although it was somewhat indented and cracked.

Although we are not yet informed concerning the air gun projectile, except for the weight given above and the pressure of the air also cited, yet when we remember that in the Gâvre experiments the pressure of the powder gas probably approached 40,000 lbs. to the square inch, it is not unfair to infer that with a pressure of 500 lbs. to the square inch a projectile will possess little or no penetrative power against the *Furieux* plates at a distance of 1¼ mile. Whether then the projectile would explode on impact or after rebounding, or whether it would break up before exploding, is a matter for speculation and conjecture. If the last condition prevails, then we can judge from some experiments recently made by Lieut.-Commander Folger, at the Naval Experimental Battery, under the direction of the Naval Bureau of Ordnance, what the destructive effect would probably be.

The target used in these experiments

is shown in Fig. 3. It consisted of eleven slightly convex iron plates, each one inch in thickness, which had been perforated with bolt holes. They were such plates as were used during the war for covering the turrets of the original monitors, and the bolt holes were made before the delivery of the plates. The plates were fastened to the twenty-inch thick oak backing by fourteen, tapering, 1½ inch iron bolts, which were flush on the surface of the target and passed entirely through the backing. These bolts are indicated in the figures by numbers. Before this target was constructed, 265 pounds of dynamite in ten successive charges, varying from five to seventy-five pounds, were exploded against plates fastened to this same structure without materially damaging it.

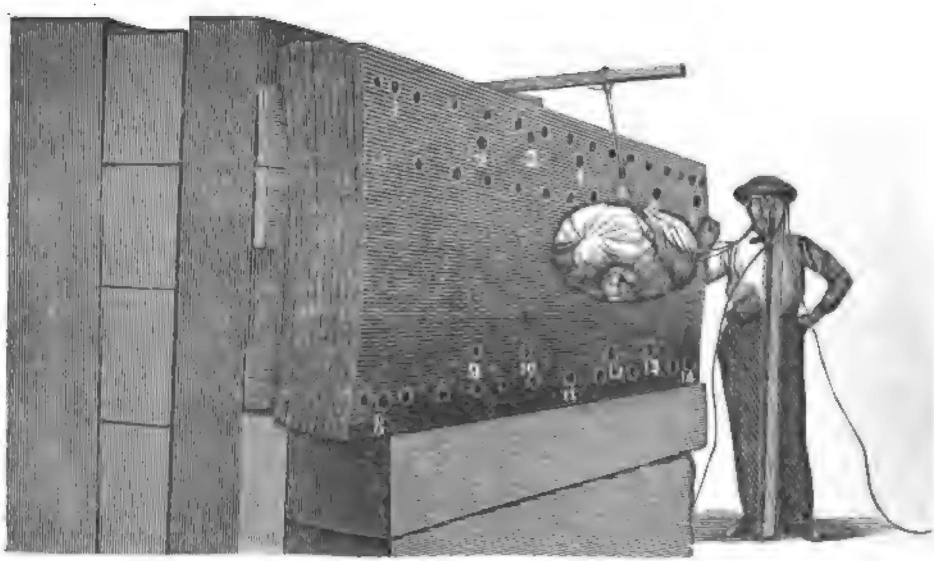

FIG. 3.

The No. 1 kisselguhr dynamite used was made at Newport, and was in excellent condition. The charge was placed in a woolen cartridge bag, a netting lashed about it and then the whole suspended from the wooden cross-piece which projected from above the target, as shown in Fig. 3. The cord suspending the charge was carried over and behind the top of the plates so as to bring the charge in contact with the face of the plates, and, as an additional precaution, the charge was jammed close against the target by pressing on it. A hole, about three inches deep, was made in the side of the charge farthest from the target, and two detonators, arranged in series, and containing together 85 grains of mercuric fulminate, were imbedded in it.

In the first experiment 100 pounds of dynamite were exploded as described. The result was the indentation of the plate to the depth of two inches at the center, and extending over a circular area of about two feet in diameter. The outer plate was sprung from the bolts at each end. The outer pair of plates separated about a quarter of an inch. In general, however, a good contact remained for all the plates. The backing and bracing were practically uninjured. An old crack in the middle upright, due to checking, had widened slightly. The whole structure had sprung back two inches, but had recovered itself in the marshy ground in which it was imbedded.

In the next experiment a one-inch iron plate was placed horizontally in contact with the target, as shown in Fig. 4, and a charge of 75 pounds of dynamite was forced tightly into the angle between the plates and exploded. The result was that the indentation in the target was increased by about one inch, but all the plates and bolts remained unbroken. The ends of the plates curved outward, the outer one

about one inch. The structure was somewhat racked and strained but still remained quite solid. It had as before sprung back about two inches. A portion of the backing on the left, which had been much weakened by bolts passed through it, was broken off and partially displaced. The effects on the backing were somewhat more pronouced than in the previous round, but this was probably as much due to the repeated blows of 440 pounds of dynamite as to the particular position of the charge in the last experiment. The platform of blocks and the iron plate, which in the experiment, represented the water surface in contact with the armor, was demolished, the plate being badly torn and the wood pulverized. The result is shown in Fig. 5. Mr. Folger concludes from these experiments, "that it is a matter which hardly admits of doubt, that a modern armor-clad will not be materially injured by the explosion, in superficial contact with her overwater plating, of charges of more than 100 pounds of dynamite." We would add to this that in the case of the breaking up of a projectile before exploding, the explosive material may be scattered over a considerable area and thus diminish the effect produced at any given point.

On turning again to the description of the air gun projectile, it will be observed that, as with nearly all of the shells in use which are to be exploded by impact, the detonator is placed in front of the explosive. As we are accustomed to regard the explosion of our high explosives, when induced by detonation, as instantaneous throughout the mass, it has seemed a matter of no importance, as regards its effect on the explosive, where the detonator was put. Berthelot has, however, advanced the theory that while the reaction started by the first shock in a given explosive material is propagated with a rapidity which depends upon the intensity of the shock, and while this intensity

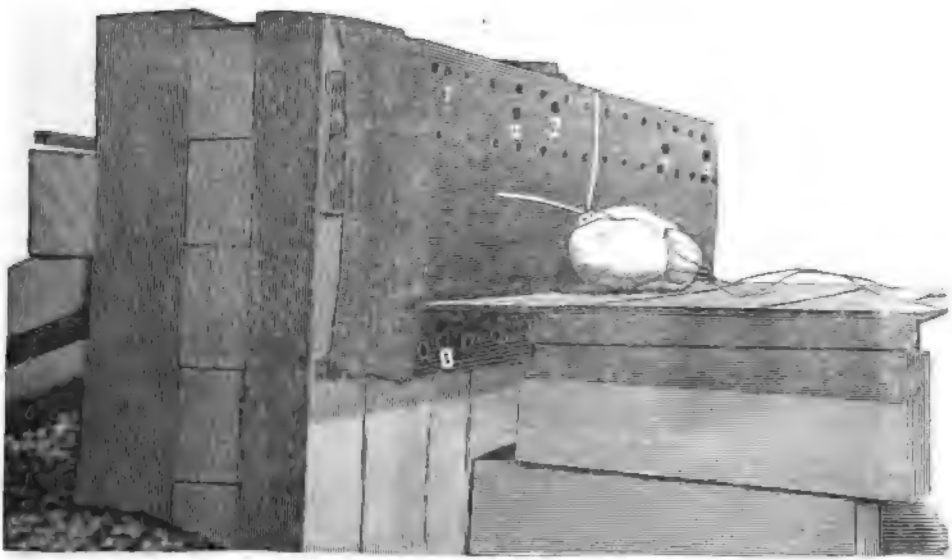

FIG. 4.

may vary considerably according to the method by which it is produced, yet the pressures which arise from the shock exerted on the surface of the explosive are too rapid to become uniformly dispersed throughout the entire mass, and the transformation takes place locally among the layers first reached. If it is sufficiently violent they may then be heated to the necessary temperature, and they will be immediately decomposed and produce a large quantity of gas. This production of gas is in its turn so violent that the shocking body has not time to displace itself, and the sudden expansion of the gases of explosion produces a new shock,

probably more violent than the first, on the layer situated below. The mechanical energy of this shock is changed into heat, in the layers which it reaches, and produces an explosion; and this alternation between a shock developing mechanical energy which changes into heat, and a production of heat which elevates the temperature of the layers up to the degree necessary for a new explosion capable of reproducing the shock, propagates the reaction, molecule by molecule, through the entire mass. The propagation of the explosion takes place in this way in consequence of phenomena comparable to those which produce a sonorous wave.

In view of this theory experiments were made at the Naval Experimental Battery. Two charges of dynamite of ten pounds each were inclosed in woolen bags, suspended as in the previous experiments, and exploded successively before the one inch iron plate shown in Fig. 6. The two experiments differed only in the fact that while in the first the exploder was inserted in the side of the charge farthest from the face of the plate, in the second case the exploder was inserted in the side of the charge in contact with the plate. The result of the first explosion is shown on the left hand of the figure. The area of the hole was about 192 square inches and the edges were quite cleanly cut. The result of the second explosion is shown on the right hand. The area of the hole was about 96 square inches, and the sides were pushed outward as if subjected to a slow and somewhat continued pressure.

In connection with his tests of the dynamite air gun, Lt. Zalinski has also tried exploding dynamite in contact with iron plates. The two experiments reported are as follows: Fifteen seven-eighths inch iron plates were piled upon one another. Upon these an iron canister, containing twenty pounds of untamped dynamite in four paper cartridges was placed.

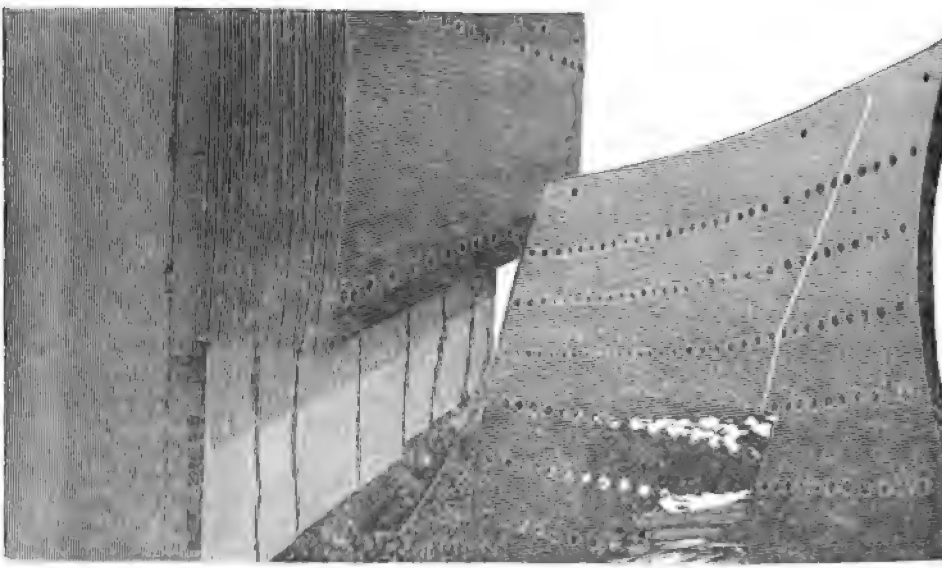

FIG. 5.

About this the remains of four large iron boilers were placed as shields, and then the dynamite was exploded. The result was that plate No. 1 was shattered, plates Nos. 2 and 3 were crushed, plate 4 fell to pieces when lifted, while 5, 6, and 7 all showed the effects of the blast. The iron boilers were blown to a distance of several yards, while the ground was torn up to some distance.

In the second experiment a pile of fifteen, five-eighths inch plates, was used and seventy pounds of tamped dynamite, enclosed in a cylinder, was placed upon it. The boilers were placed about the pile and the charge exploded. A large

hole was blown through twelve of the plates and the boilers were blown to a distance of thirty yards and riddled.

A somewhat similar experiment in which gun cotton was substituted for dynamite was made by Lieut.-Commander Folger. The target represented in strength the protected deck of a modern armored vessel. Three one-inch iron plates were firmly joined together by eight one-inch bolts. Heavy timbers, serving as a support, were placed upon the ground at a distance of $4\frac{1}{2}$ feet from one another, and the plate was laid horizontally upon them, and fastened to them by two one-inch bolts at each end, which passed nearly through the timbers. The plates were of the same lot as those used in the other targets at the Experimental Battery, and in order to counteract as far as might be the weakening effect of the bolt holes, plates of iron, one-fourth inch in thickness were inserted on either side of the middle plate, and a third one was placed above the top plate. There was thus added a strength of three-fourth inch of iron.

After a number of preliminary trials to determine the best means for producing a perfect detonation had been made, a charge of 25 pounds of wet gun cotton tied in a bag, with 20 ounces of dry gun cotton and 80 grains of mercuric fulminate as an exploder, were placed on the target. The result of the explosion is shown in Figs. 7 and 8. A hole, 56 square inches in area, was blown through all the plates and a hole, 18 inches deep, was blown in the rather friable earth beneath. The bending of the plates extended from the timbers, without starting the bolts, and reached a depth of indent of $7\frac{1}{2}$ inches at the hole. *The area of the base of the charge, before explosion, was about double that of the hole made.* The wet gun cotton used was compressed in the form of discs, and it is a most curious fact that the area of the base of the charge beyond the limit of the hole was indented by the discs of gun cotton in contact with

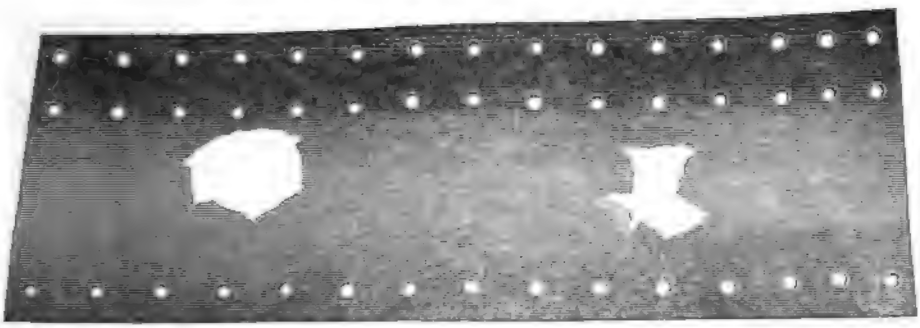

FIG. 6.

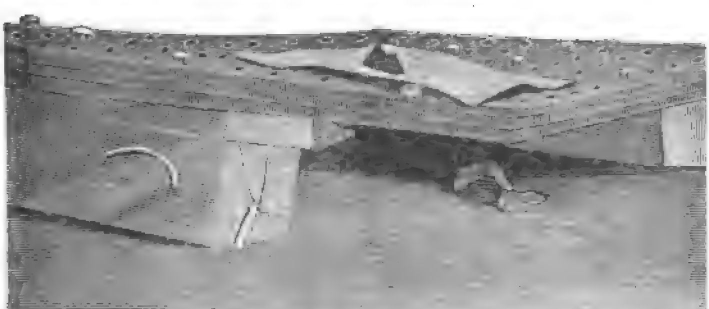

FIG. 7.

the plate before these discs exploded. As the exploder was placed in the top of the charge this observation seems to corroborate the results obtained when testing Berthelot's theory as to the transmission of the explosive reaction. The impressions of the gun cotton discs on the iron plate are shown in Fig. 8.

It will be observed that in each of the last described experiments the plates were far from being rigidly supported, and hence offered less resistance to a rupturing blow than they would have done if well braced. But as the target was arranged in the last experiment it fairly represented the somewhat flexible deck of an armored ship, and showed the degree of vulnerability of the weakest point, and what might result if we could reach that point with our explosive projectile.

Fig. 8.

At the beginning of this article we have said that it had been feared that the high explosives could not be fired in a shell with gunpowder as a motor without danger of a premature explosion in the bore of the gun. Abel, however, has long held other views, and some years ago he publicly expressed the belief that gun cotton could be fired thus without danger, but that no one had the courage to try it. The experiment has now been successfully made with several of the high explosives, and even so long ago as 1874 dynamite was so used under the direction of the Naval Bureau of Ordnance, the experiments on the high explosives made at the Experimental Battery, as reported here, being merely a continuation of these earlier experiments. More recently an experiment is reported as having been made in April last, at Greenville, N. Y., by F. H. Snyder. He fired dynamite by the aid of a slowly accelerating powder and a peculiar buffer devised by him. The cannon used was a brass field piece of about 4½ inches bore. A charge of a pound and a half of powder was put in, and this was followed by a sabot formed from alternate layers of iron, leather, copper and paper, and made to fit the bore nicely. Next to this was a brass ring holding a rubber plug which was perforated with chambers at one end. Next came a long cylinder weighing thirteen pounds and containing five pounds of dynamite in the end. The object in using the perforated rubber plug was to take advantage of the compression of the air confined in the cavities for gradually transmitting the force of the explosion to the dynamite projectile. The wooden sabot of the dynamite projectile was winged so as to increase its accuracy of flight. In

the experiments the dynamite was fired without any premature explosions, but no great range or efficiency seems to have been attained.

Another experiment is reported from Sandy Hook in July last. Here 5½ lbs. of explosive gelatine were fired in an 8-inch dynamite shell. The gelatine was inclosed in a thick pasteboard cylinder, which was divided into four sections by two intersecting partitions. There was a cushion of cork in the shell and a hollow rubber cylinder was placed between the charge and the shell. The paper case and the inside of the shell were well covered with graphite. There was no detonator in the shell, and it was probably intended to explode the gelatine by its own impact. The account of the experiments is meagre, but it said that service charges of powder (35 lbs.) were used for firing the shells. Two rounds were fired. In the first, the shot went successfully to the target, and the point indented the iron about seven inches and exploded, but did no harm. The next shell got as far as the muzzle of the gun and burst in the bore but doing no other injury, apparently, than scoring the rifling. These statements are made as reported, but it is quite probable that the shell broke up in both cases without any explosion of the gelatine.

Experiments in this direction have also been made at the Naval Experimental Battery. Some time since the trials were made with dynamite. In this instance, after a few preliminary experiments had been made (in which twelve rounds were fired using considerable air space, reduced charges, and a small quantity of oakum in the bottom of the shell to serve as a cushion) ten charges of dynamite of 5½ ounces each were fired in 12-pounder spherical shells under service conditions, that is, with a full charge of powder and no air space. There was no premature explosion with any of the twenty-two charges fired, though one shell exploded on impact with the water. Five of the shells carried ordinary time fuses, and these exploded at the point and time intended.

Finally, on August 4, experiments were made at the same place in firing gun-cotton. After five preliminary rounds, ten rounds were fired, at a range of about 2,000 yards, with the 80-pounder (6.4 inch) B. L. R. using full service charges (10 pounds) of powder, and shells filled with wet gun-cotton (3 pounds). The only precaution taken to relieve the shock at starting was in placing a layer of oakum ¼ inch thick in the bottom of the shells. The shells were plugged with wood as if it was dangerous to explode them down the bay on account of the great energy imparted to the fragments when high explosives are used as bursting charges, and as it has been demonstrated in previous experiments that it was feasible to explode gun-cotton shells at a desired range. Three rounds were fired, without fuses, at the target. The gun was fifty yards off, and the velocity of impact was about 1250 f. s. The shells exploded on impact, as was shown by the small size of the fragments, by the fan-like spread of the scoring on the interior of the shelter erected about the target and by the sound of the explosion which was heard. The effect on the target in each case was an indentation about 2 inches deep, with a diameter of 10 inches. No plates were injured. There was no case of premature explosion of the projectile in any of these experiments. It thus appears perfectly practicable to fire with safety, and under service conditions, charges of wet gun cotton from a bore of considerable size.

The World's Telegraphs.—The telegraph appears to have made more progress in the United States than in any other country. The number of American telegraph offices in 1882 was 12,917, and the number of telegrams forwarded during the year was 40,581,177. The number of telegraph offices in Great Britain and Ireland in 1882 was 5,747, the number of telegrams forwarded being 32,965,029. Germany had 10,808 offices, the number of telegrams forwarded being 18,362,178. France had 6,319 offices, the number of telegrams forwarded being 26,260,124. Russia had 2,819 offices, the number of telegrams forwarded being 9,800,201. Belgium had 835 offices, the number of telegrams forwarded being 4,066,843. Spain had 647 offices, the number of telegrams forwarded being 2,830,186. British India had 1,025 offices. the number of telegrams forwarded being 2,032,603. Switzerland had 1,160 offices, Italy 2,590, and Austria 2,696. The number of telegrams forwarded in these three last-mentioned countries was 3,046,182, 7,026,287, and 6,626,203 respectively.

THE VENTILATION OF BUILDINGS.

By J. NEVILLE PORTER.

From "The Building News."

One of our most important sanitary requirements is the efficient ventilation of inclosed spaces in large buildings of various kinds, as more dangerous diseases and many more deaths occur from the want of and defective ventilating arrangements of these inclosures than from other insanitary causes. Many persons, who take much interest in sanitary subjects may perhaps question the truth of the statement, on the ground that such a deplorable grievance has not been notified to them in any forcible or even moderately appreciable manner compared with the horrible housing of the very poor, and other leading hygienic subjects. The reason for this grave omission appears to be that very great difference of opinion prevails among sanitarians, architects, builders and medical men concerning defective and efficient ventilation of buildings, and we are pestered with innumerable false and ridiculous opinions on artificial ventilation. I have adopted a novel and somewhat arduous mode of qualifying myself for dealing with the subject. In addition to referring to a great mass of information on the question scattered through numerous British and foreign publications of different kinds, I have inspected the ventilating arrangements of almost all classes of public and other buildings in the Metropolis, Manchester, Liverpool, Birmingham, Wolverhampton, Walsall, Oxford, Cambridge, and other large towns. I have also had consultations with architects, builders, city and borough surveyors, sanitary patentees, and scientists on the subject. Having referred to the composition and vitiation of the atmosphere, the lecturer added: When compartments within buildings are dependent for ventilation from windows and doors only, they are rightly said to be almost hermetically sealed in bad weather, as these apertures cannot then be endured. Many rooms are excluded from ventilation in cold weather, and others again nearly precluded from fresh air by stopping up indifferent and bad artificial inlets and outlets. Moreover, I have noticed many occupied rooms unprovided with such inlets, and some which had no artificial outlets. No room in which there are human beings for hours at a time can be properly ventilated unless it is provided with artificial inlets and outlets of particular relative sizes, and fixed in certain relative positions, and also with efficient means for duly regulating the supply of fresh air admitted through such inlets, and warming it in winter. It would be impossible to lay down hard and fast lines for the various proportions of inlets and outlets, as much depends upon local conditions, the amount of fresh air required in a room according to the number of occupants, the amount of gas consumed, and vitiated air from other causes than respiration and fires and lights, including foul gases from chemical substances, goods stowed in warehouses, &c. It is also essential that there should be no unpleasant draughts from the inlets. Opinions materially differ as to how this requirement should be effected, and, therefore, several means have been adopted for this purpose, nearly all of which have failed. The most difficult inclosed spaces to ventilate are small rooms with low ceilings occupied for several hours by many persons, and having fires, lamps, or gas; and larger rooms in which the cubic space of air to each individual is 500 ft. or less; inasmuch as the air in these compartments should be frequently changed without perceptible movement. Dr. Parkes tells us that "in barracks with 600 cubic feet per head the rooms are cold and draughty when anything approaching to 3,000 cubic feet of fresh air per head are passing through. That is a change of five times per hour for each 600 cubic feet of air space; a change equal to three times an hour is generally all that can be borne under the conditions of warming, or that is practically attainable." The best method of

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artificially ventilating inclosed spaces in buildings not having galleries is by causing the fresh air to enter through grated openings outside the wall at or close to the floor level through each of such apertures; the fresh air should enter a vertical, commonly called a "Tobin," tube, extending about 7ft. to prevent draughts, and close to the inside wall; and in the tube a valve should be fixed to regulate the supply of the incoming air. The natural pressure of the atmosphere, and the additional velocity given to it in passing through a long, narrow pipe, cause it to generally continue as a direct current about equal to the dimensions of the tube for several feet before spreading over the upper part of the room, in which it is warmed and gradually descends to the occupants of the compartment. The height of these tubes should be so fixed that the fresh air from them will not be suddenly deflected by the ceiling so as to fall in a kind of shower before being mixed with the air in the room. Inlet-tubes should be numerous and small, and equally distributed at the sides, or more, if possible, in large rooms, so that the air from them may be properly mixed according to the law of gaseous diffusion. The area of their orifices should also be more than that of the artificial outlets. When one side of a large room does not communicate with the open air, pipes or flues under the floor from the opposite wall-gratings should be led into "Tobin" tubes for the admission of fresh air thereto, as is done in the board room of the Paddington Vestry Hall. It is also highly important that in cold weather the fresh air admitted into large rooms and other inclosed spaces occupied for hours by several individuals should be warmed; otherwise unendurable and injurious draughts will be experienced. If the fresh air cannot be warmed it should not be let in at the skirting or floor level, as it could not be borne in bad weather if it enters at great velocity; nor should the external atmosphere be admitted very near the top of the room, so as to rebound from the ceiling and fall on the head in a shower, and no flap or hopper will prevent this result; consequently the "Sherringham" valve is generally fixed near the ceiling, and admitting the outward air through a perforated brick, or iron plate, and driving it upwards is of very little use, although it often acts as an outlet and can be closed when required. Air inlets through slits in sashes, or through louvre or hopper openings in windows, as frequently noticed in hospital wards, are inefficient for ventilation, as the admitted air is neither warmed nor can be properly mixed with that in the room; therefore severe down-draughts will be felt. The inlet from McKinnell's perpendicular tube at the top of the room is objectionable, as it acts as an inlet and an outlet in a fitful way. When fresh air cannot be directly admitted at or near the floor level on account of the contaminated atmosphere close to its exterior, occasioned by offensive trades or otherwise, pure air should, when practicable, be let in from one or two perpendicular shafts reaching above the top of the building, such shafts communicating with a flue at their base, and led from this channel through tubes or grates into the building at any desired point. The method adopted by Captain Douglas Galton of warming cold outer air in openings at the back of the firegrates, and from thence admitting it into rooms is extensively adopted, and in many respects is appreciable; but his plan would be improved if a portion of the warmed air could be admitted into the center of the compartments through flues or under the floor, inasmuch as the heated air from the openings above the fireplace does not appear to mix freely with the air in the rooms so as to cause nearly an equal degree of heat among the occupants therein. It would not be practicable to cause anything like a sufficient amount of warmed air to be conducted into the room from apertures opposite the fireplace from the Galton stoves, compared with that emitted immediately over the grate, unless the heated air was propelled. I am strengthened in this opinion from the fact that in several of the new barracks where these stoves are used there are Sherringham valve inlets opposite to them and close to the ceiling for the admission of cold air and which is not properly diffused in the room. For this reason the cold air is admitted into the wards of the London Hospital through the numerous apertures in the window sills; the warm air from the back of the fire-grates at the opposite side is

not a good system of ventilation, even if the artificial outlets were excellent. Buildings containing galleries are far more difficult to efficiently ventilate and warm than other inclosed spaces. It appears that the best plan to admit the air into the former inclosures is through horizontal flues or pipes under the floors of the galleries into pillars or into perpendicular pipes connected therewith, so that the fresh air will be admitted over the heads of the people, and thus no unpleasant draughts can be experienced. If the pure air is properly warmed, it may with good effect be also introduced under the galleries as well as above them, and thus increase the more rapid distribution of the fresh air through the building. Under the floor of the Manchester Free Trade Hall a flue is constructed in the brickwork running round the walls. This channel is supplied with fresh air by three large openings from the outside. In the thickness of the wall eighteen vertical flues are carried up, which deliver fresh air into the hall between the ornamental pillars in the gallery and about 18ft. from the floor. From this elevation a sufficient quantity of fresh air can be introduced, and no unpleasant draughts are felt. Single rooms in lunatic asylums, hospitals for the insane, and prisons are generally without any efficient means of ventilation. The inlets to these would be greatly improved if fresh air, with means of warming it in cold weather, were admitted from Tobin tubes or gratings in the walls opposite the doors to these small dwellings. The inlets should be beyond the reach of the inmates, so that they could not be designedly stopped up. When a large quantity of pure air is required to be quickly and continuously supplied for several hours in crowded assembly rooms, exchanges, &c., a fan or fans, propelled by steam, are frequently necessary—a method called ventilation by propulsion—and the fresh air driven in can easily be regulated. If the air enters at a great velocity through small apertures, it will escape at the outlets without properly mixing if the latter be improperly arranged. In the Royal Courts of Justice this disadvantage is prevented by causing the air fanned into the courts to gradually enter them through numerous small apertures in their floors, the velocity of the entry being checked by the air passing through gauze sheets of large area at the openings of the flues up which it passes to the basement of the courts. There has been very much controversy respecting outlets for the escape and extraction of vitiated air within buildings. In small rooms Dr. Arnott's outlet has been much appreciated. It is a valved opening at the top of the apartment leading into the chimney; but it is mainly objectionable because smoke and wind are occasionally driven from it into the room, and also because it is seldom led into a separate extraction flue surmounted by an efficient exhaust ventilator. For large rooms containing a great number of occupants the Arnott outlet is of very little value. Upon inspecting the Municipal Offices at Liverpool, I was told that in some of them soot is blown from the outlets for foul air led into the chimney. The best means for exhausting the contaminated air from a large assembly room and other compartments occupied by numerous individuals where there is no room above, is to provide two, three, or more outlets according to the size of the room in the form of perforated rosettes at the top of the ceiling, or just under the roof, and at about equal distances apart. Each of these openings should be led into a tube, and extend outside the roof about two yards, and be mounted with a cowl or exhaust ventilator, which would act as a good extractor of foul air from, and prevent down-draughts within, the tube. A sun-burner is a powerful abstractor of vitiated air when fixed immediately under a copper cone. By far the greater number of sun-burners which I have noticed fail to answer their purpose efficiently in consequence of being fixed at the end of tubes extending several feet, and in some cases three or four yards, from the ceiling, as in this case much vitiated air remains above them. If a large room has several very small openings in addition to large outlets in the ceiling, as the Birmingham and Cambridge Town-halls, it is advisable to fix exhaust ventilators upon half-a-dozen or eight tubes from the roof, to take away the vitiated air from the roof space. The sides and top of the roof, except the tube apertures, should be sealed. By these means the foul air would soon be extracted from the rooms

below, and downdraughts prevented. Again, in a theater which has only one important foul-air outlet in the ceiling (like that at Covent Garden), above an immense chandelier, it would be much better to stop up the numerous small apertures in the roof intended for the escape of vitiated air from the theater, and fix cowls or exhaust ventilators on about half-a-dozen tubes, as just mentioned, rather than have one or two vertical tubes over the large outlet led above the roof similarly mounted, as the former outlet pipes would have far more exhaust effect, and be more likely to prevent down draught to the auditorium. Another method of withdrawing contaminated air from large compartments is by upcast open shafts of large dimensions, to which outlets from the ceiling or at the top of the sides communicate by horizontal flues. This arrangement has not sufficient power to abstract the vitiated air from all the artificial outlets connected with these channels, but only from those nearest to the shaft, although it may be intensely heated by steam pipes or coke fires for additional exhaust effect. It is essential, therefore, when practicable, to have as few openings as possible as outlets, and these should communicate with as many perpendicular shafts as can be reasonably constructed, a plan which has been more extensively adopted during the last four or five years. In the new examination rooms of the University of Oxford, two upcast shafts of large dimensions are made to extract the contaminated air from the large writing rooms, both of which have two rows of perforated rosette outlets, which communicate with the shaft connected therewith by a large longitudinal lath and plaster duct running between the outlets, from which the foul air enters by cross channels. In addition to this disadvantage, the shafts appeared to be open at the top, whereby they are rendered sluggish in their exhaust effect, and cannot prevent down draughts. To make such large shafts as efficient as they can be, they should not only be intensely heated, but extend like a tower above the buildings to be ventilated, in order to increase the velocity of the up draught, as is the case with the large ventilating shaft of the Lancashire County Prison at Manchester and the London Hospital. These shafts should also be mounted by a first-class exhaust ventilator. The system of attempting to withdraw vitiated air from many openings at the top of a room and led into an upcast shaft was adopted at the Manchester Athenæum, but had to be given up in favor of vertical shafts over the room outlets as far as practicable. In the Royal Courts of Justice, while the means of warming and cooling the fresh air and supplying such to the Courts appear to be very good, the arrangements for abstracting the foul air are very indifferent, for several reasons. In the first place, the eight vertical exhaust shafts were, when I recently inspected them, about 2ft. below the level of the roof, and therefore, though heated by coils of steam pipes at a high pressure, they lose much of their extractive force. I understand that application has been made to heighten them about 2ft. or 3ft. Another cause for the ill-ventilation of these new Law Courts is that the low shafts referred to were until recently open at the top, so that if draughts did not descend in the Courts from them, they were very sluggish in their abstraction of vitiated atmosphere. I am informed that these defects have been remedied by covering the tops of the shafts, and causing vitiated air to escape from side openings. This method is not anything like as efficacious as superior exhaust ventilators would be, and the sooner these are used the better. The defective ventilation of these Courts has also, in some measure, been occasioned by numerous cracks in the flat wooden roofs, whereby cold air has descended below and produced great draughts. Sudden gusts of wind frequently blowing in the doors of the public galleries from the corridors communicating with them has been another cause of the ill-ventilation of the Courts. To diminish this evil I suggested that double doors should be provided in these passages. I think the best outlets for large rooms with others above are perforated rosettes in the ceilings, which openings should be connected with as many vertical shafts as practicable, and surmounted by exhaust ventilators. Another class of suitable outlets to this class of rooms are square grates at the sides, leading into perpendicular tubes through upper apartments

to the outside of the roof. Outlets in the middle of compartments which communicate with louvre openings, either in turrets or lanterns, or at the sides of the roof, have no extractive power, and wind and often rain, is driven into the rooms below. In a great number of buildings I noticed that this kind of openings were the only artificial outlets for vitiated atmosphere into the open air, and was frequently informed that such apertures had been closed in bad weather, as the down draughts from them could not be tolerated. One of the officials of the Rev. C. H. Spurgeon's Tabernacle said that on cold mornings and evenings, when the windows and doors were closed in this mammoth chapel, and several hundred lamps were lit for illuminating and heating, some of the large air outlets communicating with louvre openings in the roof had to be closed to prevent unbearable down draught. Again, the Lecture Theater of the London Institution in Finsbury Circus has no outlet in the dome, and but very bad ones in the ceiling, which lead into the roof space, and are often closed on account of severe down draughts from the louvre openings at the top of the roof, intended for the escape of vitiated air, so that this room, which is frequently crowded, is often without ventilation, and especially as the numerous inlets under the seats for atmospheric air appear to be sealed with dust and dirt, occasioned by the smallness of their apertures and the inefficiency of the channel through which attempts are made to supply the room with external air. It is much to be regretted that the efficient ventilation of buildings has been greatly retarded by the erroneous decision of the sub-committee of the Sanitary Institute of Great Britain respecting exhaust ventilation cowls, but which conclusions have been fully refuted. Among the numerous ventilating arrangements which I have noticed for the ventilation of rooms and other inclosures in buildings there are several which have efficient artificial inlets and outlets; but a good system of both is not often found. It is very invidious to particularly mention whose method of ventilation is the best; but if pressed for an answer I should say that the most efficient and satisfactory means adopted to ventilate a building under a system by carefully adjusting the size and relative positions of inlets and outlets is that originated by Mr. Banner at the Council Chamber at the Guildhall in the City of London in 1878, and which was closely imitated at the Hall of the Royal Institute of British Architects in 1879, and subsequently at the Long Room of the London Custom House. It has just been adopted with great success at the Board Room of the Paddington Vestry and other buildings. The prize of the Society of Arts, in 1882, was awarded for the best sanitary arrangements in houses within the metropolitan area for the residence of Sir Daniel Cooper, Bart., 6 De Vere Gardens, where I found the Banner ventilators have been fixed and the system applied for the ventilation of the drains and other parts of the house, and which appliances are very satisfactory. I have also received very favorable notices from the officials of other buildings in which the system is used. The Banner exhaust ventilators appear also to be the most efficacious in use. Fresh air may be warmed for rooms and other inclosed spaces by hot-water pipes, by steam pipes, and by hot-air chambers. The advantages of heating air by the circulation of hot water in pipes are several: firstly, the temperature within the tubes is maintained six or eight times longer than in steam pipes after the boiler fires are extinguished. Again, the supply of heat required in any room can be regulated by valves in hot-water pipes; while in those containing steam we must have the whole or no heat from such, unless means are provided for a supply of cold air to be mixed with the warm air before being admitted within compartments, and which is very difficult to adjust. This being so, and as the heat from steam pipes is at the least about 212°, and often more, it is greater than can be generally borne. On the other hand, the heating surfaces of hot-water pipes are limited to about 160°, as the temperature of the water in the boilers to feed them seldom exceeds 200°, and the pipes are of larger dimensions than those for steam heating. Moreover, the cost of fuel is less for hot-water than for steam pipes, while the former are free from the danger of explosion. For buildings continually occupied it is nearly always better to heat the air by hot-water

pipes; but for warming cold air admitted into factories and workshops where steam is used, and for heating large foul-air extraction shafts, steam pipes are more useful than hot-water tubes. The Royal Courts of Justice are heated by hot-water pipes on the low-pressure system, while steam coils are used in the outlet shafts to create an intense heat therein for better abstraction. The supply of fresh air to rooms which have been heated by its contact with metallic surfaces exposed to coal fires is not as wholesome as exterior air warmed by hot-water pipe. The Galton stove, from which heated air is admitted into the room from the back of the fireplace is a very good hot-air apparatus for use in hospitals, barracks, or other large buildings, if it is fixed in the middle of the room. Mr. Constantine's convoluted stove, as used for warming the fresh air admitted to the Manchester Exchange and Free Trade Hall, is also very appreciable. There are also some very useful appliances invented whereby the fresh air can be heated at the inlets close to the walls by gas pipes, and whereby the products of combustion are not driven into the rooms. Opinions differ as to the position in which the heating pipes and stoves should be fixed. It is sometimes desirable to have the warming appliances in the basement, and admit the fresh air therein through tubes at or a little above the floor, or through grates at the skirting. In other cases the air may enter or pass over the heating apparatus in the middle or at the sides of rooms, or between the gratings outside and the inner walls. Heated pipes are frequently run round churches, chapels, and schools and other rooms, while more than half the places of worship are only furnished with the American "Cockle stove," whereby cold air descends upon the heads of congregations; and to lessen this evil the inlets are often stopped up, and numerous gas jets lit to warm the edifices. To these grievances must be added defective or no artificial fresh-air inlets, a large area of glass surface, and draughts from open doors. Coils of steam pipes appear to be the best means of heating fresh air entering large rooms at or a little above the skirting. A great deal has been said by patentees and others in favor of filtering fresh air before being admitted into rooms, in order to prevent dust or "blacks" being driven therein. For this object fine wire gauze, muslin, cotton wool, and thin porous flannel have been placed in the opening in the wall or tubes connected with them; but these materials have so soon got choked with dirt that they have prevented or greatly interfered with the admission of the air. Other means adopted with the view of preventing dirt being driven through inlet tubes, by fixing boxes filled with water in holes in the walls close to the grated openings, and whereby the fresh air is deflected by plates over the surface of the water before going up the tubes, are of very little practical use for stopping dirt entering, as only a very small quantity of the air comes in contact with the surface of the water and soon evaporates. The best plan to prevent floating particles of dirt entering rooms from the tube openings is to clean them well at proper intervals. The thin film of water propelled from several sets of extremely minute jets through which the air is drawn by fans for the ventilation of the Royal Courts of Justice, and several other contrivances of nearly the same kind adopted in the system of ventilating large buildings by propulsion, as St. George's Hall at Liverpool, &c., free the air from dust and "blacks," but the expense of this method of air filtering will prevent it from being extensively adopted. The means used for cooling the air for these buildings, by passing it through extremely cold water, are very appreciable; but other methods adopted for this purpose, by passing fresh air over receptacles filled with ice, are of little or no advantage, as only a minute part of the admitted air comes in contact with the ice, in the same manner as only a very small volume of sewer gas is purified by Mr. Baldwin Latham's charcoal filters, intended to purify the whole.

Mr. W. P. Buchan, of Glasgow, in opening the discussion on the paper, remarked that Mr. Porter had treated his subject exhaustively, and in many of the conclusions arrived at, he coincided with the lecturer. He agreed with the recommendation to provide exhaust shafts to outlets; for, unless the exhaust was a powerful one, the air in an occupied room

would still remain vitiated, notwithstanding the provision of ventilators. The outlet should never be placed low down in a room, as all the air in the space between it and the ceiling remained unchanged, and was the most vitiated in the room. Often, by raising the level of an outlet he had removed a persistent bad smell from a room. He did not see why Mr. Porter should have gone out of his way to speak exclusively of the Banner system. He understood that at the City of London Guildhall the Banner system had been condemned and replaced by another, and if that were the fact, he did not know why Mr. Porter should have been so anxious to commend the system and have quoted this instance of its use. He should be glad to hear particulars as to the test applied, and the system tried before the Society of Arts medal was given to this system. Could the lecturer say whether Mr. Banner carried out the system in the way described in his book on "Wholesome Houses," and as advertised? For, if so, it was not clear how the medal of the Society of Arts could have been given deservedly. He should much like to see the system in action. Heating in connection with ventilation deserved more attention than was usually given to it. It was useless to talk of providing more efficient ventilation for the dwellings of the poorer classes; what they wanted was to conserve heat as far as possible on account of their inability to buy fuel; and if any system could be devised for heating groups of this class of dwellings from one central fire it would be a great benefit. He concurred with Mr. Porter that hot-water pipes were more healthful than highly-heated steam pipes, and these again were much superior to any form of iron stove. Conical ventilators were very useful, and were the subjects of many recent patents, although as a matter of fact, they were first recommended by Mr. Philbrick, architect, in a letter which appeared in the *Building News*, in 1870, 1871, or 1872, long before the earliest of these patents were taken out. A valve-box should be provided to every outlet, to prevent down draught.

Mr. Welman thought a description of the best mode of ventilating the ordinary dwelling would have been more useful to most architects than the full details and criticisms Mr. Porter had given as to the new Law Courts. He was not a believer in the exhaust ventilators, as they occasionally acted as inlets, when they were of dubious advantage. He also had little faith in Tobin's ventilating tubes, as they were wrong in principle; they directed the supply of fresh air to the ceiling, where it mixed with the foulest air in the room, always accumulated at that level, and then descended. It was a mistake to aim at making the walls of houses impervious to the passage of air by lining them with an air-proof composition. The aim of sanitarians should be to render them more porous than at present, so that the side of the house on which the wind blew should be an inlet, and the opposite side, on which there would be a partial vacuum, should be an outlet. He had not yet worked the idea out; but if some means could be adopted for warming the incoming air in such a hollow wall, it would be a step in the right direction.

Mr. E. C. Allam, of Romford, said, that while theoretically everybody approved of the fullest ventilation, practically the first endeavor was to close any ventilator within reach so as to prevent draught. He differed entirely from the last speaker in his advocacy for porous walls, for he regarded them as most objectionable. Outlets in public buildings should be placed high up, but the inlet should not be too low down, or the fresh air would remain close to the floor level. The vertical tubes answered very well, and seemed the best system yet adopted. The treatment of small rooms was a more difficult problem, and he should have liked more information as to the subject from Mr. Porter. The outlet of a room was necessarily very low down—the fireplace, and any attempt to place it higher only gave rise to complication. For admitting fresh air, a good plan he had found was to provide an opening just over the architrave of door, deflecting the current upwards by hanging a picture in front. He preferred, for warming purposes, hot air or steam apparatus to hot water, as with either of the former you could warm and ventilate at the same time.

Mr. P. J. Davies found the best pla

of ventilating a dwelling room was to introduce the fresh air at back of fireplace; he had adopted this plan since 1868, and had never known it to fail. No system of warming answered so well as the one on the hot-water principle, which provided perfect ventilation; an excellent method was to carry the hot-air flue behind the skirting, perforating it with very small holes. He could not understand why Banner's system of ventilation should have been set up by the lecturer as the best, when there were Buchan's, Boyle's, and many other systems equally good.

Mr. Lindsay had found, for equable warming and ventilation of the rooms of the poor, an excellent plan was to carry the partition dividing the apartments on a floor no nearer to the ceiling than was necessary.

Mr. G. H. Guillaume supported the suggestion of Mr. Welman as to the value of porous walls, and referred to Dr. Pettenkofer's experiments as to the transpiration of air through walls. It would be well if something could be done to obviate the unsightliness of cowls—he would not mention the names of any—and he thought architects might do something in this respect. He suggested that the discussion might be resumed at an early meeting of the society.

Mr. Ellis Marsland asked Mr. Porter if he could explain why inlets should be larger than outlets. When he commenced practice he had to ventilate a room, and after reading up books he carried out a scheme. He invited the committee to inspect the work, but before they came he thought it well to privately test it. On the day when he took his anemometer down to the room it was very windy, and the plan worked well; but when the committee came it was a damp dull day, and the anemometer would not work. (Laughter). He found that the system of bringing in air 7 ft. above the floor answered well if the air was heated as it was brought in; but the outlet should, he believed, be larger than the inlet.

The chairman, in closing the discussion, remarked that the problem of ventilating living rooms was very difficult, as the conditions when the fire was lighted and when it was unlighted were just reversed. His experience had shown that when the acoustics of a public room were bad, the ventilation would be found to be defective, and when he had remedied the latter he had always found that people could hear better in the room. An excellent plan often adopted now-a-days was to put a broad beam at the bottom of a window so that it could be raised to admit air at the opening. If the window went well up to the ceiling level, a broad beam should be placed at the top, having in it a perforated zinc band with the rough edges outside, like the surface of a nutmeg grater; the air would then easily pass out, but would not so readily be admitted. Outlet shafts should always be provided with some means of propulsion, either a gas-jet or a water-fan.

Mr. Porter, in replying upon the discussion, said the chief points had been with regard to house ventilation and the inlets and outlets of public buildings. The inlets should always be larger in area than the outlets, in order that the in-currents might have greater propelling power. In most public buildings the inlets were proportionately much too small. Louvre ventilators were no good at all; there were always down draughts from them unless a gas-flame were maintained at the foot of the shaft. He had not treated of house ventilation in his paper, which was confined to public buildings. The Arnot ventilator for an outlet, and an ornamental hopper over door, acting as a "Sherringham" valve, would act efficiently in a dwelling room. For the drawing-room and dining-room a good plan was to provide an opening in chimney breast with separate flue carried up in front of chimney. He could not go into the ethics of advertising ventilating systems; he had simply mentioned the "Banner" system as one with which he was acquainted. He believed that at the City Guildhall another inventor was allowed to make alterations after the "Banner" system was fitted up; but, instead of improving matters, an opposite result had ensued.—A vote of thanks to the lecturer concluded the proceedings.

THE APPLICATION OF ELECTRO-MAGNETS TO THE WORKING OF RAILWAY SIGNALS AND POINTS.

By ILLIUS A. TIMMIS.

From "Iron."

The object of the present paper is to describe a new, powerful, and economical electro-magnet, capable of exerting its pull in a manner suitable for convenient application to the mechanical working of railway signals and points. Its main features are its great power, which is exerted through a long range, and is under perfect control; and its economy, both in exerting the initial pull which effects the mechanical movement required, and also in retaining the moving parts after the desired movement has been effected. The rapid growth of railway systems and their consequent complexity have rendered urgent the application of electricity for the working of railway signals and points, and for checking the work done by signalmen and pointsmen. Though much has been accomplished in this direction, both in this country and elsewhere, there has hitherto been always one initial want, namely, an electro magnet, which, with a low average current and a small electromotive force, shall give a powerful, long, and well-balanced attractive pull. A range of pull of only half an inch, which has hitherto been the practical limit with electro-magnets, is evidently not long enough to be of use for heavy mechanical work, such as pulling signal-arms over or throwing machinery in and out of gear; and although the range can be increased by multiplying gear up to two or three inches, yet the increased current then required for making the attraction powerful enough at the initial half inch distance is so enormous that two difficulties present themselves which are almost insuperable—firstly, the expense; and, secondly, the destructive violence of the final impact, in consequence of the attraction increasing inversely as the square of the distance of the armature from the magnet, and for part of the stroke inversely as the cube. In the ordinary form of the electro-magnet, a core or bar of soft iron, wrapped around with a coil of wire or tape, and bent to the shape of a horse shoe, attracts to its poles a plain armature; and, by some natural law, as yet unsolved, the working pull of such a magnet practically ceases, as already stated, when a distance of only half an inch between the magnet and the armature is exceeded. In the electro-magnet of tubular form, thence called a solenoid, a hollow core or tubular bobbin of brass is wrapped around with wire or tape; and a central rod of soft iron, sliding lengthways inside it, forms the armature, which is drawn or sucked longitudinally into the core by the attraction of the bobbin coil, until it reaches a central position in relation to the magnetic field; in this position the attraction is symmetrical in all directions, and is therefore neutralized. Although a considerable length of attractive pull can be got with such a magnet, the current of electricity required to give any strong initial pull is so excessive that its cost is prohibitory; moreover, it is clear that the holding power falls to zero when the armature reaches its central position longitudinally. In order to obtain adhesive power, or what is called a retaining pull, the internal sliding rod or armature is made shorter than the length of the hollow bobbin, and is capped at one end with a disc of soft iron, which, coming in contact with that end of the bobbin, thereby prevents the armature rod from reaching its central or neutral position within the bobbin, and leaves a corresponding reserve of holding power against the end of the bobbin.

Currie Long-pull Magnet.—In the long-pull electro-magnet, the invention of Mr. Stanley Currie, the principle adopted is a combination of the horse-shoe magnet and solenoid, with additions; but the construction is so materially modified as to give far greater power and efficiency, and the magnetic attraction is more evenly distributed over a longer range, while the initial pull is stronger, and acts

* A paper read before the Institution of Mechanical Engineers.

at a greater distance than in any other electro-magnet of the same weight and with the same current. The range already attained in practice is 3½ inches, and can be increased. The bobbin is made with a tubular core of soft iron, and the coil of wire wound around it, colored red, is surrounded by an outer casing of soft iron, of the same weight as the core, with a soft iron base plate at the bottom of the bobbin, connecting the core with the outer casing; a brass plate covers the bobbin at the top. The copper wire used in the coil is No. 18 Birmingham wire gauge, or 0.048 inch thick. The armature consists of three portions, each one playing its part in the work to be done. The central stalk of soft iron is rather shorter than its own range of motion, and is encased in a brass tube, which is prolonged below it so as to form a guide, fitting within the bobbin core. The soft iron cap or disc, fastened on the top of the central stalk, is slightly larger in diameter than the outer casing of the bobbin. It is made by preference of two or more thicknesses of flat plate, to assist in demagnetization, but it must be thick enough to prevent saturation with any working current. Around the edge of the disc runs a cylindrical rim or flange, projecting downwards; it is so shaped as to suit the attraction required, and it comes within the range of attraction of the outer casing of the bobbin when the lower end of the central stalk has entered within the core. When the rim in turn has done its duty, the disc comes within range of attraction of both outer casing and inner core.

Uniformity of Pull.—So long as the central stalk or armature rod is altogether out of the bobbin core, the attraction upon it continues to be inversely as the square of its distance from the bobbin; but as soon as the end of the iron rod enters within the orifice of the core, the force of attraction becomes lost upon so much of its length as is inside the core. The same diminution in attractive force holds good in regard to the flanged rim of the armature disc, as soon as its lower edge passes below the upper edge of the bobbin. The force of attraction varies also directly as the mass of the body attracted. Advantage is therefore taken of these two principles in combination to regulate or adjust the effective attraction in such a way as to obtain some sort of approximation towards uniformity of pull throughout the 3½ inches' range of stroke of the armature. This is accomplished by tapering the lower end of the armature stalk, and also of the flanged rim, according to the desired adjustment of the attractive force, in addition to which the thickness of the armature disc, and the distance that its flanged rim projects downwards, as well as the thickness of the rim, can also be varied; and in some cases, moreover, the bottom edge of the flange is made of a serrated or wavy form, so as to prevent the pull from suddenly increasing as the disc nears the bobbin. The result is, that when the strength of the pull on the armature stalk and flanged rim is decreasing, owing to their having both of them reached and passed the position of maximum attraction, the pull on the disc is increasing as it nears the magnet head. In this way, and in combination with a counterweight acting at a suitably varying leverage, an approximately equal pull is obtained through a considerable range, and violence of contact in the closing of the disc upon the magnet is avoided. By suitably adjusting the several proportions of the various parts, the pull can be so greatly varied both in force and range that it can be adapted to meet almost any requirement.

Double Length of Pull.—A double length of pull is readily obtained by the simple tandem combination. Here a pair of single magnets on the foregoing principle, arranged at a fixed distance apart, have their armature guide-rod in common; the lower armature disc is made fast upon the rod, while the upper disc bears against a shoulder upon it. The range of the lower disc being nearly double that of the upper, the first half of the pull is given by the upper; and by the time the upper disc has closed upon its own bobbin, it has brought the lower disc within the attractive range of the lower magnet, by which the second half of the pull is then given, the armature rod now sliding free through the upper disc. This arrangement is suitable for working signal arms that are required to stand at the three positions of danger, caution, and line clear.

Railway Signals.—In the application

of the electro-magnets to railway signals, the ordinary signal posts and arms are utilized; but it is advisable that the bearings and working parts should be made as true as practicable, because, though friction is not so material when the work is done by manual labor, it is of the utmost importance that it should be reduced to a minimum where electricity is the motive power. As there are no complicated parts, and the movements are all simple and direct, there is no difficulty on this point. In the application of a single magnet to an ordinary signal-arm intended to stand in only the two positions of danger and line clear, the magnet is fixed upright on a bracket at the back of the post, and a chain from its armature pulls upon a quadrant arc, centered on a horizontal spindle above it. The quadrant carries a lever and counterweight, acting in opposition to the pull of the magnet; and also an arm which is connected by a rod, colored red, with a bell crank centered at the side of the post. Another rod, colored blue, connects the bell crank with the semaphore and spectacle. When the magnet is out of action the semaphore is held up at danger by the weight of the spectacle in conjunction with the counterweight on the quadrant lever, which then comes against a stop. In this position all the parts are locked, in consequence of the quadrant arm being then on its dead center; that is, the quadrant arm and bell crank are so arranged that the direction of the rod (colored red) connecting them passes them through the center on which the quadrant turns. For yet greater security of locking it is preferable, indeed, to let the connecting rod be even a trifle beyond the dead center, so that any pull upon the bell crank, from wind pressure or accumulation of snow on the semaphore, shall hold the counterweight lever still more firmly against its top. The locking is thus done mechanically, and is independent of the magnet. On bringing the magnet into action by a current from the signal box, the pull of the armature rotates the quadrant arc, raising the counterweight and spectacle and lowering the semaphore; in this position the semaphore is retained so long as the electric current is continued. On the cessation of the current the semaphore is automatically raised again to danger, and locked there mechanically without the use of extraneous catches, which is a most important feature. In the application of the double magnet or tandem arrangement to a balanced semaphore centered at mid-length, the pair of magnets are fixed on one side of the post, and the quadrant arc on which they exert their pull is fixed on the spectacle spindle. A rod (colored red) connects the spectacle with a crank on the semaphore, and the weight of the spectacle brings the semaphore to the horizontal position of danger, in which it is locked mechanically as before by the connecting rod being then on its dead center. An electric current sent to the upper magnet pulls the semaphore to an angle of 45° for caution, and a second current sent to the lower magnet pulls it vertical for line clear. In either position it is held by the spectacle acting as a counterweight against the retaining pull of the magnet.

Electric Current.—Under ordinary circumstances a current of 3 amperes is amply sufficient to pull the armature down upon the magnet, and so actuate the semaphore; but to guard against wind pressure, dust, rust, and wear of rubbing parts, a current of 5 amperes is provided. In addition, a reserve current of as much as 15 amperes is kept in store at the signal box, and is at the immediate command of the signalman in any emergency. With 5 amperes an initial pull is given of 8 lbs., at a distance or range of 3½ inches, which is more than double what is required to work any signal arm properly fitted, and the home pull is 321 lbs. If the same strength of current were necessary to hold signals down as to pull them down, the use of a continuous current would be prohibited by its cost. But a current of only 0.1 ampere is more than sufficient to hold a signal down; and it is found that a continuous current of 5 amperes for moving the signal, and of 0.1 ampere for retaining it, yields an economical result. Accordingly, when the armature has finished its stroke and moved the signal, it is made to switch in automatically a resistance coil which reduces the maximum or moving current of 5 amperes to the minimum or retaining current of 0.1 ampere. The resistance coil can take the form of an incandescent lamp, which then serves as an indicator or tell-tale in the signa

box. In excess of the greatest possible requirements, it may be assumed that the whole time during which each signal is needed to be held down to show line clear is twelve hours out of every twenty-four; and that the number of times each signal has to be lowered in the twenty-four hours is 150, and that the time occupied in each lowering is two seconds. Assuming also 10 amperes instead of 5 as the moving current, and 0.2 ampere instead of 0.1 as the retaining current, then 150 lowerings $\times$ 2 seconds $\times$ 10 amperes $=3{,}000$ ampere seconds, or 0.8 ampere hour; and a retaining current of 0.2 ampere for 12 hours$=2.4$ ampere hours; the total is, therefore, 3.2 ampere hours of current C per signal per 24 hours. The resistance R being taken at 5 ohms, the electromotive force E is equal to $C \times R = 3.2 \times 5 = 16$; and assuming 746 watts $=1$ horse-power, the horse-power is equal to $C \times E \div 746 = 3.2 \times 16 \div 746 = 0.07$ horse-power. The cost of providing a current of such power by means of a secondary battery, according to well-recognized electrical data, would be only 0.4 penny, while the current actually used being only about one-third of that provided, and the real time less than one-half, the true cost would be less than one farthing per signal arm per day. The foregoing remarks respecting the electric current employed to actuate the magnets apply to the use of secondary batteries; and the results obtained from these accumulators of electrical power are very satisfactory and economical. In certain cases, however, it may be preferred to use a primary battery, such as the Lalande oxide of copper battery, the working of which is very easy and reliable. The constant and economical current it gives can be used not only for actuating the magnets, but also for lighting the lamps on the signal posts and at the points.

With a primary battery the reduction of the current from a moving to a retaining current cannot be effected in the same way as with a secondary battery by switching a resistance in; but the same levers and switches are still available for switching out the large battery which gives the stronger moving current for lowering the signals, and switching in a smaller battery, of smaller cells and with greater internal resistance, which gives the weaker retaining current for holding the signals down.

Railway Points.—The magnets and gearing for working a pair of railway points are practically of the same construction as for working signals, but are of larger size, and are wound with copper tape instead of wire, in order that they may take a maximum current with a minimum of resistance. With a current of 23 amperes and an electromotive force of 40 volts, the force of the pull commences with an initial pull of 33 lbs. at $3\frac{1}{2}$ inches distance, and increases to 54 lbs. at 3 inches, and to a home pull of 1,064 lbs. The points are pulled over and held in either position by a sliding rod, which is worked by a lever and is locked by a locking bolt. The slot in the rod is made $\frac{1}{2}$ inch longer than the width of the lever working in it, so that the first $\frac{1}{2}$ inch of travel of the lever withdraws the locking bolt by means of an incline on the extremity of the lever, before the lever acts upon the sliding rod. Where a pair of points are covered by a signal, the locking bolt, in conjunction with the armature of the magnet which pulls the points over into the position corresponding with the signal when down for line clear, completes the circuit, which enables the signal man to lower the signal. The signal is checked by its automatic repeater in the signal box. The other point-magnet completes with the locking bolt the circuit which works a repeater in the signal box.

Advantages.—The advantages of working signals and points by this system are that their distance from the signal box is immaterial, inasmuch as the electric working gets rid of all the mechanical difficulties which arise from excessive expansion and contraction of lever wires and from the severe pull required to work them through long distances.

Signals.—It is only during the continuance of the electric current that the signal can be held at caution or line clear; its normal position is at danger, to which it returns automatically on the cessation of the current, or if anything goes wrong; and in this position it is locked by simple mechanical means, and not by any electric agency whatever. Each signal is locked by its own magnet, and has a repeater or tell-tale in the signal box in the shape of a small

arm. If it is desired, the repeater is arranged to show the exact travel of the magnet armature working the signal; that is, if the armature stops half way, the repeater arm would stop in the same position; in fact, any arrangement that may be desired can easily be effected. At the instant of contact between the armature and the magnet, there is a momentary cessation of the main current; and the tell-tale arm indicates this by falling back a little from its extreme position. This affords another infallible check as to the correct working of the magnet. The reduction of the current from a moving to a retaining pull takes place automatically at the moment of the armature reaching the magnet.

Points.—The advantages in working points by these electro-magnets are as great as in the case of signals. Either can be worked at any distance from the signal cabin. There is no need for a cabin to be put up, or a stand of levers, to work any special set of points and signals; any number of points and signals can be worked from a small cabin, and can be locked and interlocked with absolute certainty. They can be so arranged that, if any wrong lever is moved by the signalman in arranging any combination, an alarm bell is rung in the cabin, and the signals already lowered go back to danger, the mechanical and electrical parts being so arranged as to provide a perfect check, without the intervention of manual labor or the will of the signalman.

Application to Railway Junctions.—In the application of this electric system to working the signals and points of an ordinary junction where there is a double line of way both on the main line and on the branch, the signal box is divided, as regards the electrical connections, levers, and switches, into thirteen divisions. For station to station signaling the transmitter and receiver instruments are used in somewhat the same way as in ordinary railway working. The continuous current used makes the working very simple, and easy and reliable. The signals and points are actuated by a main current, and this is checked and controlled by a subsidiary current, which works the transmitter and receiver instruments. A train traveling along any section of line protects itself by passing over contact levers or treadles, both on entering and on leaving the section; the depression of the treadles by the deflection of the rail breaks the current which holds the signal-arms down, and sets them free to fly up to danger automatically.

Mr. Timmis desired to add that he had had the signal working in his office in Westminster for a considerable number of months without the slightest hitch of any kind; and at the Gloucester wagon works there were five signals—a distant, a home, and three station signals—all actuated by an electric current from a secondary battery. They had been worked during very severe thunderstorms and gales, and the result was to demonstrate that no electrical disturbance could affect the working of the signals either by a primary or by a secondary battery. They had also had a set of points sent to Gloucester by Mr. Owen, of the Great Western Railway, to fit up and work with their magnets, and no difficulty had been experienced in pulling over the points at any time under any conditions hitherto met with. It might be objected that they would not pull over in heavy snowstorms, but points had now to be watched at such times, and there was no more difficulty (indeed not so much) in working the points electrically than there was by the present manual methods. Last spring the manager of the Swansea Dock and Harbor Trust had asked him to fit up a signal lamp on the ground. There was an objection to signal ground lamps because they were sometimes kicked and injured by loafing navvies, and he therefore suggested putting up a small signal, five-eighths of the ordinary size, at such a height that when the arm was down the point should be rather over 7 feet from the ground. That signal had been fitted up about four months, and had been regularly working without any hitch. The point he desired to emphasize was, that if anything did go wrong the signals went to danger. Mr. Currie, by means of an exhaustive series of experiments, had shown that by using an intermittent instead of a constant current, four times the strength of current could be sent without injuring and heating the wires. When in a signal cabin, a signal, lowered to enable a train to leave a given station, was set to "dan-

ger," the train passed what was called a contact breaker. Treadles were not used, because it was impossible to get a perfectly reliable one for an electric current, but there was no difficulty in making a contact breaker which would break the contact with absolute certainty. Mr. Timmis then exhibited one of his signals with the electric apparatus, and illustrated their mode of working.

Mr. Crompton congratulated the author on the successful manner in which he had worked out a very intricate subject. They were, he believed, on the eve of a great change in regard to the mechanical transmission of power. Much of what was now done by ropes, chains, rods, and by hydraulic power, would be accomplished by electrical transmission. A great deal had already been done in that direction, for electric lighting was nothing more than the electrical transmission of power. The results achieved by Mr. Timmis were, however, regarded by many in much the same light as the early performances of the gas engine, which were supposed to be only fit for working churning machinery at an agricultural show. Electric magnets were looked upon as mere toys, capable only of working small signals; but when it was seen that a 4-inch continuous pull of 400 lbs. could be obtained at a distance of several miles with an apparatus weighing less than 1,200 lbs., some idea could be formed of the value of the transmission of power by electricity. But the weak point in the author's project was the difficulty of providing an electric current along a line of railway. When that was done the thing would be easy enough, but how was Mr. Timmis going to charge his secondary batteries? He did not think there was much to hope from primary batteries for those electric currents. They would be very expensive to work, and even the battery mentioned by Mr. Timmis, which was a cheap one, would be much dearer than the existing method. He thought that the author could not have sufficiently considered the cost of providing electricity at the stations up and down the line. In regard to the switches, he should be glad to know how Mr. Timmis got over the difficulty, not of breaking the contact, but of making the contact again perpetually, because it had been found that sparkless switches, even as low as 5 amperes, when constantly used, suffered a great deal, and required constant attention. As to the long pull magnet, there were many modes of obtaining the same result, but the use of the counterweights, as mentioned by Mr. Timmis, involved a great waste of power. It was no doubt a useful locking arrangement, but it was not as good as it might be.

Mr. S. P. Walker desired to endorse the remarks of Mr. Crompton with regard to the great skill shown by Mr. Timmis in working out his method. Engineers connected with the working of railways would of course welcome anything that would increase the safety of life and goods, and he hoped, therefore, that there was a great future for some system of electrical signaling. He had often wondered that electricity had not been before applied to the working of railway signals. There were, however, one or two points on which he thought that Mr. Timmis had not done electricity full justice. In the form of the magnet that he had brought forward he had attacked the question at the wrong point. Nothing could be better than his mechanical arrangement for the locking of the signal, should anything happen to the electrical part of the apparatus; but he had placed some unnecessary difficulties in the way. He had stated that up to the present time it was impossibe to get more than a ½-inch pull; but he would ask him to go to the nearest dynamo machine with a piece of iron in his hand, and see at what distance it would pull it away; it would certainly be considerably more than half an inch. Mr. Timmis had also stated that the pull might be increased by leverage, but that it became too expensive, because the current was excessive and the impact too great. That, he thought was not so. From the author's figures it appeared that 8 lbs. pull was obtained at 3 inches distance. By a well-known law the pull between a magnet and its armature varied inversely as the square of the distance, therefore at half an inch distance he would get 400 lbs. On the ordinary principle of the lever, taking a lever 7 to 1, and using it at half an inch, he would only require about 56 lbs. The meaning of that was that he could use a magnet, taking the same current, one-eighth of the size of that

which he had exhibited, if he would use some simple form of multiplying gear, and that would reduce the maintenance very considerably. They might do anything they liked if only the magnet was big enough, and constructed so that the lines of force would reach the armature. Mr. Timmis was mistaken with regard to the action of the solenoid magnet. That action was simply that each coil of wire around the bobbin exerted its own pull upon the core, and as the core gradually drew into the bobbin, more and more of the wire came into action. He was mistaken in supposing that all that he had done could not be effected with the solenoid magnet. No doubt it was more expensive, because the whole pull was not got at once, but there was a gradual pull. They could not have both, and something must be sacrificed. The author might have got all that he wanted without going out of his way to invent a new magnet. As to the impact, he thought it was a rather heavy one. He saw no necessity for an impact following on the magnet at all. It would be easy to place some buffer to receive it if necessary. The construction of the magnet was very old. He remembered, eight or nine years ago, a very similar form being brought before the Society of Telegraph Engineers by Mr. Falconer, of Manchester, who was under the impression that electricians were wasting all their power. The matter was taken up by the then president of the society, and it was fouud that Mr. Falconer was correct, so far as pull went, but there was one peculiar feature about his method—though it would attract in great force it lost its power at very small distances from the pulls. What was good in the author's method, so far as the long pull was concerned, was due to the old solenoid magnet, and what was good with regard to holding power was due to Mr. Falconer's system. He disputed the economy of Mr. Timmis's method. He had given a working current of 5 amperes, and the resistance of the circuit was given as five ohms. By the well-known law that the work done was equal to the square of the current multiplied by the resistance of the circuit, he made out the result as 125 watts, or, roughly, about one-sixth horse-power. Taking the pull at 90 lbs. per second, and two seconds for a pull, as stated by the author, that would give a pull of 180 foot pounds. For that expenditure of energy they only had a force back in the magnet of 2½ foot pounds, and he hardly thought that that confirmed the author's statement with regard to economy. As to railway points, they had 1¼ horse-power to pull a railway point over. The whole thing might, he thought, be carried out at a much less expense than Mr. Timmis had foreshadowed by his arrangements. He was using too large currents, and he would suggest that instead of using 5 or 10 amperes, he might bring it down to one-tenth or one-twentieth of an ampere, particularly if he was going to work with secondary batteries. The great difficulty in connection with primary batteries was that when they were furnishing large currents electrolysis went on so fast that secondary cells were formed which required frequent attention. So long as they were furnishing small currents they had no difficulty in getting a battery that would last a long time. In providing an increased number of batteries, there was, of course, an increased consumption of material because of the increased number of cells, but the consumption was so much lessened in each cell, that there was an enormous gain. Mr. Timmis had perhaps fallen into the mistake of thinking that they were not able to get more than half an inch pull by the fact that they rarely saw an electro magnet that was constructed to pull half an inch. No one would construct such a magnet, except for some special reason, because he would know the great advantage of having the armature close to the magnet. He quite endorsed what Mr. Crompton said with regard to the trouble experienced with switches and contacts. There was nothing more troublesome than keeping surfaces clean through which electric currents had to pass, and the larger the current the greater the difficulty. Mr. Timmis was also mistaken in supposing that with a primary battery it was impossible to insert resistances as with a secondary battery. He could throw in his resistances or cut off the number of cells, adopting whatever plan was most convenient. Whatever he could do with a secondary battery, he could do equally well with a primary battery. He also endorsed what Mr.

Crompton had said as to the difficulty of providing power.

Mr. Davey asked what was the number of cells in the primary battery required to work the apparatus exhibited. It appeared to be very large.

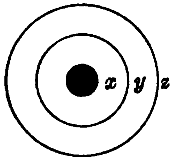
Mr. Timmis, in reply, said that the number of cells appeared to be very large to work one signal, but the cells required for that purpose would work equally well for a score of signals. Mr. Currie and himself had no doubt, not from theory, but from absolute practice, that they could work wherever they liked with secondary batteries, and also with primary batteries, if the conditions were favorable. At Swansea they had fitted up one signal, and before long there would be a great number there working from one, or, at most, from two, batteries which were being regularly charged every six weeks, without any difficulty at all, from dynamos which were used for lighting purposes. There were engines running at many big stations which could occasionally be utilized for an hour or two to run a current from a dynamo, or to recharge the batteries as they were running down. That did not appear to him to be a very extravagant arrangement for the transmission of power. Where there were not those conveniences it was proposed to use the Lalande oxide or copper battery. As to the use of a minimum or maximum current, he maintained that the current he was using was an intermittent one. He was certain that the Lalande oxide or copper battery would not want touching oftener than six weeks, and he believed not oftener than once in three months. The weight of a primary battery was not considerable. The cells were being made by M. Lalande so light that a whole battery for a fair-sized junction would not weigh more than 1 or 2 cwt. Mr. Walker was wrong in supposing that the same results could be obtained with a solenoid magnet as with a Currie magnet. It would be seen from his figures that the $\frac{1}{2}$-inch pull was 70 lbs., and not 400 lbs., as Mr. Walker had stated. The absolute weight of the counterweight was nothing like sufficient to regulate the pull in the way in which it was done by his method. Mr. Currie and himself were perfectly well aware of what Mr. Falconer had done with his magnets, and also with what Mr. Holroyd Smith and other inventors had done.

THE SOLAR TEMPERATURE QUESTION.

By F. GILMAN.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

PROF. WOOD, in the November number of this Magazine, calls for the proof of the statement that the intensity of heat emitted by a spherical incandescent body varies inversely as the square of the distance from its center. I have referred him to Captain Ericsson's experimental demonstration, but if he prefers a theoretical one it is very easily given.



Let x represent an incandescent spherical body, and suppose it to be maintained by any cause at the same temperature. After a time the flow of heat from the body will be uniform, and each point of the surrounding space will also acquire a constant temperature. This represents very nearly the state of affairs in the solar system, as regards the distribution of heat. For the temperature of the sun is nearly constant, and the average temperature of the earth's surface has not sensibly changed for a long period of time. This being premised, let y and z represent two spherical shells concentric with x, and suppose them to be very thin and perfectly diathermanous. Since the flow of heat from x is uniform, the amount that passes through the shell y, in a given time, must be equal to the

amount passing through z. For if more, or less heat, passes through y, than passes through z in the same time, the difference must be imparted to, or withdrawn from the space between the two shells; and any body placed in this space would be constantly growing hotter or colder. But this is contrary to the observed facts.

Let the amount of heat which passes through the shell y in a unit of time, as well as that which passes through z, be denoted by C. Let A represent the area of y, and B the area of z. Then the intensity of the heat passing through y will be given by $\frac{C}{A}$, and the intensity of the heat passing through z by $\frac{C}{B}$. But these quantities are to each other inversely as the squares of the distances of the respective shells from the center of the heated body x. Such is the law of the distribution of radiant heat as we advance from the surface of an incandescent spherical body outwards; but it is of course no longer applicable when we advance from the surface inwards, since we have here to do with conduction, and not radiation.

I do not see why the law of Dulong and Petit is entirely at variance with that of the inverse squares, as Prof. Wood affirms. They appear to me to be quite independent of each other. The law of Dulong and Petit gives the quantity of heat radiated in a unit of time by a body whose temperature is not more than 200° C. above that of the surrounding space. The law of inverse squares, however, as applied to a hot spherical body, does not enable us to determine how much heat the body will radiate when at a given temperature, but it shows that whatever heat is radiated will be so distributed in the surrounding space, that its intensity will be inversely as the square of the distance from the center of the body.

The medium to which I referred in my former article was the sun's atmosphere which I suppose is not diathermanous.

Photometry affords a means of verifying the law of inverse squares as applied to light.

Assuming the intensity of the sun's light at the surface of the moon to be the same as it is at the earth, if all the light incident upon the moon were reflected, the intensity of the moon's light at the earth would, according to the law of inverse squares, be about one-fifty-thousandth of the intensity of sun-light. According to the observations of Sir William Thomson the intensity of the light of full moon is one-seventy-thousandth of the intensity of the sun's light. But inasmuch as he gives this only as a very rough approximation, and since not all of the light incident on the moon is reflected, it must be acknowledged that the agreement between the theoretical and the observed intensity is as close as could be expected.

THE SCIENCE AND PRACTICE OF ENGINEERING.*

By Prof. KENNEDY, University College, London.

From "The Engineer."

Professor Kennedy said he was very glad to have an opportunity of putting before younger engineers some of the ideas which ten years' experience in combined academical and professional work had brought specially to the front in his own mind. He was rather assuming that those who heard him were engineers, or directly interested in engineering matters, and he would make no apology for being somewhat technical. The subject he had chosen was brought into prominence by his special work, and he hoped it was not too well worn to be worth a little consideration. The views held upon it were exceedingly divergent; so divergent, indeed, that it was hardly to be doubted that the true view must lie between the extremes. They must endeavor to find where. On the one hand, there was the man who prided himself on being practical, who would barely hear it said that there was any science of engineering, and who would condemn

* An address delivered on the inauguration of the Crewe Scientific Society.

beforehand any propositions or schemes in the devising of which he imagined that scientific method, or, worst of all, mathematical calculation, had been employed. On the other hand, the man of science, pure and simple, and especially the mathematician, was prone to find out that because so much engineering work could be done, or at any rate was done, without calculation—and in particular because so few engineers were really able to solve, or even to comprehend, mathematical problems, which to him were quite simple, engineers were mere empirics, that no science worthy of the name existed in engineering, and that engineers could only be considered scientific men by a stretch of courtesy. It was right to say that such extreme opinions were not held by first-rate men, either as engineers or men of science. But fourth-rate men were much more free of their opinions, much more dogmatic, more positive that they only had the truth—in engineering as in other matters—than those of the higher ranks. In what he had to say they would understand him to be using the much-abused word "scientific" in the very widest possible sense. Let "science," if they liked, include all the exact knowledge, and let "scientific" mean the method of arranging, comparing, reasoning on, and applying such knowledge. Moreover, it did not follow that if they did a thing that had been done by some scientific forerunner, they, too, were necessarily scientific. Whether it be scraping up a slide face, or drawing a stress diagram, the work might be scientific or unscientific, according to the spirit and method in which it was done. In both cases the methods of procedure had been invented for them. They had not anything original to do, anything novel in the way of scraping or of girder stress diagrams. If they did either just as and because they had been told to do it in a particular way, without knowing or caring anything as to the why or wherefore, their work was no more scientific in the one case than in the other. But in both cases equally the work might surely be thought scientific if it was carried out with a knowledge of the meaning of what was done. Of why, for instance, scraping in this case was to be preferred to planing, of why it was an improvement on the old grinding process, of why the scraper was not ground like a cutting tool, and so forth. Not, of course, that these should be leading ideas in one's mind at the time, when one's only idea should be to make the surface as true as possible, but that they should be only absent because they were laid up in a corner of one's brain, duly labeled, and capable of being brought down to explain themselves whenever called for. He mentioned as examples of engineering work more or less familiar to them the two very different things, scraping a surface and drawing a stress diagram. He chose them because the latter belonged to the class of work frequently called scientific, when the former was called mechanical. To this he objected. On the whole, the drawing of the diagram was much the easier of the two things. It was just as easy, moreover, to do it unscientifically, or, as one might say, by Molesworth. One was bound to say that the actual problems with which a workman was daily brought face to face were, in not a few cases, more complex and difficult than those to which the name of scientific was sometimes alone given. Thus, no engineer need be a mere tradesman or handicraftsman. In every grade, in every branch of their work they handled materials and carried out operations which gave them plenty of food for thought, and plenty of opportunity for scientific method. They had the proud distinction of belonging to the profession which, of all others, was *par excellence* scientific. And no profession of the delights of ignorance on the part of unworthy members of it could alter what appeared to be the plain facts of the case. He was sometimes amused with the zeal with which an enthusiastic physicist would proceed to throw all their mechanical units to the winds, and to improve upon all the ideas of engineers by expressing them in terms which were unintelligible except to a very select few. He had occasionally come down heavily upon such improvers of mechanics by suggesting that for every mechanical calculation they had to make outside an examination paper, engineers had to make twenty. So it was hardly fair to improve mechanics out of sight of the few people who definitely and positively knew it and use it in their every-

day life. But to come now more to particulars, and deal with things that were more present to their minds than some of these general considerations, let them work their way to the definite question of engineering calculations. How far and how often could an engineer arrive at definitely valuable results by calculation? To what extent could calculated results be taken to represent actual facts, and trusted in design and construction? These questions concerned all of them in different degrees, and on their answer to them depended to a great extent their method of work. It happened to be his good fortune to be at the head of a laboratory where they had special facilities for making inquiries into this very matter, directly and indirectly. He was, therefore, all the more ready to take that opportunity to speak about it. In the first place, he said that an engineer could never expect to arrive at valuable or trustworthy results by means of calculations taken from books, calculations which he did not understand, "rules," so called, from pocket-books, &c. Pocket-books, &c., were all very well to remind them of former facts which they forgot or did not carry in mind, and to give by tables the results of long arithmetical calculations. This was their legitimate and very important function. But, directly it came to taking up a formula which one did not understand, a's and b's and c's, putting in values and calculating results merely by rule of thumb—as far as one's self is concerned, the result was worth absolutely nothing, and few, if any, sensible men would trust such a result. In his mind that definitely put an end to the idea of certain mechanical engineers about universal engineering dimensions all ready calculated. There was plenty of engineering work to be done without actual formulæ—the object of so much dread and scorn—done well, and done scientifically. From his point of view it was a great advantage to a man if he could follow the reasoning, physical or mathematical, on which other parts of engineering work, and especially design, depended. Fortunately for most of them, there was for the most part no necessity that a man should be a great mathematician in order to be a fairly competent engineer. He had, or had had, the pleasure of knowing personally some of the greatest mathematicians of our time. With one exception not one of them would make his mark as an engineer, even if he confined himself to the scientific side of his work. He had also the pleasure of knowing some of our greatest, most original, and longest-headed engineers, and, so far as his knowledge went, hardly one of them had such aptitude for mathematics as would place him in even the second or third rank of mathematicians if he devoted himself to that most fascinating subject, But all the same there were engineers in plenty—not, certainly, as many as could be wished—who had succeeding in grasping thoroughly the science, mathematical and physical, of their profession without having lost the peculiar bent of mind which specially adapted them to be engineers. And into this class they doubtless wished to enter so far as circumstances allowed. The late Professor Rankine long ago pointed out that the so-called discrepancies between "theory" and "practice," between results theoretically best and those practically best, did not exist, could not exist, if only the theory were right. He feared they got into rather a slovenly way of talking on this subject sometimes. They said that a certain system of levers should theoretically enable them to lift 1,000 lbs. with 10 lbs. pressure, but that practically it only enabled them to lift 500 lbs. This simply meant that the theory was wrong, or at least incomplete. A complete theory would take into account all the friction at all the joints, giving to each its proper coefficient; all the variations from accuracy in the length of the levers, and so on. But if one chose to take theory on an ideal combination of frictionless rods, and apply it to an actual combination of frictional ones, of course the results would show a discrepancy. This reason was not to be sought in any fault in the theory, but in themselves, for expecting it to apply to a set of conditions for which it was by no means adapted. It happened in many cases that their engineering problems were terribly complex, both mathematically and physically; even to state them in any exact form was very difficult. So they contented themselves with an approximate representation of them by some theory or formula. The science of an engineer was not only

shown by his knowledge of the best working theory formulated or unformulated—very often the latter—but by the accuracy of his estimation of the extent to which his theory probably represented the actual case, of the extent to which he might trust his assumptions, and of the points in which the actual results would probably differ most from those given by the approximate theory. He had used the expression "unformulated theory." He had known men—he dared say they all had—who had wonderful physical insight, with very little mathematical knowledge. The exact process of reasoning by which they arrived at their results they had difficulty in putting into words, probably for want of familiarity with scientific terminology. But they had wonderfully accurate and correct notions as to where a given piece of machinery would give way, how best it ought to be strengthened, and so on. Of course such kind of knowledge was dangerous, because it was mainly based upon experience, and that within a certain limited range. It was, therefore, apt to give extraordinary results when applied outside that range. In itself it was to be respected, and within certain limits trusted. But the man possessed of this talent ought, if he had a right knowledge of himself, to be the first to mistrust his own judgment applied to matters beyond the range of his own experience. Then must step in some one who could give reason for the faith that was in him, who was capable of tackling a new problem, of seeing through it, and getting at its real meaning. But even here, results did not always presuppose actual formulæ. He rather insisted on this idea, because he would not like them to think that they were altogether shut out from the scientific side of their work by being bad mathematicians. Within certain limits, no doubt, the better capacity one had for mathematical reasoning, the better would one be able to work at the science of engineering. But he should like them to believe that a little mathematics, if only it be sound and accurate as far as it went, might go a long way. Also, that much mathematics misapplied might go only too short a way. He would now ask them to look at the different sets of considerations which go to determine the dimensions of an engineering structure or machine, and see how far they were subjects for calculation; and then he would say a few words about the calculations themselves. (I.) One set of considerations which they had to keep in mind was the architectural or æsthetic considerations. These considerations referred to form generally, and were very important. What was the criterion of beauty in engineering affairs? Or was it necessary to have one? He was not competent to lecture on art, and however enthusiastic he might be on the subject outside his profession he did not think that art took a very important place in engineering as generally understood. But at least in structural engineering the point of view occurred often enough to be pretty important. As to machines, he thought most of them would be disposed to use the word ugly to describe anything ill-proportioned; and they would consider a thing ill-proportioned if its different parts were unequal in strength or in wearing resistance; if, in fact, its metal was put in the wrong place. Sometimes metal had to be used for its own sake for weight and mass only, but that was seldom. As a general rule, uniformity of strength throughout the whole machine, absence of superfluous material everywhere, was the thing to be aimed at; and obvious departure from this rule was always ugly. In the case of structures, also, he believed that the true beauty of an engineering structure was to be found in the fitness of each of its parts to do its own particular work. He really believed that in time, as one learned to judge of engineering structures by proper standards, they would come to think those structures the most beautiful which were designed and proportioned from an engineering point of view. Recently he had an opportunity of seeing the large suspension bridge between New York and Brooklyn, and it appeared to him that it was singularly graceful, indeed handsome in a way that such structures seldom could be. In regard to cast-iron columns for rooms, it was quite a mistake to attempt too much ornamentation. He believed that surface decoration would be much more effective than mouldings. (II.) The next set of considerations determining the dimensions of structures and machines were

considerations of duration—of resistance to wear. He supposed that none of them had gone further into this matter than to gather from their own experience and other people's certain constants. But if they wanted to know how little they really knew about that matter let them look up Mr. Towers' experiments and see how an axle floats in oil and causes actual fluid pressure there at its center of nearly double the nominal pressure. Much interesting work remained to be done here, and the results were of that pleasing kind which had a direct relation to practical work as well as a high value in pure science. (III.) Now he came, lastly, to the principal set of considerations, viz., considerations of strength. Here calculation and the application for exact scientific formulæ and reasoning came in frequently. Here, therefore, of all places, it behoved them to know what they were doing in applying formulæ—for here, of all places, might a misapplication of calculations have disastrous results. Much trouble would be spared if engineers always kept in view that their results could not possibly be more accurate than their data. Now, their data were ordinarily conditioned by three things: (1) The accuracy with which the forces causing stress is known. (2) The accuracy with which the stress caused by the forces is known. (3) The accuracy with which the resistance of materials to stress is known. The engineer had first to see that he thoroughly understood his own problem—that he knows what question he has to ask, why he has to ask it, and what data he has to go upon. He had then carefully to examine his data, and to make up his mind as to how far they are certain, and how far the maximum forces are known. Next, he had to find some representative problem which can be put into a simple form, and which shall be as near as possible to the problem he wants, actual problems, taken in their completeness, being in almost every case too complex to deal with. Then he had to get his data put in the representative problem, and work out an answer. Lastly, he had to remember that the answer is to the representative, not to the actual problem; and he had to exercise his own judgment as to its application. But their society was called a scientific, not an engineering, society. He had been taking for granted, as he supposed he might fairly do, that practically they were all engineers, and that it was an engineering science that they had chiefly to do with. This, however, he hoped, was not altogether the case. But even if the science of engineering in its various aspects was the only one which they proposed to discuss in their society, he hoped they would individually give more or less attention to the other branches, especially those of natural science. An engineer was a man more or less trained to habits of accurate observation, more or less compelled to look into and around things that presented themselves to him. It was the very frame of mind for any science, not only that of their profession. For himself, he confessed he thought the principle a good one to know all about something and something about everything. Nothing increased one's enjoyment in life, made life so much worth living, as the gathering in of knowledge of all kinds about the world we live in, and the educating one's self to appreciate and understand it, and all the beauties it contains. And not only the physical world, but the world of men, the writers and thinkers, the musicians, the painters, the poets, the architects of the past aud of the present. Their profession, with all its fascinations and interest—in enthusiasm about which he himself would yield to no one—was essentially in the aspect most familiar to them a modern one. But they themselves were not modern. He urged them not to allow their profession to separate them from the ancient universe all around, nor to work and learn as if no one had worked and learnt before them, or as if their little specialty were the great domain of nature itself. Let them try and keep touch with nature, and with their fellow men, and not only with nature and the men of to-day, but with those of all time. The more they tried to know, the more they knew how little they knew. In one's own profession one might know much, perhaps be only too conscious of that fact. But let them listen to the musician, hear what the violins say, "riding the stormy symphony in royal orchestras," or tenderly telling of all that is beautiful in the lovely quartett. Where had they got the

ideas that were to be put alongside these, where in their sphere could they turn for similar means of helping, encouraging, ennobling others? Or, let them see how the poet weaves out of his own brain the thoughts that somehow seem to have been made especially for us too, who lived centuries after him; how he not only sums up our wishes, our desires, our aspirations, but in doing so raises them and us together into higher regions than they would otherwise have ever entered. Or, to come back once more to what folk called science, let them take a country walk with anyone who knew the flowers, the stones, the rocks, the trees, the insects, all the pictures that nature ever put before us, and see how desperately ignorant they would find themselves if they had not looked at those subjects. They would see how one man could live in the same universe with another, seeing the same things and yet knowing nothing about them, in fact seeing them not at all. He did not speak as a biologist or a geologist, he only knew enough of those things to know how fascinating they are, how much one may long to know more. But he knew even a small knowledge of them added to one's interest in life. Everything one sees or touches was full of life and interest. and yet more and more full of interest as one got to know more and more about it, and one saw how great everything is, and how wonderful, and how beautiful and how no work in nature was scamped, but was as exquisite in its minuted microscopic detail as it was grand in its mountain outlines. And one learned, or one tried to learn, how little one is one's self; how insignificant as compared with the vastness of nature, how ugly, sometimes, as compared with the beauty of nature. One got peeps into a world grander than the Alps that some of them loved, more beautiful than fairyland. One was awed into modesty and sobriety of opinion. One was constrained to try to make one's work worthy of its place in the universe, worthy to follow on the good that had gone before, worthy to be looked upon by our successors as something which, being the fittest, had survived to them. And perhaps after the end there might be spoken of them in some small measure the noble words of Marcus Aurelius, which had recently been applied to that greatest of men of science, greatest of Englishmen, he would say—Charles Darwin—words which perhaps expressed the character of an ideal man of science: "Remember his constancy in every act which was conformable to reason, and his evenness in all things, and his piety, and his serenity of countenance, and his sweetness, and his disregard of empty fame, and his efforts to understand things; how he would never allow anything to pass, but carefully examined and clearly understood it, how he bore to be blamed unjustly without blaming others in return, how he did nothing in a hurry, how he was not given to reproach people, how firm he was in friendship, how tolerant he was in freedom of speech, how religious he was without superstition; and how it were to be wished, when the last hour comes, to die as he died."

MEASUREMENT AND FLOW OF WATER IN DITCHES.

By AUG. J. BOWIE, Jr.

A Paper read before the Technical Society of the Pacific Coast.

In California, where the rainfall over large portions of the State is small and periods of drought of not uncommon occurrence, the development of the agricultural as well as the mining interests, necessarily resulting from the continued influx of population, will be dependent greatly upon the careful husbanding of the water supply. The sources of this supply are comparatively limited, and the problem of systematic irrigation will grow daily in importance from the necessities of the farmers; and the demand for water will steadily increase with the more extended cultivation of the soil.

The costs of construction and maintenance of the necessary canals and ditches (depending principally on their capacity)

will be of prime importance to the owners, who will appreciate fully the value of a correct determination of the flow of water. The easy-going farmer who purchases the water, will ultimately discover the necessity of knowing how much he is receiving, and then will come the demand for a standard of measurement of water.

The history of northern Italy, from the fourteenth to the eighteenth century, is replete with accounts of disputes and difficulties arising from the non-existence of some accepted standard of measurement of water. Similar troubles have arisen at times in the mining regions of this country, as can be attested by numerous court reports.

With the experience of the past, and in consideration of the future interests of the country, it would seem advisable that some uniform gauge and standard of measurement of water should be adopted.

The miner's inch has only led to confusion and is the relict of the Mexican and Spaniard, who possibly took it from the Italian. Like the Italian *oncia,* which varied in nearly every province, so its brother the miner's inch has followed suit to even varying in the same district.

In the construction of the various water-supply systems for the different placer regions in this State, certain experience in the measurement and flow of water has been acquired, and it has been considered of sufficient import to place some of the results of this work on record, with a view of assisting in clearing up the confusion about the *Miner's Inch,* and giving to those interested in the profession the benefits derived from the several works.

The Miner's Inch.—The Miner's Inch of water is a quantity which varies in almost every district in the State; no one gauge has been uniformly adopted, nor has any established pressure been agreed on, under which the water shall be measured. In some counties there are 10, 11 or 12-hour inches, and in others there is a 24-hour inch. The apertures through which the water is measured are generally rectangular, but vary greatly in width and length, being from 1 inch to 12 inches wide and from a few inches to several feet long. The discharges are through 1′, 1½′, 2′, and 3-inch planks, with square or with square and chamfered edges, combined or not, as the case may be. The bottoms of the openings are sometimes flush with the bottoms of the boxes, sometimes raised above them. The head may denote the distance above the center of the aperture, or again that above the top, and varies from 4½ inches to 12 inches above the center of the aperture.

The Smartsville inch is calculated from a discharge through a four-inch orifice with a seven-inch board top; that is to say, the head is seven inches above the opening, or nine inches above the center. The bottom of the aperture is on a level with the bottom of the box, and the board which regulates the pressure is a plank one inch thick and seven inches deep. Thus, an opening 250 inches long and four inches wide, with a pressure of seven inches above the top of the orifice, will discharge 1000 Smartsville miner's inches. Each square inch of the opening will discharge 1.76 cubic feet per minute, which approximates the discharge per inch of a two-inch orifice through a three-inch plank with a head of nine inches above the center of the opening, the said discharge being 1.78 cubic feet per minute. The Smartsville miner's inch will discharge 2534.40 cubic feet in twenty-four hours, though in that district the inch is only reckoned for eleven hours.

Other Inches.—The miner's inch of the Park Canal and Mining Company, El Dorado County, discharges 1.39* cubic feet of water per minute. The inch of the South Yuba Canal Company is computed from a discharge through a two-inch aperture, over a one and one-half inch plank, with a head of six inches above the center of the orifice.

At the North Bloomfield, Milton and La Grange mines, the inch has been calculated from a discharge through an opening fifty inches long and two inches wide, through a three-inch plank (outer inch chamfered), with the water several inches above the center of the opening.

Determination of the inch experiments at Columbia Hill.—To determine the value of this miner's inch, a series of experiments was made at Columbia Hill, latitude 39 N., elevation 2900 feet above the sea level. The *module* used was a rectangular slit fifty inches long and two inches wide; head seven inches above the center of the opening. The discharge was over a three-inch plank; the outer inch chamfered.

* Estimated by J. J. Crawford, M.E.

The size of the opening was taken with a measure (micrometer attached), which had been compared with and adjusted to a standard United States Yard. Time was read to one-fifth of a second; the level of the water (drawn from a large reservoir) was determined with Boyden's hook, micrometer adjustment. The following results were obtained:

One Miner's Inch will discharge in one second....	.026	cub. ft.
One Miner's Inch will discharge in one minute...	1.57	"
One Miner's Inch will discharge in one hour......	94.2	"
One Miner's Inch will discharge in 24 hours......	2260.8	"

The coefficient of efflux is 61.6 %. These figures are within the limits of $\frac{1}{500}$ possible error.*

As the two-inch aperture requires too much space for gauging large quantities of water, custom has changed the form of the module, and an aperture twelve inches high by twelve and three-quarter inches wide, through a one and one-half inch plank, with a head of six inches above the top of the discharge, is now used. These openings discharge what is accepted as 200 miner's inches.

A series of experiments was made at La Grange, Stanislaus County, California, latitude 37° 41′ N., elevation 216 feet above the level of the sea, to determine the value of the inch thus delivered in the claims. The results here given are the mean of a series of gaugings taken from nine different apertures, discharging in the aggregate 1,800 miner's inches.

The water was drawn directly from a flume and discharged into a small reservoir, across the lower end of which was fitted a gauge. The velocity of the water issuing from the flume was broken by several drops as it entered the reservoir, and the gauge at the lower end was raised sufficiently to prevent any flow due to an increased velocity which might have been required in the flume.

The level of the water was determined with a Boyden's hook.

The discharge from the module was caught in a flume and conducted to a box fitted and leveled for the purpose. Time was read to one-fifth of a second. The following results were obtained:

		Cubic Feet.
1 Miner's Inch discharged in	1 sec..	.02499
1 " "	" 1 min..	1.4994
1 " "	" 1 hour.	89.9640
1 " "	" 24 hrs.	2159.1460

Effective coefficient of efflux, 59.05 %.*

An experiment on a single aperture of this form, made by Hamilton Smith, Jr., gave a discharge of 2179.4 cubic feet per miner's inch in twenty-four hours. The 2230 cubic feet of the North Bloomfield inch can only be considered an assumed rough estimate of discharge in 24 hours for 1 miner's inch.

The theoretical velocity in feet per second, of a fluid flowing into the air, through openings in the bottoms or sides of a vessel or reservoir, the surface level of which is kept constantly at the same height, is equal to that which a heavy body would acquire in falling through a space equal to the depth of the opening below the surface of the fluid, and is expressed as follows:

$$v=\sqrt{2gh}.$$

In which v = velocity in feet per second.
g = the acceleration of gravity.
h = the height fallen in feet.

This is called Torricelli's theorem, which supposes indefinitely small orifices with thin sides, and assumes that the upper surface of the water and the orifices are under the same conditions as regards atmospheric pressure. Conditions and size of sectional area of the aperture, friction resistance of the air to motion and pressure of the atmosphere are all neglected.

The value of g varies in different latitudes, but for all practical purposes is taken as equal to 32.2.

$$\text{The theoretical head} = \frac{v^2}{2g}$$

The acceleration of gravity at latitude 45° = 32.17 feet per second, being represented by g, for any other latitude, l.

$$g'=g\ (\mathrm{I}-\mathrm{O}.\ 002588 \cos.\ 2l)\ \dagger$$

If g represents the acceleration of gravity at the height, h and r the radius of the earth, the acceleration of gravity at the level of the sea equals—

$$g'=g\left(\mathrm{I}+\frac{5h}{4r}\right)$$

* The experiments were made in 1874, by H. Smith, Jr., C. E.

* The experiments were made by the author.

† See professional papers, Corps of Engineers, U.S.A., No. 12, page 26.

Flow of water in open channels.—There is no generally accepted formula for determining the velocity of water in open channels. The tables based on theold formulas published prior to the works of D'Arcy and Bazin, in France, and of Humphreys and Abbot, in the United States, being founded on a data which ignore the important factor of the nature of the bed and the sides of the channel, have proved unsatisfactory. Hydraulic engineers have been compelled to rely for correctness of calculated result on the application of a combination of a few known laws with experimental data, which latter, though all important, have been too restricted for the deduction of reliable mathematical theory.

The formulas, in terms of dimensions of cross section and slope, are based upon the supposition of either "permanent" or "uniform" motion. Permanent motion approaches the condition of streams, permits changes of cross section and slope of the water surface, excepting sudden bends, causing eddies and undulations, but demands that the discharge from the different sections should be identical. Uniform motion, in addition, requires an invariable cross section and constant slope of the fluid-surface. The general formulas based on permanent motion, differ from those restricted to uniform motion, "by taking into account changes of living force produced by changes of cross section at the different points." * If these variations are unknown, the difference between the formulas disappears.

Chezy considered that the resistances encountered by water in uniform motion were in direct proportion to the length of the wetted perimeter, to the length of the channel and to the square of the mean velocity, from which he deduced the formula.

$$v=c\sqrt{rs}.$$

v is the mean velocity in ft. per second.

c a coefficient taken at a constant value.

r the mean hydraulic radius in feet.

s the fall of surface in a unit of length.

The equation indicates the relation of the mean velocity to the slope and the mean hydraulic radius. The value of the coefficint c has been demonstrated empirically to have a wide range. This formula, however, has been considered the simplest, and has been used by many engineers, different values being given to c, varying from 84 to 100 for large streams, and being as low as 68 for small streams. "Though there is abundant evidence," says Higham (p. 5), "that the latter is much too high for low values of v in earthen channels, and that 100 is too low for very large rivers, as high a value as 254.4 having being deduced from the Mississippi observations."

D'Arcy and Bazin, by their experiments on channels of moderate section with limited variation of grades, proved that the coefficient c involved not only r and s, but also a constant for the different degrees of roughness of the channel, the formula being applicable within certain limits of inclination and values of r.

Humphreys and Abbot make the velocity vary with the fourth root of the inclination, while Hagen assumes the velocity to vary with the sixth root.

Ganguillet and Kutter considered that the Chezy formula, $v=c\sqrt{rs}$. was the correct point of departure, but that the coefficient should be made variable, involving not only r and s, but likewise the degree of roughness, in the bed or channel.

Ditches in California.—In the mining districts of California, ditches are constructed boldly with steep grades and on irregular lines with numerous sharp curves. The cross sections, originally uniform, become more or less varied. Absorption, percolation, evaporation and leakage, reduce the flow. A distinct reliable factor for each of these sources of loss cannot well be incorporated in the coefficient of discharge. If, then, it is intended to cover all of these common sources of loss by such a coefficient, its value must be a material modification of values given commonly in the text books. It would be certainly an affectation of accuracy to apply so complicated a formula as that of Kutter in such a case, since the modifying conditions which can be estimated but roughly, call for a large reduction of the calculated result. This will be apparent from the measurements of discharge given further on. The simple formula, $Q=ac\sqrt{rs}$, expresses more fitly the result of experience in such cases, wherein—

Q—Is the quantity of water which the

* Humphreys and Abbot, Mississippi Report, p. 207.

ditch is capable of carrying in cubic feet per second.

a—The effective area of cross section of ditch as constructed originally, in square feet.

r—The hydraulic mean depth in feet.

s—The fall of surface in a unit of length.

c—A coefficient covering all common losses.

Examples of value of coefficient in Ditches.—In its application to the North Bloomfield Main Ditch,* (length 40 miles, sectional area 23.89 square feet, grade 16 feet per mile) with its abrupt turns and sinuous course, the value of the coefficient c, as determined, varies from 44.7 to 37.7 in accordance with the season of the year.

The Texas Creek† branch ditch is about seven-tenths of a mile long. Its sectional area is 13.5 feet and the grade is 20 feet per mile. The sides are rough and the curves are sharp. With a flow of 32.8 cubic feet per second, the ditch runs about full. The value of $c=33$. In connection with this ditch there is a rectangular flume 2.67′ wide × 2.83′ deep, made of unplaned boards, set on a grade of 32 feet per mile. The flume has some sharp but regular curves, and the water from the ditch runs it nearly full at these points. With the discharge 32.8 cubic feet per second, $c=59$.

On the Milton line, from Milton to Eureka, a distance of 19.4 miles, the sectional area of the ditch is 20.39 square feet, grade 19.2 feet per mile for the earthwork and 32 feet per mile for the flume. The line is very irregular, having many drops and chutes. The distance from Milton to the measuring box at Bloody Run is 29½ miles. The minimum established grade for the last 10.1 miles was 16 feet per mile, with a sectional area for a ditch of 23.05 square feet. The coefficient *c* determined from the gauging at the measuring box has varied from 22 in its leakiest condition to 31, which latter can be taken as correct for the present condition. In the succeeding 30 miles below the gauge, owing to a better character of ground, the coefficient reaches 41.

* Increase capacity of this ditch is limited by the pipes across Humbug Canon.

† For details of Texas Creek ditch flume, see paper by Hamilton Smith, Jr., Transactions Am. Soc. C. E., Vol. XIII, pp. 30-31.

The La Grange main ditch, 17 miles long, has a sectional area of 22.5 square feet, and a slope of 7 feet per mile. From the delivery at its Patricksville junction the coefficient *c* is determined to be 52, but it is based upon the assumption that the depth of the canal is three feet, whereas in the original construction it was supposed to have been made four feet deep, the discharge, therefore, due to such a sectional area, would diminish necessarily the ascribed value of *c**.

In all these canals, after the artificial banks are well consolidated, the water area is increased beyond the original excavation in the natural ground.

Accuracy cannot be expected in calculating the values of Q for proposed ditches of such character. Important losses must vary in every ditch, depending on the nature of the ground, and the character of the construction of the work and the season of the year. The feeders along the lines compensate largely for these losses. In order to be safe in estimating the capacity of a ditch, the value of the coefficient *c* for the dry season should be taken.

The following facts show the magnitude of the losses due to absorption, leakage, evaporation, etc.:

Three thousand miner's inches of water (a flow of 75 cubic feet per second) turned in during the dry season at the head of Bloomfield ditch, will deliver 2700 inches (67.5 cubic feet per second) at the gauge 40 miles distant. Twenty-four hundred inches of water (60 cubic feet per second) turned in at the head of the Milton ditch delivered formerly at the gauge, 29½ miles distant, 1450 to 1600 inches (36.25 to 40 cubic feet per second), but at present 2500 inches (62.5 cubic feet per second) turned into the head of the ditch, deliver 2000 inches (50 cubic feet per second) at the gauge. The exact loss of water between the head of this ditch and the measuring box is shown in the following summary, taken from the official records for the month of August for the years 1875 to 1882, inclusive. This month is taken as a dry month, as prior to that time the numerous side streams swell the amount delivered at the gauge.

* The grades given in all the above cases, from which the different values of C were calculated, exclude the drops, chutes, flumes, etc. Sectional areas represent minimum cross sections.

RECORD FOR AUGUST.

	Water turned at Milton, 24 hours, inches.	Water turned at Bloody Run, 24 hours, inches.	Per cent.
1875	44,000	34,950	79.4
1876	59,700	42,625	71.3
1877	67,875	44,700	65.9
1878	76,050	58,875	77.4
1879	82,725	51,350	62.0
1880	74,080	55,325	74.7
1881	66,850	48,325	72.3
1882	68,300	50,984	74.4

The Eureka Lake ditch, with 2500 inches turned in at the head, delivers at the gauge, thirty-three miles distant, about 1800 inches in the dry season.

The above statistics lead to the adoption of values of the coefficient c, varying from 31 to 45, in estimating the capacity of ditches on heavy grades of forty miles length flowing from sixty to eighty cubic feet per second, such as referred to—that is:

$$Q=31 \text{ to } 45\, a\sqrt{rs}.$$

The loss incurred in the distribution of water is denoted by the following figures, taken from the official records of two mining companies. The amount received is measured, at or near the distributing reservoirs; the amount used, at or near the pressure boxes. The difference shows the losses from leakage, evaporation, absorption, and wastage arising from excess of constant supply over the amount needed, with interruptions at the claim.

NORTH BLOOMFIELD COMPANY (24 HOUR-INCHES).

Year.	Amount Received.	Amount Used.	Loss.
1870 to 1879, including	5,838,865	5,504,758	334,107=6 per cent.
1880	945,550	920,612	24,938=2½ "
1881 *	950,340	866,962	83,378=9 "
1882	1,025,880	1,005,977	19,903=2 "
1883	862,660	836,251	26,409=3 "
14 years	9,623,295	9,134,560	488,735=5 per cent.

MILTON COMPANY (24 HOUR-INCHES).

Year.	Amount Received.	Amount Used.	Loss.
1882	685,933	635,884	50,049= 7 per cent.
1883 †	446,224	361,877	84,347=19 "
2 years	1,132,157	907,761	134,396=13 per cent.

* Much water ran to waste during 4 months, owing to cessation of work caused by litigation.
† English reservoir was destroyed June 18, 1883, from which source the main water supply was obtained.

SPEED ON CANALS.

By FRANCIS ROUBILIAC CONDER, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

I.

THE amount of resistance to the propulsion of vessels through narrow channels, due to the size, the form, and the surface of the channel, has not hitherto been fully studied. That this resistance increases in some ratio to the diminution of free water-way is known. In the "Minutes of Proceedings" will be found the statement that a steamer which attained a speed of from 16 to 18 miles an hour at sea, could not make more than from 8 to 9 miles per hour in the narrowest part of the Clyde; and that a boat which had a speed of 10 miles per hour in the Liffey, could not make more than 7 miles per hour in the Royal Irish Canal. The instant acceleration of a boat on passing into deeper water, which was mentioned in the debate cited, is well known to all boating men. But no formula for determining the proportion is to be found in engineering text books.

It was the advice of the author, on being consulted on one of the most important hydraulic projects of the day, that there should be instituted, in the first place, so thorough an investigation of the main scientific questions involved as to leave

no point open to hostile criticism. Among the special steps recommended to this end was "the commencement of a series of experiments for determining the form of cross-section best suited for canal navigation." The recommendation not having been carried out, the author has endeavored to apply to the elucidation of the question certain known facts as to the movement of water in channels of various forms, and as to the movement of vessels in open waters. As to the first, the experiments of Darcy and Bazin have been chiefly of use; and as to the second, those of the late Mr. W. Froude, M. Inst. C. E., on the wave-making resistance of ships.

Since the commencement of the inquiry, its importance has been accentuated by the extraordinary degree of attention that has been excited by the Suez Canal, and by the remarkable phenomenon of the retardation effected in the passage of vessels, although not amounting to an average of five each way per day. The average time occupied in actual movement through the canal has increased from seventeen hours per ship in 1876, to eighteen hours sixteen minutes in 1881, and to eighteen hours fifty-seven minutes in 1882; the speed slackening from 5.88 to 5.47 and 5.27 miles per hour. The time passed in the canal by each vessel has risen from thirty-nine hours in 1876, to fifty-three hours forty-six minutes in 1882. And a very recent report cites three cases of English mail steamers detained for seventy-one hours each in the canal. It is thus undeniable that the question of canal capacity for transport assumes a foremost rank among the practical engineering problems of the day.

In the absence of direct experiment, the only method available for the purpose of research is the comparative method. By this, mathematical considerations may be illustrated to a certain extent; and it may be at all events hoped to get so far as to determine where direct experiment becomes indispensable.

A vessel in its progress is continually displacing a mass of water equal to its own submerged bulk. This mass is usually calculated as proportional to the greatest immerged cross-section of the vessel; an approximation sufficiently close for the present inquiry. In open water, the vacuum that would otherwise be left in the wake of the vessel is filled by the water rushing in from all sides. It is unnecessary now to inquire how this movement of the water affects the speed of the vessel, as the ordinary performance of the latter in open water is taken as the unit of comparison.

When the movement of a vessel takes place in a restricted channel, the case is altered. There is no longer an indefinite supply of water all round the vessel to rush into the hollow at the wake. That hollow is filled, either by water which follows the movement of the vessel through the canal, or by that which flows as a counter current, being driven by the head due to the wave caused by the vessel. The first of these two actions is so limited that it may be neglected except in river navigation. The backward current, taken alone, will be directly as the speed and as the cross-section of the vessel, and inversely as the free water-way, or excess of the cross-section of the canal over that of the vessel.

Thus a vessel moving through a restricted channel has to encounter an opposing current which is a function of her own movement. Her speed will be the resultant of her proper motion and of that of the current, in so far as it affects her, and will be, roughly speaking, the difference of the two speeds. It is thus possible to obtain, subject to further elucidation, a general formula for the retardation of a vessel in a canal due to the back-current produced by her own movement.

Let A = the cross-section of the canal;
a = " " " vessel;
$n=\frac{A}{a}$;
V = the speed in open water;
y = the back-current in the canal;
x = the speed of the vessel in the canal.

Then $V=x+y$,

and, as the free water-way is = to $n-1$, $y=\frac{x}{n-1}$;

therefore $V=x+\frac{x}{n-1}$. (1)

Let, for example, $n=4$.

Then $V=x+\frac{x}{3}$,

and $x=\frac{3}{4}V$.

It results, however, from the experiments of Darcy and Bazin, as well as from theory, that not only must the area of the channel be regarded, but also its form, and the special nature of the wetted surface. Of all forms of channel, according to this authority, the semi-circular is that which offers least resistance to the flow of water; as it is also that of which thehydraulic radius is the largest, in proportion to the area.

But the hydraulic radius, or the area divided by the wet perimeter, $= \frac{\frac{a}{\pi}}{2}$, is approximately the same for a semi-circle and for a semi-ellipse of equal area. It will be at once admitted that, if an ellipse be taken of which the axes are, for example, as 6 to 1, it would be rash to assume that the flow of water would be the same through two equal semi-elliptical sections, one with the minor axis vertical, and the other with it horizontal. The periphery in each case would be exactly the same, and thus the ordinary formula, as dependent on the hydraulic radius, would be the same, and the volume and weight of water would be the same. But the hydrostatic pressure on the periphery, and therefore the frictional resistance, would be very different. Here, then, is a case where present formulas are inadequate fully to investigate the question of speed.

For convenience of navigation, the top width of a canal may be generally taken as from 7 to 10 times its depth; and, with the reserve just mentioned, the formula of the hydraulic radius may be applied to a semi-elliptical section of this proportion, as if it were a semi-circle. With this allowance, the formula above given, corrected for any difference of the hydraulic radius, as below exemplified, may be applied to those few facts which are attainable in the absence of further experiment.

The Suez Canal has a depth of 26 feet; a bottom width of 72 feet; sides sloping at 2 or 2½ to 1 to within 5 feet of the water-line, and a top width of 326 feet. Thus, with the exception of one or two portions of the course, there are flat shallow shoulders on each side of the navigable channel, which is only 112 feet wide at a depth of 16 feet. The area of the cross-section thus formed is 3,862 square feet, and the hydraulic radius is 12.34 feet at best, and in some parts of the canal not more than 10.12 feet.

A semi-elliptical section, 163 feet wide at top, and 30 feet deep in the center, would have an equal sectional area of 3,862 square foot, and a hydraulic radius of 21.31 feet. Thus, according to the usual estimate of the value of the hydraulic radius, whatever be the resistance due to the back current caused by the movement of a vessel in the canal (as determined by the formula $V=x+y$), it will be increased, owing to the resistance to that current due to the bad form of the trapezoidal section, as measured by the hydraulic radius. This as before observed, is treating the semi-ellipse as equivalent to a semi circle. A comparison of the two sections will show, that the fair-way afforded for vessels of 20-feet draught by the semi-elliptical section would be equal to that obtained by widening the existing trapezoidal section by 28 feet. But the present inquiry is as to retardation due to section.

In 1870, the "Warrior" steamed through the Suez Canal in twelve hours and fifty minutes, being at the rate of 6.85 knots per hour. The vessel of that name in the Royal Navy is 380 feet long; and, with an immersed midship section of 1,219 superficial feet, and an indicated H.P. of 5,469, is credited with a speed of 14.386 knots at sea. The application of the previously given formula (1) will be as follows:—

$A=3,862$ square feet.

$a=1,219$ "

$n=3.16.$

$V=14.356$ knots per hour.

$y=\frac{x}{2.16}=4.544$ knots per hour.

$x=9.812$ knots per hour.

The back-current y has thus a velocity of 7.68 feet per second. As the hydraulic radius of the canal is only about one-half that of the best form of channel for an equal cross-section, the resistance due to this speed (58.98) has to be doubled, and the square root of the product abstracted. This gives a retardatory current of 10.86 feet per second, or 6.429 knots per hour, and reduces the value of x to 7.927 knots per hour. The speed

actually maintained was only 6.83 knots per hour, and the amount of direct retardation due to the section of the canal might account for the difference. But the state of the canal in 1870 was such as to reduce the maximum speed in several parts of the course, although a higher speed is attained through the lagoons. In point of fact, the speed obtained has generally been regulated rather by regard to the damage caused to the banks by the wave produced by the steamer than by other considerations.

On application to the Admiralty to ascertain whether the ship in question was really H. M. S. "Warrior," it proved that it was not. In 1874, according to a Parliamentary Return (C. 1392, 1876, p. 69), a British ship called "Warrior," of 797 tons net tonnage, was re-measured by the Suez Canal Company. The area of cross-section of a vessel of this size will be only about one-fifteenth part of that of the canal. The retardation from back water will therefore be only one-fourteenth part of the speed, and the smaller vessel, with proportionately much smaller horse-power, will beat the larger in a restricted channel.

In the Forth and Clyde Canal, according to a statement lately published by Sir Arthur Cotton, R. E., the sectional area of the canal is three times that of the boats. A speed of 7 miles per hour in the open river is reduced to 5 miles per hour in the canal. According to the formula (1) (taking areas alone, without correction for the hydraulic radius), a speed of 7½ miles per hour in open water would be reduced to seven miles per hour where the channel has an area between ten and twelve times that of the boat, and to 5 miles an hour where the areas are as 3 to 1. On the River Lee, according to the late Mr. Beardmore, M. Inst. C. E., with a boat having a cross-section of 50 square feet, an increase of section from 165 to 209 square feet was attended by an increase of speed of ¾ mile per hour. It is not, however, stated on what speed the increase was obtained. At a rate of motion equal to 10 miles per hour in open water, the difference of velocity due to the difference of areas would be 0.6 mile per hour, and there would be a slight further gain due to the improvement of the hydraulic radius. These examples, deficient as they are in precision, may perhaps be taken as evidence that the effects of retardation due to restriction of area, and to bad forms of channel, are by no means exaggerated by the formula above suggested.

The effect of rough sides, projecting jetties, and the growth of weeds, either at the side or at the bottom of the canal, as also that of mud in suspension in the water, have further to be taken into account as increasing retardation. Coefficients of some of these conditions are given in standard works on hydraulics. The effect of all such obstructions must be to retard the flow of the back-current along the sides or the bottom of the canal. But as the back-current, as a whole, is a function of the speed of the boat, this retardation of a part of it must be accompanied by a corresponding increase of speed in the part of the current farthest from the obstacles, that is to say, against the sides of the boat. It is thus evident why the foul state of a canal or river exercises that directly retarding influence on speed of transit which persons familiar with canal navigation know to be produced under such circumstances.

There is one important point as to which direct experiment appears to be necessary before it can be attempted to formulate a complete theory of retardation in canals; that is, the respective influence of width and of depth on speed. As to this, the data at present available have somewhat anomalous results. Thus, on an Indian canal, 60 feet wide, an officer in charge of the Madras Public Works department informed the author that as the depth of a long reach increased from 6 to 12 feet, the speed of a light steamer increased from 5 to 10 miles per hour. And the extraordinary starts, mentioned by several of the speakers in the debate above cited, made by boats in passing into deeper water, seem to be more rapid than is explicable by mere difference of area.

It may probably be found that these apparent anomalies are referable in part to the relation between the form of the vessel and of the wave that it produces, and that of the canal. Thus, on the Indian canals, where the boats have a draught of from 15 to 21 inches, and a horizontal entrance, being in some cases

propelled by a single paddle-wheel at the stern, the displacement of the water is effected downwards, and the slightest variation of depth is instantly felt. In other cases, especially in river navigation, where the entrance is vertical, and throws a wave to either side of the channel, it is possible that the effect of a change in width may be more directly felt than that of a change in depth. As to this, although a simple formula may be suggested, direct experiment is highly desirable.

Where the cross-section of a canal is very small in proportion to that of the craft navigating it, the advantage to be obtained by enlargement is most conspicuous. Thus, for the same boat, in passing from a canal where $A=2a$ into one where $A=3a$, there will be an increase of 33 per cent. in speed in the latter as compared with the former. Suppose the same speed to be maintained in the two cases, the cost of traction will be nearly as 8 to 5. Considerations of this nature are of the highest importance, as determining the section that should be given to any new canal. In the case of a ship canal, which is the class of enterprise on which much public attention is now concentrated, it appears from the preceding investigation that a gain of 1.8 knot per hour may be attainable by the adoption of a scientific section, without any increase in the quantity of excavation. This is equal to a gain of from 17 to 18 per cent. in time; or, if the same speed be maintained, to about 21.5 per cent. in cost of traction.

The speed maintained on inland waterways is kept down by (1) the changes of level; (2) the increase of resistance, which is as the square of the velocity; and (3) the fear of eroding the banks. Fig. 6 shows the ratio of increase of resistance to increase of velocity in open water.

Human labor is still employed for towage on some of the Dutch, Belgian and German canals. Boats of from 15 to 26 tons are towed by men at a speed of 1 to 1⅛ mile per hour. Dr. Meitzen, a German authority, allows a duty of 11 miles a day, including all stoppages.

Steam tug-boats on the Belgian canals are restricted to a speed of 2⅝ miles per hour, and on the wider rivers to 4½ miles per hour. On the canal joining the Tiege to the Vistula, steam-tugs draw trains of barges 410 feet long, the speed being restricted to 3 miles per hour. The steam-tugs put by Mr. Beardmore on the River Lee towed from 50 to 60 tons, at from 2 to 2½ miles per hour, in the cuts, 3 to 3½ miles per hour in the larger sections, and 5 miles per hour in the Thames. On the Grand Junction Canal the speed of a steamer towing one vessel is put at from 3 to 3½ miles per hour. On the Rotterdam Canal four boats, of 130 tons each, are towed by a propeller steamer, which also carries cargo, at 5 miles per hour. In Sweden, as well as in Holland, where the channels are narrow, the usual speed is 3½ miles per hour, but 5 miles an hour is generally attained, the difference depending on the area of cross-section.

In curves and shallows, in narrow canals or rivers, a breaking wave first appears at from 3 to 3½ miles per hour. At 4 miles an hour the effect of the wave on the banks becomes injurious. At 5 miles an hour the wave increases, breaking over the towing-path, and being followed by other waves in succession. In parts of the Clyde from 120 to 150 feet wide, and about 10 feet deep, vessels of from 120 to 150 feet long, with from 16 to 18 feet beam, and from 5 to 6 feet draught, are propelled by engines of from 80 H. P. to 100 H. P., at a speed of from 8 to 9 miles per hour. At this speed a surge rises at from 2 to 3 miles ahead, and a wave is caused which measures 8 or 9 feet from the crest to the bottom of the trough. A head of this height gives a theoretic speed of 16 miles an hour, which shows a loss of 50 per cent. due to restriction of channel.

A speed of 5 knots per hour, or 8.37 feet per second, corresponding to a head of 1.08 foot of water, is the limit of speed fixed for the Suez Canal. This may perhaps be taken as the normal speed to be sought on the canals of England. On the determination of the normal speed, and of the tonnage of the boats to be accommodated, will depend not only the steam power required, but the section of the canals and the dimensions of the locks. A speed of about 30 miles a day, including stoppages, is even now attainable on English canals.

The loss of time due to locks is more serious on many English than on most continental canals, although there are

canals in Belgium with far more numerous locks than the average in England.

A rise of 8 feet is overcome on some canals in three and a-half minutes. On the Aire and Calder Navigation, Mr. Bartholomew, M. Inst. C. E, has attained a rise of 7 feet 6 inches in two and a-half minutes, and a rise of 13 feet 6 inches in three and a-half minutes. These figures give the rates at 2.3, 3, and 3.8 feet per minute, either for ascending or for descending, a speed which may probably be increased by better construction of locks.

By the use of lifts or of inclined planes, where a rise can be concentrated, much greater speed than the above may be attained. A height of 51 feet is cleared by the Anderton lift in eight minutes. On the Morris canal incline a height of 51 feet is overcome in three and a half minutes; and on the Blackhill incline, on the Forth and Clyde Canal, a height of 96 feet is overcome in ten minutes. The corresponding speeds are 6.37 feet, 14.5 feet, and 9.6 feet per minute, or about three times the speed now attained by locks.

The heights to be overcome in crossing England from the Thames to the Severn are, 358 feet on the 204 miles of the Wilts and Berks route, 474 feet on the 180 miles of the Kennet and Avon route, and 392 feet on the 206 miles of the Thames and Severn Canal route. This gives an average change of level, counting ascent and descent, of 4.14 feet per mile, or a little more than one-fourth of the ruling gradient laid down by Mr. Robert Stephenson for the London and Birmingham railway. From the Report of the Select Committee on Canals, p. 125, it appears that on 2,440 miles of canal there exist one thousand nine hundred and one locks, or a lock to every 1.37 mile. This gives an average rise or fall for the system, as far as it is represented by the times returned, of 5.84 feet per mile. At the rate of 3 feet per minute, these figures show a retardation of from 1 to 1.75 minute per mile over the Thames and Severn lines of junction, and of 1.95 minute per mile as the general average.

Taking the more uneven section, a running speed of 5 knots, or of 5.76 statute miles per hour, on an ordinary English canal, will be reduced by the delays caused by the locks to a speed of 4.9 miles per hour; allowing of the performance of a distance of 58.8 miles in twelve hours, or nearly double the speed of prompt canal service at present. This is about one-third of the running speed of the mineral trains on the railways of the United Kingdom. But a terminus to terminus speed of 5 miles an hour is as much as is sometimes attained by the mineral trains; and it is in evidence that the deliveries of despatches made by river and canal from Gloucester to Birmingham are as prompt as those made by railway between those two important towns.

The cost of speed on railways may be ascertained by analyzing the expenditure, and arranging it under the heads of costs independent of velocity, costs increasing with velocity, and costs decreasing as velocity increases.

The object of the author in the foregoing paper has been, first, to call the attention of the profession, and of those who consult its members, to the character of the experiments which are requisite for the determination of the true theory of loss of speed in canals; and, secondly, to propose an hypothesis, illustrated by such facts as are at present on record, according to which future observations may be so grouped as to lead to the ultimate determination of the true theory.

DISCUSSION.

Sir Joseph Bazalgette, President, said, as the paper had taken a large and comprehensive view of the subject, it was perhaps a little difficult to know exactly how to treat it so as to make the discussion most effective. Probably the best way would be to treat first upon the practical and theoretical considerations which govern the section of a canal with reference to the traffic to be passed through it; next to deal with the best means of traction along the canal; and then to draw a comparison between the cost of transit by inland canals and railways. If the discussion was divided under those three heads by those who were about to speak, more effect would be given to the arguments which might be deduced therefrom.

Mr. Conder said he had only been able to touch the fringe of a very large

subject, and one which must be of great interest to the members of the Institution, as it was to all the manufacturing districts of the country. It was a subject on which the Continent was greatly in advance of England. In Belgium, Holland, Germany and France, inland navigation had an importance quite equal to that which it assumed in England before the commencement of railways. In France, at the present moment, a very large sum was annually laid out by the State for the completion of the internal navigation of that great country, which would allow vessels of 120 tons to pass from sea to sea and from frontier to frontier. The question of the comparative cost of land and water transport, which had been repeatedly and carefully investigated in France by the Chamber of Deputies, by the Senate, and by the Ministry, had not been hitherto investigated to any great extent in this country, although it was now being considered by a Select Committee of the House of Commons. He had to apologise for bringing the subject before the Institution at a somewhat late period, but that was not altogether his fault. He had knocked three times at the door of the Institution in the hope of introducing the subject. He had not a word to say against the wisdom of the Council in the selection of papers, but he thought it due to himself to make the apology which he had done for not having brought forward the subject earlier and in the first instance, before the Institution. He had, however, the advantage that he could now set aside the question of pounds, shillings and pence, because the evidence printed by the Select Committee upon canals was so full and clear as to have settled the question of comparative cost. Members would find in the evidence, and in the tables attached to it, the cost of land and water transport in Austria, Belgium, Germany, France, Italy, the United Kingdom, the United States, and New South Wales, in sea passages from Liverpool to New York, from England to Bombay and Calcutta, and in coasting steam collier passages. There was a sufficient accord in all those various sources of information to enable any one to see that, in round numbers, the cost of inland water-carriage did not exceed one-third of the cheapest cost of inland land-carriage, even with the great advantage of the railway system. It was, however, but fair to mention that the railway lengths were almost invariably shorter than the canal lengths; and therefore from the two-thirds there would have to be deducted the difference between the ordinary length of the railway and the ordinary length of the canal. That, however, would still leave the cost considerably less than one-half. As to the question of speeds, it might be taken that the proportion was as a day to a week. There was so much loss of time in shunting, and in the terminal distribution of mineral traffic by railways, that many witnesses before the Committee had given distinct evidence of delivery in equal time by railways and canals. But taking the proportion as a week to a day (which was least favorable to canals), it appeared to him that what the manufacturing interest of the country demanded was to be able to choose whether or not it would pay for speed. If manufacturers had the choice of taking a week for their work and paying accordingly, or sending their goods in a day and paying accordingly, a sifting of traffic would take place which would enormously benefit the railway shareholders. A very much larger tonnage could then be carried upon railways, because only that kind of traffic would be carried which could afford to pay for rapid speed; a proportionate rate would of course be paid, and two or three times the amount of goods could be carried on the same track. He wished to point out that the actual capacity of a conduit, whether a canal, a railway, or a pipe through which a stream of bullets was dropped, did not depend upon actual speed. At whatever speed a number of trains or boats were sent out, they would arrive the same distance apart at the other end, and in the course of the year the same amount of traffic would pass over the line. The moment a different speed was introduced, the line was strangled, and its power of conveying traffic greatly diminished. Take as an instance French railways and English railways. It was perhaps not generally known that the earnings of the six great French railways during ten years, from passengers and goods alone, had been nearly pound for

pound per mile the same as on the English lines. The English earnings included in addition about £800 per mile for mineral traffic, which was not represented on the French lines, because, although on the Great Northern Railway of France there was a considerable mineral traffic, it was at a freight so much higher proportionally than the English freight that it did not greatly affect the comparison. Taking the legitimate railway traffic as equal, and taking the mineral traffic, on which certainly not more than 10 per cent. profit was made, as additional transport on the English lines, it would be found that the capital of English lines was upwards of 50 per cent. more than that of the French lines. It seemed difficult to conclude otherwise than that there was thus laid on upon English lines 50 per cent. more cost for the sake of carrying a double and mixed traffic; and that upon a third of the capital the receipts were only £80 net per mile. He desired to call attention to Fig. 6, showing the effect of speed on the cost per hundred ton-miles. The diagram showed the costs of the railways of the United Kingdom for 1878, distinguishing those which he considered to be independent of speed, those which increased with speed, and those which diminished as speed increased. The second, he presumed, would be principally for fuel; but he had added the whole cost of the locomotive repairs. It would be seen that there was a certain point, which on the average of English lines was about 30 miles per hour, where the lowest cost was attained by the running speed. Thus the idea of saving by running enormously heavy trains at lower speeds was a mistake, because, although there was a considerable decrease in the cost of fuel, the increase in wages was sufficient to eat it up. On the question of hydraulics there was an expression in the paper to which some of his brother engineers would probably take exception. Whether he was right or wrong, he had not made the remark either from neglect or ignorance. He was willing to bow to any correction he might receive, but he desired to explain his own views. He had spoken of the sweeping, rolling force of water as being determined by the total hydraulic pressure. He was aware that he was in opposition to the text books, and also that the supreme authority of Newton would be quoted against him. He had, however, gone through the passage in Newton, which would be found in a long scholium to the fortieth Proposition of the Second Book of the *Principia*. Newton made a number of beautiful experiments by dropping balls of wax loaded with lead through 15 feet of water, and he connected with those experiments the dropping of various bodies from the dome of St. Paul's, for a height of 200 feet, and his result was that resistance in a fluid was proportionate to the density of the fluid, and not to what was understood as hydrostatic pressure. With that he unhesitatingly agreed. There had also been some beautiful experiments of Coulomb to the same effect. He had vibrated plates in water by the torsion of wire, and had arrived at the same result. The result indeed was an integral part of Newton's system, and he supposed that no one would be bold enough to question it. But it should be remembered that that was a consequence of the incompressible character of water. In air it was not exactly the case, although as far as Newton's experiments went he had not the means of telling the difference; but the air being more compressed near the surface of the earth, according to Newton's own theory, the resistance would be greater, and therefore the velocity would be checked. To say that friction in a fluid was the same thing as the friction of the fluid itself rushing over the surface of the ground, appeared to him to be contrary to common sense. It was in fact saying that if there was a river slope, or a mountain side, down which there was poured an inch of water at a given velocity, which was regulated by the slope, and then the inch was increased to 10 feet of water, that quantity, at the same velocity, would not erode the bed of the mountain side more than the inch of water would erode it. It required no argument to show that such a statement would be absurd. That view of the case, however, struck at the root of all existing hydraulic formulas. In questioning the accuracy of those formulas, he wished to support himself by the authority of men whose names were well known. Among the latest books on the subject published in this country, were those of Mr. L. D'A. Jackson. He would

say nothing of the books themselves; but would only refer to the testimony in them of the utter valuelessness of all hydraulic rules. Mr. Jackson had pointed out that of three sets of formulas the results were utterly irreconcilable. Professor Macquorn Rankine had also plainly stated that there was no general hydraulic theory for the laws of the friction of fluids, and that it was necessary to adopt a rule of thumb, and arrive at the result in the best way possible. So also with regard to the paper read before the Institution by Major Alan Cunningham,* in which so many minute measurements were given, the result was still the same —the want of a general theory. Amongst those who had studied the question was Mr. Graef, the author of the latest mathematical book on hydraulics, who had endeavored to apply the resources of analytical investigation in tracing the movement of every filament of water, and the result at which he had arrived was that the only thing that was absolutely clear was *la courbe des debits.* That meant taking a certain vertical in a river and gauging the actual passage of the flood from time to time, and comparing the passage of the flood and the height of the hydrometer, and plotting the heights and quantities. None of them, he said, were perfectly right, but by plotting them and then drawing a fair curve through them, the best result would be obtained. That was the very latest outcome of the highest mathematics on the subject. It appeared to him that the difficulty to be contended with was the thoroughly non-mathematical character of the formulas. The formula $V=c\sqrt{rs}$ was the basal formula of all hydraulic works. It was represented in a more elegant form by Hagen, who gave $V=C.R.^{z}I.^{z}$ The former formula might be considered as indicating the square root, but American engineers were not satisfied with the square root; they had plotted the third and fourth root and some others to see which fitted best, and found that for one set of experiments the square root was best, for another the cube root, and for another the fourth root. For that reason he thought that Hagen's formula was more intelligible than the ordinary one. But he ventured to attack the ordinary formula root and branch as erroneous in every item, and proposed to substitute for it the simple formula $V=u+v-r$, where u represented the measured velocity at the entrance on a given section, v the theoretic acceleration due to gravity on the section, and r the sum of resistances in the section. These once determined, would then be of permanent value.

Mr. J. H. Taunton was quite sure that the Institution would appreciate any efforts made by new minds in the advance of science to build upon old foundations so long as those efforts were based on the results of experience. He thought the author had not quite understood, or that he had underrated, what had been done before in ascertaining the resistance due to the passage of boats through narrow channels. One of the founders of the engineering profession—Smeaton—had studied the subject in early days. He had also studied the form and proper section of a canal, and he stated that the proper speed on a canal for a horse to travel was from 2 to 2½ miles an hour, and that the useful effect of the horse was 650 tons hauled 1 mile in a day. Whether the horse had been replaced by another agent or not, the effect calculated still remained. Subsequently Mr. Bevan had investigated the same subject with similar results. There was also the extended experience of a Past-President of the Institution, the late Mr. James Walker, who in May, 1827, submitted a paper to the Royal Society, "On the Resistance of Fluids to Bodies Passing Through Them."* Still later there was the experience of the late Mr. H. R. Palmer, Vice-President Inst. C. E., who had made a variety of experiments on the Ellesmere canal, the Grand Junction, and other canals in different parts of the country, especially the Irwell and Mersey canal.† Those experiments had been investigated by Professor Barlow, who derived from them the conclusion that the resistance to the passage of boats through such canals as existed in this country was as the cube of their velocities, and not, as stated in the paper, the square of their velocities. There were also a variety of experiments, conducted by means of fly-

* Minutes of Proceedings Inst. C. E., vol. lxxi. p. 1.

* Philosophical Transactions, 1828, p. 15.

† Transactions Inst. C. E., vol. 1, p. 165.

boats, upon which the late Sir John MacNeill was requested to report.* The result of the experiments was that with a velocity of $2\frac{1}{2}$ miles per hour the power necessary to move 1 ton was $2\frac{1}{4}$ lbs.; with a velocity of 4 miles an hour from 7 to 11 lbs.; and with a velocity of 5 miles an hour (which was spoken of by Mr. Conder as the velocity with which boats were to traverse canals in future), the tractive force necessary was from 20 to 30 lbs. Those results were not encouraging, and did not point to the success of any suggestions such as those made in the paper. They did not point to any means of working traffic at a faster rate than it was worked at present, and they showed conclusively that the proper rate for moving heavy goods on canals such as existed in this country was from 2 to $2\frac{1}{2}$ miles per hour, which agreed with Smeaton's original calculation. He would not enter into the question of the formula referred to by the author, except to say that it did not appear to him to be of a sufficiently distinctive character to show what resistance was due to the width and what was due to the depth of the canal. He might be permitted to mention some experience of a practical character bearing on the subject of boats traversing canals. Midway on the Gloucester and Berkeley Canal, which was a length of level water of about 16 miles, was a place called The Junction, where it joined the Stroud water canal, about 8 miles from Sharpness. Boats built with a longitudinal bottom traversed that distance of 8 miles with an ordinary horse, and carrying 60 tons, in about three hours, while boats of the same character, built with a cross-bottom, required about four hours to traverse the distance, the slight difference being that the angle of the boats built with longitudinal plank was removed. That was a fact within the experience of all the boatmen who worked on that navigation. On the navigation with which he was especially concerned, the boats were built so as to carry as large a cargo as possible, and they had perfectly flat cross bottoms; while the boats on the Wilts and Berks Canal had a cradled bottom and a keel. Their own boats, after the horse was unhooked, would pull up in little more than their own length, while the other boats with a keel bottom would run three or four lengths before stopping. Those were simple matters, but they were indicative of points of some importance bearing upon the subject under discussion. As reference had been made to the Thames and Severn Canal, which he had had under his management for many years, he might be allowed to say a few words on the subject of lockage. He could not conceive anything more erroneous than the way in which that subject had been treated by the author, who had stated the total distance from the Thames to the Severn to be about 206 miles, but it was really 186 miles. The height of the summit level above the sea, instead of being 392 feet, was 365 feet. The author had taken the whole length and doubled the ascent and descent for lockage, and so obtained 4.14 feet as the average fall per mile, which he stated was much less than the ruling gradient of the London and North-Western Railway. That was a singular way of obtaining a ruling gradient. Whatever might be done upon a railway, it was certainly not the way to deal with the flat level water such as was traversed in a canal. In the Thames and Severn Canal, which crossed the Cotswold Range, there was a lockage ascent from the Severn of 365 feet, and then a descent to the valley of the Thames of 128 feet, the ascent per mile being 24 feet, and the descent per mile 10 feet. He had worked it out carefully, taking the total mileage and the levels. The author proceeded to say that Mr. Bartholomew had attained, in emptying his locks, a speed of 3 feet in a minute; that was very likely. No doubt, with large paddles, a speed of that kind might be obtained. In order to find the speed attained on the Thames and Severn Canal, he had on the previous day made observations on the passage of a boat through three locks, and the result of those observations was that the locks, having a capacity of about 10,000 cubic feet, filled and carried an ascending boat in six minutes, and emptied for a descending boat in five minutes. There was a difference in speed, because there was a difference of velocity at the paddles. The lower gates having a greater head of

* Ibid. p. 287.

water on them than the upper gates. The locks were 8 feet deep, giving somewhat more than 1 foot to a minute. On the narrow canals, only 7 feet, there was a greater speed, because, instead of 10,000 cubic feet, the contents to be emptied were only 5,000 cubic feet. No doubt there were some locks which could be cleared in from two to three minutes. It was not, however, the question of the speed attained, but it was the practical question of working the locks that had to be considered. A boat was of course delayed in approaching a lock, and it might have to be tied up for a time on arriving there. Wanting the lock empty, the boat might find it full, waiting for a descending boat. The result of his own experience was that it took twenty-four minutes per mile for the ascending locks, and ten minutes per mile for the descending locks, and the result was that in a distance of 35 miles it took eight hours and twenty-one minutes in working through the locks. It would seem, therefore, that any calculation founded upon the idea of 1.37 minute per mile due to lockage being sufficient was altogether erroneous as far as actual practice was concerned.

Mr. E. J. Lloyd desired to confirm what Mr. Taunton had said, by two experiments of his own with regard to the question of retardation. A steamboat with one boat following started from Stockton, Warwickshire, for London. The steamer carried 22 tons, and the following boat 29, making a total cargo of 51 tons. He had been told that the boat had traversed a part of the canals under his management at the rate of 4 miles an hour, and he thought he should like to investigate the matter somewhat further. Hearing that the boat was again in his district, he met her, and traveled with her over a portion of the canal where she was said to have acquired the speed he had named. He then found that she did acquire that speed, the canal at the point where the trial was made being exceptionally good. At a speed of 4 miles, for a distance of 109 miles, the passage would occupy twenty-seven and a-quarter hours. He had also measured the time occupied by the locks, in the same way as that in which it had been taken by Mr. Taunton, and he found that it varied, being on the northern section of the Warwick canals two minutes; on the southern section one and three-quarter minute, and on the Grand Junction two and a-quarter minutes. Taking the number of locks passed and the time occupied as above stated, it amounted to three hours nineteen minutes for passing the locks, making the total time occupied in motion thirty hours forty-five minutes. But he found that the boat took forty-nine hours to get to London, so that there was a retardation, due to other circumstances than the passage of the channel and the locks, equal te nineteen hours. Another boat started from Birmingham for London, a distance of 148 miles, and the time occupied was sixty hours; the cargo carried by the steamer being about 16 tons, and by the following boat 24 tons, making a total of 40 tons. The calculation was that the boat was to travel at 3½ miles an hour. She was capable of passing in a cross-section of 120 feet at that rate; but it was found that in running over a section of 9 miles, she did the distance in exactly two and a quarter hours, which would be at the rate of 4 miles per hour. The time taken by the locks was five hours and six minutes, making a total of forty-seven hours and six minutes, showing a retardation by other causes of thirteen hours fifty-four minutes. It might be asked, how was the thirteen hours fifty-four minutes occupied? He had taken out the number of boats passing through that section of canal within the last year, and he found there had been issued upwards of 22,000 bills of lading, and this number of craft was exclusive of light boats, which were passing and re-passing on the canal, each one causing various delays to the progress of every other craft. One of the great causes of delay was that the boats could not arrive at the locks exactly at the right time. A rising boat might arrive just as the lock was beginning to get ready for a descending boat, and there was thus a considerable loss of time in waiting for turns. Another source of delay, he was sorry to say, was the non-removal of shoals. A third reason was the retardation of every boat in approaching a bridge, and in acquiring its full speed after it had passed. Sharp turns in the water-way and bad or indifferent hauling power also caused delays. Taking those matters into consid-

eration, and making an approximate estimate of what was lost by them, he found that it was not less than fourteen hours on the journey from London to Birmingham. The author had stated that a steamer on the Liffey attained a speed of 10 miles an hour, and only went 7 miles an hour on the Royal Canal. That was very likely because she would probably then be within 4 inches of the bottom, and there would be no room for the water to go under her. If there had been sufficient space for the under-current, she would have probably made another mile or a mile and a-half. The diminution of the speed in the Suez Canal was, in all probability, due to the fact that the current caused by the great draught of the vessels passing through the canal had carried off the top of the slope in the trapezoidal section at the bottom of the canal, and the displaced sand had narrowed the fairway of the canal at the bottom, and probably also somewhat diminished the general depth. He was not aware whether it had been ascertained that there had been any decrease in the depth of the canal, but he thought it very probable.

It was stated in the paper that Mr. Beardmore in some experiments upon the River Lee had got an additional speed of ¾ mile in consequence of a comparatively small alteration in a section. It so happened that he was with Mr. Beardmore on the occasion of one of his trials. It was not right to say that there was an additional speed of ¾ mile, because he believed the maximum speed was 3½ miles, and the average 2½ miles. The boat was altogether unsuitable for experimental purposes, and it was not very remarkable that when they got into deeper water they managed to go a little faster. With regard to the passage of water through locks, the late Mr. James Potter, M. Inst. C. E., constructed a new portion of canal in the neighborhood of Birmingham, which was now under his control. Mr. Potter speeded the locks at eight seconds per foot of fall. An endeavor was made to work the canal with paddles of the size required to attain that rate of fall, but it was found that the destruction of the canal works, more particularly at the lower end of the locks, was so great that it became necessary to stop up a portion of the water passages, and they still continued stopped up. It was found impracticable to work the canal with such a fall. Some years ago an experimental boat, to test the question of speed that might be attained on canals under particular circumstances, was built and sent to him. It had a deck 6 feet 10 inches in width, by 71 feet in length. It was built with the ends contracted from 7 feet from the stem and stern. The plating was of steel, 1½ lbs. per square foot, and the ribs 1 lb. per square foot. There was a rib every 12 inches throughout the boat, and a light longitudinal rib also of steel and of a T section passed along on the top of the keelson. The machinery was guaranteed to attain a speed of 10 miles an hour, and it weighed 16 cwt. The entire boat, inclusive of machinery, weighed 3½ tons. The forward draught was 1 inch, midship draught 4 inches, stern draught 5 inches. The total depth of side was 20 inches throughout. It had a midfeather 25 feet in length, starting from the level of the bottom of the boat, and attaining a maximum depth of 12 inches, along this the propeller shaft was carried. He made some experiments with it. In a canal of about 123 feet cross-section, that boat, only displacing 2½ feet of water in maximum cross-section, could not be made to attain a greater speed than 5.62 miles an hour. He drove her through a tunnel having a cross-section of 90 feet at the rate of 4.8 miles. He found, when he got some distance into the tunnel, that, looking back into the darkness, and judging as nearly as was possible under the circumstances, the boat was followed by a wave caused by the counter-current having a fall from the crest to the bottom of the trough of 2 feet or more. Feeling assured that if this wave overtook the boat it would be inevitably swamped in the darkness, and probably all on board drowned, he was obliged to put the engine to its utmost speed to avoid being sunk in the deep water, and fortunately they emerged into the wider water at the mouth of the tunnel in safety. On coming into the daylight the horror-stricken face of the constructor of the boat presented an appearance he should never forget, nor could he forget his gasping exclamation, "Sir, in that tunnel I have been nearer the confines of eternity than ever I was before in my life."

Arriving at the next bridge he seized his bag, jumped ashore, and left his boat, his experiments, and everything else; he never came again.

The question of attainable speed was limited not only by the form of channel, but also by the form of boat and the relation of the displacement to the sectional area and to the depth of the waterway. The problem to be solved was, not what speed could be attained in a channel of indefinite area and of the form necessary to provide for the passage of the counter-current without delaying the progress of the boat at all, or to an unappreciable extent, but what form of channel was suitable, having regard to the capital outlay necessary for its construction, and to the economic results on an attainable weight of traffic? This, again, necessitated the consideration of the best form of boat, and its suitability to the class of cargo to be carried. As every canal carried various classes of cargo, and the craft plying thereon are required, in many instances, to pass into and across the tideways communicating with the outports, it was evident that the solution of the question by any formula was not practicable. It might, however, be assumed that the best cross-section of channel would be about equal to five times the maximum displacement of the largest craft navigating it, and that the depth should exceed the maximum draught by one-fourth at least, and the bottom width be equal to twice the beam of the widest boat. As to the form of channel, probably a semi-elliptical form was best, at equal cross-sectional areas; and in a channel closely approximating to this form very good results had been attained, but it was so costly in construction and maintenance that it was doubtful if it could be economically adopted.

On the other hand, a trapezoidal section with side slopes varying from 1 to 2 feet horizontally, to 1 foot vertically, according to circumstances, and having a bottom width fully equal to two beams of the largest craft navigating it, could be constructed at a much less cost, and probably the loss of speed would not be very important, especially if the form was somewhat varied by the introduction of dwarf walls on either side to about 3 feet under water-level, as was now being done on many canals having a large traffic. Even with the best attainable channel, not more than an average speed of three miles per hour could be maintained if economy in the cost of transport was studied, and it was self-evident that canals were not, under any circumstances, enabled to compete in speed with railways, and that their proper function was not so to compete, but to carry great weights of raw material and goods not requiring prompt delivery at a very low cost.

Many of the statements in the paper appeared to be wanting in precision, or not to be based on reliable data. For instance, it was stated that the vacuum caused by the motion of the boat "is filled, either by water which follows the movement of the vessel through the canal, or by that which flows as a counter-current, being driven by the head due to the wave caused by the vessel," and that "the first of these two actions is so limited that it may be neglected" in restricted channels. This was so far from being correct that the motion of the following water and its retardation by the irregularities of form of channel, and the presence of weeds and other impediments to its forward motion, formed a self-evident and easily appreciable cause of lessened speed, and the more restricted the channel the more evident this would be.

Again, it was stated, "Thus, for the same boat, in passing from a canal where $A=2a$ into one where $A=3a$, there will be an increase of 33 per cent. in speed in the latter as compared with the former. Suppose the same speed to be maintained in the two cases, the cost of traction will be nearly as 8 to 5."

No data were given on which the accuracy of this statement might be examined; but it was certain that such results were wholly at variance with known facts, and that increase of speed in direct proportion to cross-sectional area was not an invariable rule.

The question of the loss of time due to the differences of level was not clearly stated in the paper. It would be evident that a lock of small dimensions could be more rapidly filled than one of much larger cubical contents, with large locks such as were spoken of on the Aire and Calder, and with such a considerable rise as 13 feet 6 inches, three and a-half minutes was a very short time for filling the

lock chamber. On the Wilts and Berks Canal, with very small locks, 0.434 minute per foot of rise seemed a long and wasteful delay.

There was, however, as already stated, a limit to the rapidity with which a lock could be properly filled and discharged. In the case mentioned on the Warwick Canal, it was found that the bed of the canal was eroded to a considerable depth, and for some distance from the tail bay of the lock, the disturbed material being piled up and forming a troublesome shoal. On the Napton Canal, where the locks were—many of them—in very close proximity, he had adopted a plan of having a larger area of discharging passage at the head of the lock than at the lower gates, to avoid the difficulty spoken of by Mr. Taunton, namely, that consequent on the greater pressure of water on the paddles at the lower gates, a larger volume of water was discharged in the same space of time, causing irregularity of level and waste over the by-wash. The rapidity with which locks on a canal could be worked depended on more than one consideration. Where water was scarce and costly the locks were worked slowly to ensure the avoidance of waste; but in cases where water was plentiful, a far greater speed of discharge might be safely attempted. In practice, it had been found that with a canal bed of fairly solid material, such as clay or gravel, a speed of discharge equal to 1 foot in 0.25 minute might be attained. Where, however, the bed of the canal consisted of quicksand or peat, not more than 1 foot in 0.33 minute was safe, and this had in some cases been found too rapid. With respect to the experimental boat mentioned, it was an honest, although impractical endeavor to solve the question now under consideration. The originator of the idea who bore the whole cost, stated that "he desired to build a boat of exceedingly great buoyancy, which should, if possible, successfully demonstrate the possibility of attaining a very high speed on narrow canals." It was pointed out that this supposed buoyancy was an error, and that any extraordinary lightness of construction only gave additional cargo-carrying capacity at any given draught, and could in no degree govern the speed attainable. What the experiments did satisfactorily prove was, 1st. That the volume of swell and consequent counter-current was in a great measure due to the form of boat and its relation to the sectional area and not to the area of displacement; 2d. That the attainment of a running speed of 5.76 miles per hour on an ordinary English canal, as proposed in the paper, was impossible.

Sir Charles Hartley remarked that the author recommended that a series of experiments should be made to determine the form of cross-section best suited for canal navigation. It seemed to him that however useful the results of such experiments might prove to engineers in designing canals through very hard or rocky ground, they would be of little service in determining the best kind of profile for ship-canals through soft or sandy ground, where the adoption of flat side-slopes would be a necessity. In designing cross-sections for such important waterways, the chief consideration would continue to be as hitherto, the nature of the material through which the canal was to be passed, and how best to provide the greatest amount of accommodation for ocean steamers at the smallest cost. So far as he was aware, the floors of all canals had as yet been cut down to a perfectly level surface, with the view of giving the same draught of water over the whole width of the floor. In course of time, however, owing to slips in the banks in places where the upper parts of the slopes were not protected against wave-action, the cross-sections gradually assumed a rounded outline, and in this way the channel went on deteriorating at a rapid pace unless maintained at its original depth by dredging. Now, even supposing the form of as emiellipse, represented the best kind of profile theoretically, taking into account the relation between the velocity and the resistance encountered by ships in confined channels, it would be an impossible section in soft ground, unless the sides were protected by walls of masonry founded, in the absence of benchings, at a great depth below the surface of the water, an expedient which he deemed to be inadmissible on account of the great expense which would attend its application. In French canals, the width and depth of the navigable channel were regulated by

the amount of traffic anticipated, and the class of vessels to be accommodated, and the slopes, as a rule, were $1\frac{1}{2}$ to 1, varying, of course, where the nature of the ground rendered a flatter slope advisable. A similar profile, according to a recent number of the *Scientific American* was also being adopted in soft ground at the Isthmus of Panama near Aspinwall, where an American firm had contracted to dredge through 10 miles of what was called marsh-work, to a cross-section 100 feet wide at the bottom, 180 feet at the top, $27\frac{1}{2}$ feet deep and with slopes at $1\frac{1}{2}$ to 1. Another instance of modern French practice in ship-canal making was the St. Louis Canal, which was opened in 1872, and which he had two opportunities of visiting when under construction. This canal was 2 miles long, and flanked the mouth of the Rhone. It was cut through soft silt and fine sand, containing every here and there beds of hard clay. The cutting was 33 meters wide at the bottom, 63 meters at the top, and 6 meters deep over the whole width of the floor. The slopes had an inclination of 2 to 1 from the bottom up to 2 meters below low water, at which level there were benchings on either side $6\frac{1}{2}$ meters in width. From these benchings the slopes were protected with smooth-faced stone walling in hydraulic lime. This revetment was carried up at an inclination of 45° to the level of 4 feet above the water line. It would be seen from this description that the cross-section of the St. Louis Canal closely resembled that of the Suez Canal. If, indeed, the lower slopes of 2 to 1 were extended to the depth of 8 meters, one would have a width of 22 meters or 72 feet, at the bottom, the actual width of the floor of the Suez Canal.

With regard to speed in ship canals it was stated in the paper that the speed of vessels in the Suez Canal had slackened from 5.88 miles per hour in 1876, to 5.27 miles per hour in 1882. Now the maximum rate of speed allowed by the Canal Company was 10 kilometers or 5.4 knots per hour, and this, as the author observed, had been regulated rather with reference to the damage caused to the side slopes by the waves produced than by other considerations. The average speed through the canal proper was really less than the author had indicated; for throughout the Bitter Lakes, in the open water from one lighthouse to the other, a distance of 8 nautical miles, vessels were free to steam as they pleased. With the view of protecting the banks from injurious erosion, caused by the high speed of passing steamers, the upper parts of the slopes of the canal, down to depths varying from 3 feet to 9 feet below the water line, were being gradually protected with smooth-faced masonry in hydraulic mortar, laid at inclinations of from 2 to 1 to 5 to 1, according to the nature of the ground. With regard to the average time occupied in actual movement through the canal, the author stated that the actual time passed in the canal by each vessel had risen from thirty-nine hours in 1876, to fifty-three hours thirty-six minutes in 1882. On reference to the latest statistics published by the Canal Company, he found that this statement was practically correct; but it should at the same time be explained that, although the average time occupied by actual movement in the canal was precisely thirty-nine hours thirty-two minutes in 1876, it had only risen to forty-one hours in 1880, and that the very marked slackening in speed in 1881 and 1882 was principally due to the establishment of quarantine from the month of September, 1881, to August, 1882. In 1883 an improvement took place, for the average throughout the past year fell to forty-eight hours forty minutes, notwithstanding that in March, owing to three storms of unusual violence, the average for that month rose to seventy hours ten minutes, and this of course affected the average for the year. With reference to the cross-sections of the Suez Canal, it should be observed that there was hardly any fixed section at present, and that the width at the water-line varied according to the ground traversed by the canal. Thus, although the navigable channel was uniformly 72 feet wide and about 26 feet deep, the width at the water-line varied from 200 feet in the long cuttings of El Guisr and Serapeum to 328 feet between Port Said and El Guisr, and throughout the Suez Plain.

The author mentioned incidentally that the cross-section of the Suez Canal was only about one-half of the area originally

intended. This statement was substantially accurate, and the circumstance was so important as to merit some more special notice on that occasion. In the "Avant projet" of 1856, and in the Report in the month of December of that year of the Suez Canal International Commission, on the roll of which appeared the names of Mr. Charles Manby, Honorary Secretary Inst. C. E., and the late Mr. J. R. McClean, Past-President Inst. C. E., it was stipulated that the canal should be wide enough, not only to allow two vessels to pass each other, but to give place to a third line of vessels which might, from whatever cause, stop on the way. It was therefore recommended, and provided for in the first estimate of £10,000,000 for the whole work, that the canal between the Red Sea and the Bitter Lakes should have a width of 64 meters, or 210 feet, at the bottom; and of 100 meters, or 328 feet at the top; and that the channel from the Bitter Lakes to the Mediterranean should be cut to a width of 44 meters, or 144 feet, at the bottom; and 80 meters, or 262 feet, at the water-line. In addition to this liberal allowance of width, provision was also made for sidings, excavated at equal distances along the line of the canal as at present. The construction of the canal had made but little progress when it was found that its cost had been altogether under-estimated, and consequently the width of the cross-sections was made to tally with the amount of money available, thereby enabling the skillful and plucky "*Fondateur*," M. de Lesseps, to announce to the world in the month of November, 1869, that his great enterprise was practically a *fait accompli*. The narrowing of the navigable channel by one-half compelled the adoption of the single line, or block system, of passing vessels from sea to sea, and it should not be forgotten, in lamenting over the consequences of an insufficient area of cross-section, that by the slow but safe mode of transit that had been unavoidably adopted up to this time, not only had serious collisions between ships in motion been avoided, but that where the currents were strong—and they frequently attained a velocity of 2½ knots an hour between Suez and the Bitter Lakes—the re-floating of vessels, which so often got aground by defective steering, and were driven athwart the channel by the wind or currents, was a much easier operation than if the canal had been cut to the width at first intended. As this was not the place to discuss the merits or demerits of the plans proposed for widening the existing canal, or for the construction of a second one across the Isthmus of Suez, he would only observe, in conclusion, and with regard to canals generally, that according to his long experience in the navigation of confined waterways, the acquirement of extra depth was of more importance even than that of additional width, where greater speed was required; for the reason that when the keel of a vessel was near the bottom, or, as sailors said, she began "to smell the ground," her speed, irrespective of other causes, was very considerably diminished, and her steering qualities became so much impaired as to make her very difficult to handle, even when she had her head to the current and her progress was not baffled by the wind.

MODERN BRONZE ALLOYS FOR ENGINEERING PURPOSES.*

By PERRY F. NURSEY, C. E.

From "Iron."

In order to mark the progressive steps of man, his early history has been divided into three periods, each being named after the materials chiefly used in them for supplying him with weapons, tools, and ornaments. Thus we have the age of stone, the age of bronze, and the age of iron. To these our own times have added the age of steel, which, of course, simply means a further, but most important, development of the age of iron. It occurs to the author that in like manner a similar development is going on in the present day with respect to bronze. As, however, no generic term has been invented in these very inventive times for

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the new variations of that most ancient and useful alloy, he is unable to designate this fifth age by a new title, and so is content to consider it as a revival of the bronze age in a more advanced and more highly developed form. As, however, the age of stone was divided into two parts, namely, the old stone age, when men simply chipped stones, and the newer stone age, when they learned to grind and polish them, so perhaps it may be admissible to consider the present as the newer bronze age. Nor is it altogether inapt so to consider it when we see what a many varieties of bronze have been produced within the last few years possessing very distinctive features from the ancient alloys, and some very remarkable qualities as compared with them, and how very numerous the purposes are to which these varieties are applied, superseding, as they do in many instances, iron, and even steel itself. These considerations, and the knowledge of the great value of these alloys to the engineer and the shipbuilder, have led the author to bring a few facts concerning them before the society in the hope that they may prove interesting and possibly useful to the members.

Although, for the purpose of drawing a fanciful parallel between the past and the present, the author has taken his hearers back to the childhood of the world, when men first mixed and melted copper and tin together, and fashioned in a rude way from the resulting alloy their spear-heads, their hatchets, their ear-rings, and their bracelets, he does not intend to fill up the gap formed by the intervening centuries by following up the gradations by which this alloy was made at length to subserve the highest purposes of art, which, as regards statuary, probably reached a climax during the reign of Alexander the Great. He purposes only to touch upon a few points of metallurgical interest relating to the bronze of the past, and then by a rapid transition to come down to the present, and to enter upon a consideration of the various alloys of the bronze family which the ingenuity of our own day—stimulated, doubtless, by the necessities of the age we live in—has succeeded in rendering available for engineering purposes during the last few years.

Turning then, briefly, to the bronzes of the ancients, we find them varying considerably in the proportions of their two ingredients—for in the main copper and tin only were used—according to the purposes for which they were intended. Thus, modern chemical analysis shows that ancient bronze nails contained 20 of copper to 1 of tin; soft bronze consisted of 9 to 1, medium bronze 8 to 1, hard bronze 7 to 1, and mirrors about 2 to 1. The bronze weapons and tools of the ancients contained from 8 to 15 per cent. of tin. A Roman sword blade found in the Thames showed 85.70 copper and 10.02 tin, whilst another one found in Ireland gave on analysis—copper 91.39 and tin 8.38. The bronze weapons of the Greeks and Romans have been found not only to be of the true composition for insuring the greatest density in the alloy itself, but the cutting edges, by undergoing a process of hammering, were brought up to the highest degree of hardness and tenacity. And here the author would digress for one moment to observe that this is exactly the way in which he has seen the edges put on scythe blades and other cutting tools in Styria, where he has seen some of the finest steel made in the simplest and rudest manner. Returning to the subject of the paper, it is to be observed that most of the ancient coins were of bronze, a small percentage of zinc being added in some cases to improve the color. According to analyses made by Mr. J. A. Phillips, the quantity of tin relatively to copper varied very slightly, even over a range of 300 years. The following are the proportions of copper and tin, the other ingredients being omitted:

Coins.	B.C.	Copper.	Tin.
Alexander the Great	335	86.72	13 14
Philippus V	200	85.15	11.10
Athens	—	88.41	9.95
Ptolemy IX	70	84.21	15 59
Pompey	53	74.11	8 56
The Atilia family	45	68.72	4 77
Augustus & Agrippa	30	78.58	12.91

Bronze, pure and simple, consists of a mixture of copper and tin in certain proportions. These proportions, as we have seen, are varied according to the purpose for which the compound is intended. Other metals, moreover, such as zinc, lead, phosphorus, manganese, silicium, and iron, may be, and have been, added without unclassifying the product,

which is still called bronze, provided that copper and tin are the chief contituents. The bronzes of France are known to contain nearly always four metals, namely, copper, tin, lead and zinc. It is also stated that some contain minute and variable quantities of nickel, arsenic, antimony and sulphur. It is the addition to bronze, pure and simple, of certain proportions of one or other of the metallic substances previously referred to that constitutes the modern development of bronze manufacture, and which has given us some of the most useful and at the same time some of the most remarkable alloys known. These comprise no fewer than eleven distinct products, all of which find their uses in connection with the practice of engineering. These are—phosphor bronze, silicium bronze, manganese bronze, delta metal, phosphor copper, phosphor-manganese bronze, phosphor-lead bronze, phosphor tin, aluminium bronze, silveroid, and cobalt bronze. These alloys form the subject for present consideration, and they will be dealt with by the author in the foregoing order. There are other bronzes which are used as substitutes for gold in cheap imitation jewelry; but although they are, in the main, only variations of some of the bronzes with which the author has to deal, their applications are such that their notice does not fall within the scope of the present paper.

Attention was directed some years since to the use of phosphorus in improving the character of bronze for various purposes, and eventually with very successful results. The action of phosphorus on copper alloys is principally due to its reducing qualities, by virtue of which the oxygen absorbed by the molten metal is removed, or rather the oxides thereby produced are eliminated, and there is imparted to the metal that degree of homogeneity, strength and toughness which are peculiar to the chemically pure metal. The phosphorus, by producing these effects is converted into cuprous oxide, which floats on the surface of the molten metal in the shape of a very fluid slag, whilst the superfluous quantity combines with the metal. This being the case, it is not desirable to add to the bronze a larger quantity of phosphorus than will suffice to reduce the oxide present. It is thought by some that the phosphorus itself imparts to the bronze the qualities of hardness and strength, and that, therefore, the more phosphorus put into the metal the better the result as regards hardness. This, however, is not the case, inasmuch as hardness would be obtained at the expense of toughness. The question of producing the various qualities of this class of metal depends not so much upon the quantity of phosphorus as upon the correct proportioning of the various ingredients, phosphorus included. Some of the alloys to which the author will direct attention are formed by the addition of a small proportion of a compound of phosphorus and copper or other metal to the bulk of the copper to be treated. Inasmuch however, as great care is required in determining the exact proportions of the ingredients in making phosphor-bronze alloys, it appears to the author that it would be much safer, and probably much more economical for manufacturing engineers to obtain the alloys ready prepared for the special purpose for which they require them, and which would, other things being equal, obviate all chance of failure by reason of a careless workman adding too little or too much of the phosphorized metal to the bulk.

Phosphor Bronze.

The first of the modern bronzes for notice in the order of time is phosphor bronze, which was invented by Dr. Kunzel, of Blasewitz, Dresden, and was brought into practical use in this country early in 1873 by the Phosphor-Bronze Company, who have from time to time patented several improvements both as regards alloys and methods of manufacture. Phosphor-bronze alloys are composed of copper, tin, phosphorus, and other ingredients in definite proportions, and are made to be either as ductile as copper, as tough as iron, or as hard as steel, accordingly as the proportions of the constituents are varied. The alloys used for rolling and drawing have very different proportions to those employed for castings, for bearings, and parts of machinery. The castings of the metal, owing to its great fluidity when melted, are perfectly sound and homogeneous. Wherever strength, toughness, and durability are desiderata, phosphor bronze is

found to be far better adapted than gun metal and brass, and in many cases than iron and steel. With regard to the applications of phosphor bronze, it may truly be said that their name is legion. This remark applies in the main to most of the modern bronze alloys presently to be described, so that, in order to save repetition, the author will here observe that chief amongst their many applications are the manufacture of wire, rods, tubes, sheets, ornamental castings, screw propellers, pinions, cylinders, valves, bearings, bushes, and other parts of machinery exposed to friction. Phosphor bronze possesses the advantage of not becoming crystalline under the action of repeated shocks and bends, and is therefore well adapted for making wire rope; and as it is not acted on by corrosive liquids, as found in mines, or by the atmosphere, its value as a metal remains constant. It is being used in the shape of sheets for the hulls of torpedo boats and steam launches with satisfactory results. In order to ascertain its resistance to the chemical action of dilute sulphuric acid, two similar sheets of copper and of phosphor bronze were immersed in acid water 10° Beaume strength, and at the temperature of the surrounding atmosphere; after three months it was found that the copper had lost 4.15 per cent., and the phosphor bronze only 2.3 per cent. Phosphor bronze sheet, moreover, stands the action of sea-water much better than copper. In a comparative experiment made at Blackenberghe, lasting over a period of six months, between the best English copper and phosphor bronze, the following results were arrived at. The loss in weight due to the oxidizing action of sea-water averaged for the copper 3.058 per cent., while that of phosphor bronze was but 1.158 per cent.

In making castings from phosphor-bronze alloys, a new or clean plumbago crucible is used, so as to avoid any admixture of other metals, and some charcoal or coke is kept on the metal during the melting to prevent oxidation. For large castings the moulds are thoroughly dried and dressed with a mixture of black-lead and water. Small work is cast green. In order to avoid segregation, it is necessary to pour phosphor-bronze alloys only just before the setting takes place. This is accomplished by cooling the molten metal by putting in ingots or runners, and when the metal no longer melts these, but adheres to them it is a sign that the pouring should take place. Previously to pouring, the molten mass is well stirred by means of an iron rod covered with a paste of either fireclay or plumbago. Besides the original phosphor bronze with which the author has dealt so far, the Phosphor-Bronze Company a year or two ago brought out two other varieties. These were the outcome of an endeavor on the part of the company to meet as far as practicable the various requirements of engineers and millwrights, particularly in connection with parts of machinery exposed to combined friction and pressure. By slightly changing the proportions of the component parts of some of the ordinary mixtures, new alloys having very valuable and distinct characteristics have been produced, and which have been practically tried and proved. These new alloys are known as phosphor-bronze duro A, and phosphor-bronze duro B. Duro A is a very dense metal, adapted for all bearings carrying heavy wheels running at great velocities, and generally for all quick-speed purposes. Duro B is intended for the bearings of hot-neck rolls, and for all bearings having to withstand great pressure, such as plate and sheet roll bearings, and for general engine purposes.

Having referred to the great durability of phosphor bronze under conditions of work, the author will here notice one instance out of several which have been brought under his notice from time to time. About two years since he inspected a pair of slide valves, which had been then recently taken out of one of the North-Eastern Railway Company's express engines after six and a-half years' working, during which the engine had run 261,182 miles between Newcastle and Edinburgh. They were taken out to replace the cylinders with a pair of a different type.

The engine was of the following dimensions: Cylinders, 17 inches diameter by 24 inches stroke; four coupled wheels, 7 feet diameter; working pressure 140 lbs. per square inch; weight of engine in working trim, 39 tons 16 cwt.; weight of tender, 26 tons 4 cwt.

Mr. Fletcher, the assistant locomotive superintendent of the North-Eastern Railway, states that the slides, in the six and a-half years, had only worn down to the thickness at which they generally took out gun-metal slides, and that, had it not been that they were putting in a pair of cylinders of different type, he would certainly have let them run longer, as he considered them quite safe, taking into consideration the great superiority of phosphor bronze over gun metal. The original thickness of these slides was 1 inch, and they were worn down to ⅝ inch thick. Gun-metal slides rarely exceed eight months' work when they are worn out. The cylinder faces were in excellent condition, the wearing being, as it should be, on the valves.

Tables I. and II. give the results of some comparative experiments made by Mr. David Kirkaldy with phosphor bronze and various other alloys and metals, whilst Table III. gives the results of tests made with axle bearings of various metals.

Silicium Bronze.

We come, in the next place, to silicium bronze, which, in some respects, may be considered as an outcome of phosphor bronze, although its invention is not due to Dr. Kunzel, who died some years ago. The inventor is M. Lazare Weiller, of Angouleme, who exhibited phosphor-bronze telegraphic and telephonic conductors at the Paris Electrical Exhibition of 1881, where the author first saw them, and where they were novelties. M. Weiller carried out an exhaustive series of experiments with this wire, the results of which went to show that it possessed a conductivity one-third that of copper, but 2½ times that of iron and steel. Phosphor-bronze wires, therefore, proved very useful for telephonic communication, but not for telegraphic purposes, where higher conductivity is required. M. Weiller, therefore, set himself the task of discovering a material analogous to phosphor bronze, and his labors were at length crowned with success by the discovery of silicium bronze. In this alloy the phosphorus is replaced by a silicious metalloid which produces a better conductor than does phosphorus. M. Weiller thus obtains a wire presenting the same resistance to rupture as phosphor-bronze wire, but with a much higher degree of conduc-

Table I.—Results of Experiments to Ascertain the Elastic and Ultimate Tensile Strength of Twelve Pieces of Phosphor-Bronze, Six Pieces of Ordinary Bronze, and One Piece of Brass.

Description.	Stress.		Ratio of Elastic to Ultimate.	Contraction of Area at Fracture.	Extension Sets at	
	Elastic per Square Inch.	Ultimate per Square Inch.			Lbs. 20,000 per Sq. Inch.	Ultimate.
	Lbs.	Lbs.	Per cent.	Per cent.	Per cent.	Per cent.
Phosphor-bronze..	23,800	40,876	58.2	3.9	0.40	3.8
" " ..	21,900	49,865	43.9	9.8	0.09	10.6
" " ..	19,800	33,623	58.9	5.9	0.18	4.6
" " ..	15,400	24,193	63.6	7.9	1.94	6.7
" " ..	14,900	21,755	68.5	5.9	2.26	4.3
" " ..	13,700	25,715	53.3	9.8	3.12	10.9
" " ..	21,400	35,024	61.1	5.0	0.11	3.6
" " ..	19,200	43,082	44.6	15.1	0.26	14.1
" " ..	16,100	44,448	36.2	31.9	0.98	33.4
" " ..	13,400	36,044	37.2	19.9	1.52	16.5
" " ..	11,800	24,988	47.2	13.4	5.13	11.1
" " ..	10,700	34,024	31.5	23.0	3.06	17.4
Ordinary " ..	19,700	29,534	66.7	8.4	0.18	4.0
" " ..	19,600	27,376	71 6	5.0	0.18	2.6
" " ..	17,800	22,592	78.7	5.0	0.72	2.3
" " ..	17,700	21,946	80.7	3.3	0 50	1.6
" " ..	17,600	20,482	85.9	1.5	0.68	1.2
" " ..	16,400	20,296	80.8	3.3	1.10	1.7
Brass............	10,100	27,518	36.7	18.3	5.80	16.1

LE II.—RESULTS OF EXPERIMENTS TO ASCERTAIN THE TENSILE STRENGTH AND THE RESISTANCE TO TORSION OF VARIOUS WIRES.

Description.	Pulling Stress. Wires as Drawn.				Twists in 5 Inches.	
	Diameter.	Area.	Stress.		As Drawn	Annealed.
			Total.	Per Sq. In.	Mean of 3.	Mean of 3.
	Inch.	Sq. Inch.	Lbs.	Lbs.	Twists.	Twists.
Phosphor bronze	0.0655	0.003367	340	100,980	5.0	91
" "	0.0640	0.003216	389	120,957	22.3	52
" "	0.0600	0.002827	352	124,313	7.0	87
" "	0.0610	0.002922	379	129,705	8.3	98
" "	9.0595	0.002778	336	120,950	13.0	124
" "	0.0585	0.002655	395	147,113	7.5	97
" "	0.0640	0.003216	513	159,515	13.3	66
Copper	0.0640	0.003216	203	63,122	86.7	96
Brass	0.0605	0.002871	233	81,156	14.7	57
Steel (ordinary)	0.0600	0.002827	342	120,976	22.4	79
Iron, galvanized, best best C	0.0580	0.002643	170	64,321	26.0	44
" " best charcoal E.	0.0580	0.002643	174	65,834	48.0	87

TABLE III.—RESULTS OBTAINED WITH VARIOUS AXLE BEARINGS.

Kind of Bearing.	Composition in 100 Parts Alloy.	Comparative Cost of 100 Kilos. Bearings inclusive of Melting Expenses, Loss, &c.	1 Kilo. Bearing Metal Runs			Cost of Bearing Metal per Wagon with 4 Bearings per 100 Kilometers.	Name of Railroad where used.
			German Miles.	Kilometers.	Wear per 100 Kilos. for 4 Bearings.		
		Marks.			Grammes	s. gr.	
Gun metal....	83 copper, 17 tin.	260.20	12,052	90,390	11.60	0.301	Austrian Railway
"	82 " 18 "	260.80	13,320	99,900	10.01	0.260	Gr. Cent. Belge.
White metal..	3 copper, 90 tin, 7 antimony..	298.68	9,104	78,280	14.64	0.395	Austrian Railway
" ..	5 copper, 85 tin, 10 antimony..	293.40	11,750	88,145	11.35	0.331	Niederschlesisch-MarkischeBahn.
Lead composition........	84 lead, 16 antimony.......	118.56	10,338	81,280	12.30	0.145	Austrian Railway
Phosp'r bronze	—	360	57,226	429,200	2.33	0.081	Gr. Cent'l Belge.
Gun metal on brake cars..	82 copper. 18 tin.	260.80	1,218	9,134	109.48	2.844	" "
Phosp'r bronze on brake cars	—	350	14,320	107,410	9.31	0.325	" "

tivity, rendering it applicable for telegraph lines, and bringing the valuable qualities of lightness and non-oxidizability within easy and economical reach. In a paper upon electrical conductors, read by Mr. W. H. Preece, F. R. S., before the Institution of Civil Engineers, in December, 1883, that gentleman observed, in reference to phosphor and silicium bronze wires, that in their manufacture phosphorus and silicium had the property of removing impurities, particularly the oxides, though doubtless some of the flux remained. Phosphorus had a most injurious influence on the electrical resistance of the alloy. Silicium was far superior; hence the silicium bronze was preferable for telegraphic purposes.

Its efficiency was very great: in fact, phosphor bronze had disappeared for telegraph wire, and had been replaced by silicium bronze. It is important to note that the properties of this alloy are such that, although the wires are only one-tenth as heavy as the ordinary wires, they are of equal strength. Moreover, it is affirmed that, if broken, they will not fall to the ground as the ordinary wires do, but by reason of their high elasticity, they will spring back and coil up close to the standards. The author should mention that M. Weiller has associated himself with the Phosphor Bronze Company, who are manufacturing silicium-bronze wire, which, the author is informed, has come largely into use for overhead telephone lines, and proves a satisfactory substitute for the cumbrous iron wire. Table IV. shows the relative strength, resistance, and conductivity of various wires.

TABLE IV.

Description of Wire.	Tensile Strength per Square Inch in Tons.	Resistance per Mile in Ohms.	Relative Conductivity.
Pure copper	17.78	33.2	100
Silicium bronze (telegraph)	28.57	34.5	96
Silicium bronze (telephone)	48.25	103	34
Phosphor bronze (telephone)	45.71	124	26
Swedish galvanized iron	22.86	216	16
Galvanized Bessemer steel	25.40	249	13
Siemens-Martin steel	26.67	266	12

MANGANESE BRONZE.

The author now comes to that class of bronzes into the composition of which iron in one form or other enters, and of which there are two principal varieties, namely, manganese bronze and delta metal. It is stated that some of the ancient bronzes have been found on analysis to contain a small percentage of iron, but it does not appear that any traces of manganese have ever been discovered. It is thought probable that the ancients knew that the addition of iron to bronze would increase its hardness, and introduced it for that purpose. Modern inventors have proposed combinations of iron with brass alloy, and some have also introduced manganese by reducing the black oxide of manganese and combining it with the copper. Until a comparatively recent date, however, none of these alloys appears to have been brought into permanent practical use. More than a hundred years since James Kier proposed an alloy of 10 parts of iron with 100 parts of copper and 75 parts of zinc, and in later times Sir John Anderson, when superintendent of Royal Gun Factories, carried out a number of experiments with similar alloys, and with some very good results, but none of them appear to have been brought into practical use. The addition of iron unquestionably increases the strength and hardness of these alloys, but, according to some experiments made by Mr. P. M. Parsons, they would appear to acquire these qualities at the expense of ductility and toughness, and it is probably on this account that this class of alloys had not come into general use up to the time of Mr. Parsons' experiments. Mr. Alexander Parkes and the late Mr. J. D. Morries Stirling, both eminent metallurgists, appear to have been the first to propose and carry into practice the use of manganese. Mr. Parkes combined manganese alone with copper, and used this alloy to form improved alloys of brass and yellow metal, of which to make sheathing, rods, wire, nails, and tubes.

Mr. Stirling in 1848 proposed to use manganese in various brass alloys in which iron was present. At first he combined about 7 per cent. of iron with the zinc, and added to the copper a small percentage of manganese, by reducing the black oxide of manganese with the copper, in the presence of carbonaceous materials, and then added to it the requisite quantity of the iron and zinc alloy to make the improved brass required. Mr. Stirling's idea was to combine the iron with the zinc by fusion, but in practice he found a more ready means of procuring

the zinc and iron alloy by employing the deposit found at the bottom of the tanks for containing the melted zinc for galvanizing iron articles. This product consists of zinc with from 4 to 6 per cent. of iron, but this percentage is very variable, and the results of its use, therefore, in some cases unreliable. The author is informed that metal made by this process was in use for sometime for carriage bearings, on the London and North-Western and other railways, with very good results; but it has long since been superseded, and does not appear to have ever been introduced for any purposes where the requirements were great strength, hardness, and ductility.

The time, however, arrived, namely, in 1876, when these requirements were met by the aid of manganese in the manganese bronze of Mr. P. M. Parsons. This alloy is prepared by mixing a small proportion of ferro-manganese with copper, and which is afterwards made into alloys similar to gun metal, bronze, brass, or any other alloy of which copper forms the base. The ferro-manganese is melted in a separate crucible, and is added to the copper when in a melted state. The effect of this combination is similar to that produced by the addition of ferro-manganese to the decarburized iron in a Bessemer converter. The manganese in a metallic state, having a great affinity for oxygen, cleanses the copper of any oxides it may contain, by combining with them and rising to the surface, in the form of slag, which renders the metal dense and homogeneous, as already explained by the author in respect of phosphorus. According to Mr. Parsons, a portion of the manganese is utilized in this manner, and the remainder, with the iron, becomes permanently combined with the copper, and plays an important part in improving and modifying the quality of the bronze and brass alloys, afterwards prepared from the copper thus treated. The effect is greatly to increase their strength, hardness, and toughness, the degrees of all of which can be modified, according to the quantity of the ferro-manganese used, and the proportions of the iron and manganese it contains.

It will thus be seen that Mr. Parsons' method of making manganese bronze is ltogether different, both in principle and ffect, from the inventions of either Parkes or Stirling. Stirling's method of combining the iron with the zinc, in order to introduce it into the alloys, precludes its use in any but those alloys in which a considerable portion of zinc is employed, such as brass or yellow metal. It could not be applied to any of those important alloys, of the nature of gun metal or bronze, in which copper and tin are the chief ingredients. An equally important difference in the manufacture of manganese bronze consists in adding the manganese in its metallic state, in the form of ferro-manganese, to the copper, by which the copper is cleansed from oxides, which it cannot be when the manganese is reduced from the black oxide and combined with the copper by one and the same operation, as was done by Parkes and Stirling. Another point of importance is the great nicety with which both the iron and manganese can be adjusted, and their effect controlled by adding the ferro-manganese to the copper, as pursued in the manufacture of manganese bronze. The amount of manganese required for deoxidizing the copper, and for permanent combination with it, being well known by experience, it is found that very slight variations in quantity have a perceptible and ascertained effect in modifying the qualities of the alloys produced. The toughness can be increased, and the hardness diminished, or vice versa, at will, precisely as is done in the manufacture of steel, by increasing or diminishing the percentage of carbon and manganese. In preparing the ferro-manganese for use, Mr. Parsons prefers that which is rich in manganese, containing from 50 to 60 per cent. This is melted with a certain proportion of the best wrought-iron scrap, so as to bring down the manganese to the various proportions required. At the same time any silicon it contains is reduced and the metal refined. About four qualities of ferro-manganese are made in practice, containing from about 10 to 40 per cent. of metallic manganese. The lower qualities are used for those copper alloys in which the zinc exceeds the tin, and the higher qualities for those in which tin is used alone, or exceeds the zinc used in combination; and the amount of ferro-manganese added varies generally from about 2 to 4 per cent. After a number of careful experiments and crucial tests, the Manganese Bronze and Brass Company, who are the

sole manufacturers of manganese bronze, adopted the manufacture of five different qualities of this alloy, although other varieties can be produced for special purposes. In No. 1 quality the zinc alloyed with the copper is considerably in excess of the tin. It is cast into ingots in metal moulds, and then forged, rolled, or worked hot, and made into rods, plates, sheets, sheathing, and it may also be worked cold, and drawn into tubes and wire. When simply cast, it is stated to possess a tensile strength of about 24 tons per square inch, with an elastic limit of from 14 to 15 tons. When rolled into rods or plates, it has a tensile strength of from 28 to 32 tons, with a limit of 15 to 23 tons per square inch, and it stretches from 20 to 45 per cent. of its length before breaking. When rolled cold, the elastic limit rises to over 30 tons, and the breaking strength to about 40 tons, and it still elongates about 12 per cent. before breaking.

Manganese bronze No. 2 is similar to No. 1, but stronger, and it can be cast in sand for special purposes where strength, hardness, and toughness are required. But it must be melted in crucibles, as passing it through the reverberatory furnace injures the metal, and causes unsound castings. It is not, therefore, adapted for general brassfounders' purposes. One of the most important applications of this quality is that of producing articles cast in metal moulds under pressure. Blocks of this metal thus simply cast are said to have all the characteristics of forged steel as regards strength, toughness, and hardness, without any of its defects. It is perfectly homogeneous, and while not possessing a fibrous texture derived from rolling or hammering, it is still fibrous in character, and this in not one but in all directions alike, and, when broken, shows a beautiful silky fracture. Its tensile strength is from 32 tons to 35 tons per square inch, and its elastic limit from 16 to 22 tons, with an ultimate elongation of from 12 to 22 per cent.

Another feature of No. 2 quality is that it can be cast on to any object, and will shrink on to it with a force equal to its elastic limit, and when released, will show an amount of resilience of about double that of steel. As an instance of this, the author is informed that a hoop of this manganese bronze, shrunk on to a solid cylinder of iron, stretched, when hot, 0.03 of its diameter in the process of contraction, and, when cold and released, sprang back about 0.003 of its diameter. As regards hardness, it is about equal to mild steel.

In order to ascertain its efficiency in this respect, and to compare it with gun metal, wrought iron, and steel, a knife-edged angular die was forced into the flat surface of a piece of each of these metals, and of the No. 2 manganese bronze cast under pressure. In order to produce an indentation of equal depth in each of these specimens, the following pressures are required:

	Cwt.
Gun metal	12
Wrought iron	15
Mild steel	20
" "	25
Manganese bronze as cast	20
" " hardened by pressure	22–23

These results point to this material as the most suitable for the construction of hydraulic and other cylinders required to stand great strains and possibly for ordnance. No. 3 quality is composed principally of copper and tin in about the same proportions as gun metal, combined with a large percentage of ferro-manganese. Its chief characteristics are great transverse strength, toughness, and hardness, the facility with which it can be cast, and the soundness and uniformity of the castings produced. It will stand melting in an ordinary reverberatory furnace without injury to the metal, which is a point of importance in producing large castings. The author is informed that a bar of this metal cast in sand in the ordinary way, 1 inch square, placed on supports 12 inches apart, requires upwards of 4,200 lbs. to break it, and before breaking it will bend to a right angle, and it will sustain from 1,700 to 1,800 lbs. before taking a permanent set. This quality of manganese bronze is used for wheel gearing, supports and connections of machines, crank pin brasses, the shells of main and other bearings of marine and other engines, axle boxes, and other parts of locomotive engines. It is also adapted for statuary and art purposes generally, as well as for large bells.

The most important application of this quality of manganese bronze from a com-

mercial point of view would appear to be that of screw propellers. Owing to the great strength of this quality of the metal, and its non-liability to corrosion, propeller blades can be made thinner than even those of steel. Their surfaces are very smooth, and, when cast, they are said to be theoretically true to form, whereas, with steel propellers, allowance has to be made against the corrosion which takes place, and their deficiency in toughness, by increasing their thickness, and their form is liable to become distorted in the annealing oven they have to pass through after being cast. The author is informed that it has been proved conclusively by the logs of steamships which have had their steel propellers replaced by manganese-bronze blades that their speed has been increased, and the consumption of coal diminished, whilst the vibration and strain on the ship and machinery have been reduced.

The commercial bearing of the question alluded to by the author is, that these advantages are secured at a considerably less ultimate cost than by the use of steel, taking it upon the average life of a vessel; for although the first cost of a propeller with manganese bronze blades is double that of steel, it is said to be practically indestructible, whereas, at the end of about three years steel blades become so corroded that they have to be renewed, which brings up the total cost of the steel blades on an average to two or three times those of manganese bronze. That these propellers are incorrodible, and in every other respect efficient, is said to be proved by experience, as some have been at work for nearly four years, and are as perfect as when first applied.

The soundness and tenacity of the No. 3 quality of manganese bronze thus applied was demonstrated by an accident which occured to the propeller of the *Garth Castle*, on its launch from the yard of Messrs. John Elder & Co., in 1880. One of the blades came in contact with the jetty, and was bent round without a crack to nearly a right angle, and was afterwards hammered back cold to its original form without detriment. Another example is afforded by the propeller of the North German Lloyd's steamship *Mosel*, which was wrecked. One of the propeller blades after it was recovered from the wreck, was found to be completely doubled over, thus affording an idea of the tenacity of the metal.

With regard to the Nos. 4 and 5 qualities of manganese bronze, the author has only to observe that they have no particular claim to strength, but are effectively used for bearings, slide valves, slide blocks, piston rings, and, in fact, wherever friction occurs.

Table V. shows the results of some tensile tests applied to eleven specimens of manganese bronze. The tests in the first series were made at the Royal Gun Factories, and at University College, London, respectively. Those in the second series were made at the Royal Gun Factories, whilst those in the third series were made by the Manganese-Bronze Company. The specimens tested at the Royal Gun Factories were 2 inches in length, whilst those tested at University College, and by the company, were 8 inches in length. It is to be observed with reference to the last two samples that No. 10 was cut from the side of the ingot, whilst No. 11 was taken from the center. The ingot shows a higher quality on its exterior, which indicates its suitability for guns, where the interior would be bored away. The Manganese-Bronze Company are, in fact, now making some castings for artillery.

Delta Metal.

The second and latest example of the successful addition of iron to bronze is afforded by delta metal, which was brought out by Mr. Alexander Dick in 1883. And here it may be as well if the author explains how this alloy came to receive its name. He does so because it was one of the first inquiries he addressed to the inventor, and because he has several times been asked the question, and if the invention had any reference to the delta of rivers. The author need hardly say that it has no such reference. The name "delta" was given to it by Mr. Dick simply for the purpose of connecting it with his own name, delta being the Greek for the letter D, the initial of the inventor's surname. In his researches and early experiments, and, in fact, in the development of delta metal into practical form, Mr. Dick was influenced by the circumstance that some twenty years since Aich and Baron Rosthorn, of Vienna, intro-

TABLE V.—TESTS OF MANGANESE BRONZE BY TENSILE STRAIN.

No.	Description.	Where tested.	Elastic Limit.	Breaking Strain.	Ultimate Elongation.	Remarks.
			Tons ℔ □″	Tons ℔ □″	Per cent.	
1	No. 1 rods rolled hot.	R. G. F.	11.00	29.00	44.6	Mild, annealed for riveting cold.
2		U. C. L.	18.17	29.29	33.4	Annealed.
3		Do.	23.54	31.60	26.5	As delivered from the rolls.
4		Do.	24.32	31.43	23.3	
5		R. G. F	34.40	39.60	11.6	Ditto, and finished cold.
6	No. 1 plates rolled hot	R. G. F.	14.06	28.46	23.2	Pulled across fiber. (Annealed)
7		Do.	14.06	30.13	47.8	Pulled with fiber.
8		Do.	14.80	30.78	34.1	Pulled across fiber.
9		· Do.	16.70	30.10	28.8	Pulled with fiber.
10	No. 2 cast under pressure	M.B. & B Co.	18.00	35.00	22.0	Cast an iron cylinder and pressed while liquid.
11		Do.	16.23	31.90	12.4	

No. 10 cut from side of ingot, and No. 11 from center.

duced a small percentage of iron into copper-zinc alloys, with the view of improving the same. The results obtained, which are tabulated by Dr. Percy in his work on *Iron and Steel*, show that the alloys possessed very remarkable strength and tenacity, and it seemed strange to Mr. Dick that, having such valuable qualities, they did not come into general use. A London brassfounder, who used to manufacture these alloys, informed Mr. Dick that at times he obtained excellent results with bearings and other parts of machinery made therewith, and then again the results were the very reverse, in spite of his taking the greatest care in manufacturing, as he thought, in identically the same way. Unable to account for the different results, he and several other manufacturers were obliged to abandon these alloys in spite of their promising features.

Mr. Dick endeavored to ascertain the cause of the uncertainty of these results, and he produced various quantities of the alloy, apparently in exactly the same way, by dissolving wrought iron in molten copper according to the Austrian method. The qualities of the resulting alloys, however, varied very much, simply because the amount of iron dissolved varied in each parcel. His first object, therefore, was to find a method by which he was enabled to introduce a known and definite quantity of iron, which he succeeded in doing by dissolving the iron in molten zinc to saturation, and adding the same, with or without pure zinc, to the molten copper. The desired quantity of iron can be introduced with great nicety. In consequence of the metals partly oxidizing during the process of remelting, the castings, however, again varied in character, the oxides being dissolved in the alloy and destroying its tenacity and strength. This second difficulty was overcome by adding a small percentage of phosphorus in combination with copper. In some cases Mr. Dick also introduces tin, manganese, or lead into the alloy, to impart special qualities to it. By a series of experiments the most useful combinations were then ascertained, and alloys of definite compositions, and possessing special and very valuable qualities, are now produced under the name of delta metal.

The specific gravity of delta metal is 8.4, its melting point 1800°. In color it resembles gold alloyed with silver. It can be worked hot and cold. When melted, it runs freely, and the castings produced from it are sound and of a fine close grain. Like all copper alloys, it does not weld, but can be brazed like copper or brass, and if the object is of sufficient thickness, it can be "burned" with great facility. Cast in sand, it has a breaking strain of over 21 tons per square inch. When forged at a dark red heat, the breaking strain is raised to from 33 to 35 tons, and when hammered or rolled cold, it will stand a strain of more than 40 tons per square inch. The varieties destined for working cold can be drawn into tubes and wire, or rolled into sheets

and rods, whilst those intended for working hot not only can be rolled with great facility when heated to about 1600° Fahr., but are also capable of being stamped or punched, similarly to wrought iron and steel, into a great variety of articles which have hitherto been cast in bronze or brass. The author would draw special attention to this quality of delta metal, as the possibility of hot stamping offers great advantages over castings—the articles are turned out much cheaper, they are of perfect soundness, and possess three times the strength of brass castings. Blowholes, which frequently can only be detected after expending time and labor, are impossible, besides which a great saving is effected in the finishing of such articles, as, unlike castings, the stampings leave the die almost perfect, requiring little or no tooling, but ready to be polished. Experiments are being made at the present time to utilize the semi-plastic state of heated delta metal to press it by hydraulic pressure into tubes and rods of round, hexagonal, and other sections in a way similar to that in which lead tubes are pressed.

It is interesting to know that the iron introduced by Mr. Dick's process is really chemically combined. This is proved by the alloy not rusting when exposed to the moist atmosphere, and also by its having no influence whatever on the magnetic needle. Experiments have shown that by suspending a piece of delta metal on a thread, and at various angles between the ends of a powerful electro-magnet, no oscillations of the suspended metal could be observed, which evidently proved that the iron contained therein had lost its magnetic properties.

The uses to which delta metal can be applied are very numerous. It is said to replace the best brass and gun metal with advantage, and in many instances iron and steel also, as it does not corrode or rust. Thus parts of rifles, of guns, of torpedoes, tools for gunpowder mills, parts of bicycles, gongs, and a number of domestic articles, are now stamped in delta instead of steel; while spindles for steam and water valves, plungers, and pump-rods are forged in the same metal. In the International Exhibition at the Crystal Palace may be seen a steam launch constructed throughout of delta metal by Messrs. A. F. Yarrow & Co. This launch is 36 feet long over all, with a beam of 5 feet 6 inches, and a depth from gunwale to keel of 3 feet. Delta metal having been proved by experiment to be equal in strength, ductility, and toughness to mild steel, the plates and angle pieces of the launch are of the same scantling as they would be if steel were used, viz. $\frac{3}{32}$ inch thick. The stern, keel, and sternposts are of forged delta metal, and scraped together as is usually done. The angle frames are made of the same material, but are placed longitudinally instead of transversely, by which means greater longitudinal strength is secured. The propeller, which is cast in delta metal, is four-bladed, 2 feet 4 inches in diameter, and 3 feet pitch. The engine is of the usual direct-acting inverted type, of sufficient power to develop a speed of from 8 to 9 knots per hour. The superiority which delta metal possesses over steel and iron for shipbuilding is that it does not rust like they do. Such launches are specially adapted for the transport of salt, sugar, and chemicals, which rapidly corrode steel. The extra first cost would probably be quickly repaid, besides which the metal always retains it value.

From further experiments it was found that the elastic limit of delta metal is 31,571 lbs., or 14.1 tons per square inch. The commencement of permanent set took place at 49,757 lbs. equal to 22.2 tons per square inch. The breaking strain per square inch was 80,658 lbs., equal to 37.34 tons; the elongation was 12.9 per cent., and the contraction of area of fracture 17.4 per cent. The crushing test, with a stress of 22,000 lbs., gave 0.80 per cent. of compression; 44,000 lbs., 1.33 per cent.; 66,000 lbs., 2.03 per cent.; 77,000 lbs., 2.71 per cent.; 88,000 lbs., 3.87 per cent.; 99,000 lbs., 5.77 per cent.; 110,000 lbs., 8.20 per cent; 121,000 lbs., 10.76 per cent.; and 132,000 lbs., 13.41 per cent. The ultimate or crushing stress per square inch was 135,700 lbs., equal to 60.5 tons. In the torsional tests the sample was 10 inches long, and it will be seen that it was twisted more than twice round, the torsion being registered in degrees. The chain tested was a portion of one of those supplied to the Brazilian armor-clad turret-ship Riahuelo. The length tested was 10 feet, the links being 0.733 inch in diameter, and the length

giving way under a stress equal to 19.3 tons per square inch.

Phosphor Copper.

We have now done with the modern bronzes of what the author may call the primary series, and have reached those which may be considered as forming a secondary series, in the sense that several of them are more or less but modifications, combinations, or adaptations of those previously described. The author has already referred to the beneficial effect of phosphorus on copper and its alloys, and which consists in producing a material of absolute closeness, of the highest possible degree of toughness and elasticity, or, according to the percentage of phosphorus added, the metal can be rendered soft, or as hard as steel. These excellent qualities must not be attributed alone to the phosphorus in the metal, or only in a secondary degree, but are owing chiefly to the absence of oxygen, which, by the energetic reducing action of the phosphorus, is entirely eliminated. This question having received the attention of Mr. W. G. Otto, of Darmstadt, that gentleman conceived the idea of introducing phosphorus into copper in order to facilitate the work of engineers and founders by enabling them to add a certain proportion of phosphor copper to a given bulk of metal, so as to obtain a phosphor bronze suitable for various purposes. Phosphor copper contains from 15 to 16 per cent. of phosphorus, and Mr. Otto, who is represented in England by Mr. G. Hartmann, applies this product to the purposes of producing phosphor-bronze, homogeneous copper castings, and copper alloys of all classes. In practice the copper is melted in the ordinary way as rapidly as possible, and is kept covered with charcoal. After the requisite quantity of tin, zinc, &c., has been added to the liquid metal—or if remelting old or scrap metal, after these have been completely melted—the crucible is taken out of the furnace and the metal carefully freed from the charcoal, &c., floating on the top, after which the small quantity of phosphor copper necessary is added whilst thoroughly stirring the metal. A skin which is found covering the liquid metal (bronzes in particular) then disappears, and the metal assumes a perfectly bright surface, which is a sign that the quantity of phosphor copper added was sufficient to remove all oxides present. The metal is then cast at once, care being taken to prevent foreign substances and any skin which may meanwhile have formed again from being poured into the moulds. The presence of a perfectly bright surface is absolutely necessary, and also careful stirring of the metal down to the bottom of the crucible, for which purpose Mr. Otto makes special stirrers. With regard to the proportions to be observed, the author is informed that for producing phosphor bronze and remelting old gun metal an addition of from 10 to 12 ozs. of phosphor copper containing 15 per cent. of phosphorus per 100 lbs. of metal is generally sufficient, whilst for making and remelting brass and brass scrap an addition of only 5 to 7 ozs. of phosphor copper is required per 100 lbs. of metal. The author understands that Otto's phosphor copper is in use at many continental government works, as well as by foreign railway companies and at private works, with every satisfaction. Without for one moment wishing to appear as questioning this, the author still adheres to the opinion already expressed, namely, that to purchase the ingots of metal already prepared for the various required purposes by those whose sole business it is to produce them, and whose credit is at stake if an error of proportion be committed, is to his mind the more satisfactory course. He conceives it to be pitting rule of thumb against careful research and practical experience.

Phosphor-Manganese Bronze.

There is in the market a compound known as phosphor-manganese bronze, which is stated to be used for engineering purposes. The author has twice written to the producers of this alloy for information respecting its nature and uses, but without receiving a reply to either of his letters. It is, therefore, the manufacturers' misfortune, and not the author's fault, that this production is not described in the present paper. Silence, however, is said to be golden, and, looking at the compound name of the alloy in question, the adage may apply with special force in the present instance.

Phosphor-Lead Bronze.

About the middle of the year 1881 a

new alloy under the name of phosphor-lead bronze was brought to the author's notice. It was stated to be specially adapted for all purposes where gun metal, brass, or other material is used in the construction of those portions of machinery subject to constant wear or continuous friction. The introduction of lead into its composition, and its homogeneousness, were said to give it special properties, rendering it most efficacious for the purpose mentioned. It had been subjected to every test that experience could suggest, and was reported to have passed them all most satisfactorily. The advantages claimed for phosphor-lead bronze were self - lubrication, greater wearing capacity than any other metal or alloy, great tensile strength—combined with extreme hardness—and non-liability to fracture. It was averred that it remained perfectly cool under continuous and excessive friction, and it was said to be in extensive use on the Continent by manufacturing engineers, shipbuilders, and others with satisfactory results. It was being introduced into this country by Messrs. H. H. Kuhne & Co., of Lobtau, near Dresden; but the author has not heard anything further of this alloy.

Phosphor Tin.

Under the name of phosphor tin, Messrs. Billington & Newton manufacture a compound which is used for making phosphor bronze. This compound is a mixture of phosporus with tin in certain proportions, the metal being sold to consumers who make their own phosphor bronze by adding the phosphor tin to copper. The copper is melted in a crucible, and when in a fluid state, the phosphor tin is added in the same way as ordinary tin. The mixture is then well stirred with an iron rod, covered with a coating of blacklead. The metal is poured just before it begins to set in the crucible, and the moulds are always to be very dry, and where 20 per cent. of phosphor tin is used, the castings are made in chills if possible. The author has applied to the manufacturers for detail particulars and tests, but he has not been favored with them. It will be seen that phosphor tin is used in the same way as the phosphor copper, already referred to, and although the use of phosphor tin is stated to be attended with good results, the author can only repeat the opinion already expressed, that, considering what diverse effects a slight variation in proportions appears to bring about, it is better for most purposes to obtain the alloy ready prepared than to trust to workmen for obtaining uniform results. The author is aware that he may be met by the statement that successful results are daily obtained by workmen using these compounds. This is no doubt true so far as it goes, but it is no answer to his assertion that rule of thumb can never hope to compete with scientific exactitude. He could give instances in the metallurgy of iron and steel where rule of thumb principles held out for a long time against the advances of higher science, but to whose dictum they were in course of time compelled to bow.

Aluminium Bronze.

The history of the practical manufacture of aluminium does not extend very far back into the past; in fact, its commencement dates within the limits of the present generation. The three international exhibitions which have been held in Paris since aluminium began to be worked on a commercial scale form so many landmarks of its progress. In 1855 it was met with for the first time in the Palais de l'Industrie in the form of a large bar, and was exhibited as silver produced from clay. In the exposition of 1867 it was to be seen in a more advanced stage, worked up into castings and various kinds of useful and ornamental articles. There also for the first time was seen the alloy alluminium bronze. The Paris exhibition of 1878 witnessed the maturity of the aluminium manufacture and its establishment as a current industry, having a regular demand and supply for certain purposes within the limits permitted by its somewhat high price. To France, then, is due the merit of having been the first country to carry out Wohler's process for the production of this metal on a commercial scale, and to have created the aluminium manufacture. Until recently, moreover, France appeared to be the only country in which the trade was able to prosper. The English manufactory established at Washington, near Newcastle-on-Tyne, by Messrs. Bell & Co., did not answer, and was closed some ten years ago. The German factory started

at Berlin, by Messrs. Wirtz & Co., hardly lived at all, having drooped before it was well started. In France, however, the manufacture appears to have gone steadily on from the first.

The chief obstacle which has retarded the development of the manufacture of aluminium in England appears to have been the difficulty of obtaining the metal pure, the least portion of foreign matter rendering it useless for the purpose of alloy. This circumstance, coupled with its high price, has caused its use to be very limited, although the value of aluminium alloys has long been well understood. About two years since, however, Mr. James Webster perfected his invention for producing aluminium, and which is now being practically worked. It is claimed that by his process both the objections just stated are removed, the alumina being produced in a condition of great purity, without a trace of iron, and so by care in the succeeding stages the aluminium itself is free from all contamination with foreign substances. In Webster's process the source of the alumina is potash alum. This is ground to powder and mixed with pitch and gas-tar, in the proportion of 8 parts of alum to 1 part of pitch and 1 part of tar. The mixture is then thrown on the bed of a reverberatory furnace and carefully heated. By this means the water only is driven off, while the sulphur and iron remain with a little carbonaceous matter in the cake. The operation requires careful attention, since, if the temperature be raised too high, burnt alum, which is unalterable, is the result. The fused mass is then removed from the furnace, and is ready for transference to the hydrate retorts. These are made of refractory fire-clay, and are vertical. They are charged from the top, whilst a mixture of steam and air is blown in at the bottom. At a red-heat decomposition takes place, sulphuric acid and sulphur, together with iron sulphate, being carried over mechanically by the steam and condensed in cisterns at the rear. The alumina and potash remain behind as a white cake, which is drawn from the retorts, transferred to a large tank, and lixiviated with water. The liquor which contains all the potash is run off into a pan and evaporated, while the alumina is transferred to bags and allowed to dry.

The Webster process appears to be a simple and successful one. All the by-products are regained, the potash and sulphur are extracted, the iron is thrown down as a fast and brilliant blue, and the residual sulphuric acid is concentrated for use. The alumina thus obtained is balled with charcoal and salt, made into a chloride and reduced for metal, the aluminium obtained being perfectly pure, and containing no iron whatever. The process is being worked by the Aluminium Crown Metal Company, and the metal itself combines strength and lightness with elegance of appearance and general utility. The metal is of two kinds, white and yellow, the former being used for cutlery and other table requisites where silver and plated goods are now employed, and for every kind of metallic fittings, lamps, harness, and carriage furniture, chains, wire, and wire ropes, and, in fact, for every purpose where a non-oxidizing bright surface is a desideratum, strength also being kept in view. The yellow metal is adapted and is stated to be used for guns, screw propellers, engine bearings, tubes, and all the various details of machinery where gun metal and other alloys are now used. The metal, as made, is supplied in ingots to the manufacturers of the various articles indicated, who produce the finished goods for the market. It is reported to stand exceedingly well in engine bearings, and also to give perfectly satisfactory results as applied to the manufacture of screw propellers. The metal is made in five qualities, and each quality is made either hard or soft as may be required. Samples of aluminium bronze tested by Mr. Kirkaldy have given very high results as regards tensile strength, one specimen showing an ultimate stress of 42.4 tons per square inch of original area.

Silveroid.

This metal was introduced to public notice in the early part of the present year. It is an alloy of copper and nickel adjusted with either zinc, tin, or lead in various proportions according to the purpose for which it is intended. Beyond this, however, there is a special method of treatment at a certain point in the manufacture which is stated to be the secret of success. The result is a metal of great whiteness, brilliancy, closeness of grain, and tensile strength. It is the invention of Messrs. Henry Wiggin & Co.,

and it is mainly intended to take the place of alloys of the brass, bronze, and gun metal classes, in fact of every inferior metal where color, polish, weight, and richness of luster are desirable. The author is informed that it is in use for machinery bearings and for all kinds of cocks, valves, and engine and boiler fittings.

Cobalt Bronze.

Since bringing out their silveroid, Messrs. Wiggin have developed another alloy, which is now being introduced under the name of cobalt bronze, and which is a whiter but slightly more expensive metal than silveroid. It is, perhaps, the more interesting of the two, because there is introduced into its composition small quantities of the metal cobalt. The malleability of cobalt in a pure metallic form has long been understood; but the author believes it was not until a few years ago that it was demonstrated by Messrs. Wiggin that it might be rolled into sheets, and wrought, like other metals, into articles of utility. Its high price, however, interfered with its production, and militated against its use. This fact induced Messrs. Wiggin to endeavor to compound an alloy in which the sterling qualities of this valuble metal could be fully represented, and which, at little more than the cost of ordinary German silver, might possess in a large degree all the attributes of the pure metal itself. Possessing, as it is said to do, many of the qualities and every appearance of metallic cobalt, it is manufactured in several qualities, the higher grades being preferable, on account of their suitability for casting purposes, their close steel-like surface, their susceptibility of a high polish, as well as their hardness, toughness, and great tensile strength. Cobalt bronze is intended to be used for the same purposes as silveroid, especially in high-class fitting work.

The author has now fulfilled the promise made at the outset of his paper, which was to place before the society a few facts concerning such modern bronze alloys as are being usefully employed for engineering purposes. He does not lay claim to any independent personal research or experiment upon the subject, such being precluded by the constant demands upon his time in other directions. He has consequently had to accept the results obtained by others, but he accepts them in perfect good faith. As they stand, they indicate the value and importance of the modern bronzes, and will serve as guides in the choice of a metal for a given purpose. The question of modern bronzes, however, is a very important one, and presents a wide field for scientific investigation and practical research. At the present time we are comparatively in the dark upon this subject, which has not received the careful attention at the hands of metallurgists that it deserves, and if the present paper only awakens attention to this fact it will not have been written in vain. It will be seen that the author has divided the bronzes into two classes, namely, those which come under the head of original productions and those which he can only consider as imitations. In thus distinguishing them, he desires to give offence to none, but he sees no way of evading what he believes to be a common-sense and equitable conclusion. In conclusion, the author will only suggest that, in selecting a material for practical use, proper regard should be had to the precise purpose the metal is required to serve, and that careful scientific preparation, based upon long experience, should be allowed to prevail against rule of thumb and a possible small saving in first cost. He would the more strongly impress this view of the case upon the members because he well knows that, although imitation is the sincerest form of flattery, flattery usually has to be paid for by someone or other.

For a considerable number of years proposals have been made for joining Sydney and the North Shore. Among the plans recently suggested for the connection of Sydney and the North Shore is that of a continuous suspension bridge, high enough to allow of the passage of shipping underneath. Mr. S. Pollitzer, C. E., of Sydney, has prepared a design of such a bridge, estimated to cost £430,000. The structure is designed to connect Dawes Point and the opposite headland. It includes two massive piers of masonry, which support the floor of the bridge at a certain height; and the cables are carried through the towers above. The central span is 700ft., and each of the two side spans 350ft. The height is given at 100ft. clear. The sectional area of the chains is 1904 square inches, and of the rods, 1152 square inches. The weight of wrought iron to be used is 7,880 tons, and that of cast iron, 945 tons; the weight of each abutment is 13,800 tons, and the mass of masonry altogether is 52,600 cubic yards. The cost per foot run is £307 8s.

THE MODERN ARCHITECT AND HIS ART.

By JOHN D. SEDDING.

From "The Building News."

In approaching the consideration of the modern architect and his art I feel, to use Mr. Lowell's recent words, that there is little chance of beguiling a new tune out of the one-stringed instrument on which we have been thrumming so long. Without, however, affecting to say anything new, "where everything has been said before, and said over again after," I desire to draw attention to a view of our art which has been singularly neglected, and which, to my mind, deserves infinitely more prominence than any words of mine can give it. What I have to say about modern architecture refers not so much to its archæological triumphs, its teeming types and annual revivals, nor to anything that therein is, but to that therein is *not*. So, also, what I would say about the modern architect refers not so much to his wide knowledge, his daring anachronisms and matchless manipulations of historic ornament, but to his shortcomings—not to how he bewitches the general public by what he is, and what he could do if he tried; but to how the intelligent public may fairly be disappointed by what he is *not*, and what he cannot do. In a word, it is as as to the scope—or perhaps I should say the limitation—of modern architecture and the ideal of the design (if he have any) to which I wish to draw your attention. Naturally, I have chosen a subject which interests me, and, in pleasing myself, I hope I may please you; or at least I may offer an agreeable diversion to brains sorely racked and tired with studies and designs in various styles and periods. There is, I am aware, some danger attached to the criticism of a close profession like that of architecture, which has a royal charter dating from the 7th year of William IV., and which knows how to consume its own smoke. As, however, my point of view is quite an impersonal one, and my remarks general, and as I come before you without a single half-brick in my pocket to heave at anybody, my harmlessness is manifest. I shall then speak my humble mind with all the directness I can command, and trust to your kindness to take no offence where no offence is intended.

It is idle to shirk disagreeable questions, and so I begin with a simple proposition which covers much of the ground we shall traverse to-night. Is architecture, as practised by the modern architect, worth living for? It is a question I have more than once asked myself, but I am not candid enough to confess to you what reply I gave to it. In placing it thus in the forefront of this paper, let me say that the very last thing in my mind is to propagate doubt in the fold of the faithful where none exists; or to shake the confidence of such practitioners as are satisfied not only with the prospects of modern architecture, but with their own prospects and with the worth of their own contributions to the great volume of immortal art. To my mind the question is most suitable to the present time. I will not say that a "crisis" is approaching in the affairs of architecture, because the phrase has lost all its potency by frequent repetition in the newspapers, where we understand that a "crisis" occurs in national affairs every second day. But I will say that these are critical times for us. A strange calm has come. There is a sense of impending change. This is a time of felt uncertainty, of stranded purposes, of searchings of heart—a time when the issues of things connected with the arts of design are hanging in the balances. This is a time, too, of disillusionizing alike for architects and for people, when we ourselves are not quite so confident about our method of pushing architectural design forward by means of impulses of an essentially fleeting nature, and when people are beginning to realize that every branch of architecture is well represented by outsiders, and when they are beginning to question the *raison d'être* of the architect at all. This question is, then, a practical one, and one which it is desirable to face and to answer. It at once puts the

modern architect and his art in their right place. It makes us compare ourselves not with ourselves (which is not wise), but with the masters of old who brought trained powers, sleepless ambition, and passionate devotion to their work. It has this good effect, moreover—it at once breaks the spell of that direful boa-constrictor of art, mere professionalism. Yes, and in addressing it to the Architectural Association I cannot forget that I am speaking to those to whom the destinies of English design are to be committed, and it is for you to ask yourselves how you view and how you estimate the art you follow—whether you look upon architecture as a divinely inspired art that can rightly claim all the devotion of your being, or whether you take up architecture merely as an honorable profession and a gentlemanly calling. If you take up architecture as your vocation, to be followed with the ardor of a religion, I am not sure that you will succeed in gaining riches or fame; you may have to be happy with small opportunities and small gains, and have to live a life of quiet unnoticed worth. But you will be happy and contented and grateful all the same. If, on the other hand, you go in for architecture as a profession which only needs the efficient handling of a T-square and ruling pen, you may, if you are a good, steady fellow, rise to be an eminent practitioner. And if you are a successful practitioner your rewards are great; you may have access to the best society, and to the best columns of the *Times* newspaper; you may be a lion at evening crushes, and wear brown velveteen; you may pose as the patron of the very fine arts, and be a judge of *bric-à-brac*, and a connoisseur of Queen Anne teapots, Chippendale chairs, and such like; you may even hope to be the F. S. A. and the F. R. I. B. A., and even the P. R. I. B. A., if you have paid your subscriptions and are alive when your turn comes. Nay, if as architect and surveyor you have a sufficiently large and lucrative city practice and have time for such things, you may aspire to reach the souls of the people by the art of your tongue as well as by the art of your hand, and almost succeed in adding M.P. to your other titles. And to win these rewards you have only to be a rough-and-tumble ordinary man of the world, with a head on your shoulders, an eye for figures, a well-supported air of general competency, good business qualities, some power of gracious fooling, and the faculty of turning out just what the world expects from you with promptitude and dispatch. But as for art, and the mastery of the crafts, and the power of color and form and all that sort of thing, you may neither have any, nor need your friends suspect that such things come within the make-up of the modern British architect! Of course it is ever the snare of enthusiastic youth to press inconvenient speculations home, and it is because I am in the presence of the aspiring fledglings of artistic gifts and good parts who form the Architectural Association, that the question as to the innate worth of modern architecture comes before me. In another place, where the birds are not only fully fledged but have feathered their nests, and, like Jeshurun, are not exactly able to soar, I dare not hazard it, nor you either. Let it not be supposed that I have low opinions about architecture, or that I would willingly shake the allegiance of any young heart that has found peace in its pursuit. Let no waverer be downhearted; there may be a lucrative future before him. Let him stick to his last, by which I mean his T-square and ruling-pen.

To proceed. I said just now that this question touching the worth of modern architecture as a serious life's pursuit puts our art in its true place. Instinctively one feels that while it is applicable to the modern architect and his art, none but a fool would have put it to William of Sens, Jocelyn of Wells, Alan of Walsingham, William of Wykeham, Thomas Chard of Glastonbury, or of Bramante, Michael Angelo, Christopher Wren, Inigo Jones, or Adams or Chambers, and there must be a reason for this.

Again, none but a fool would ask the modern musician, or the sculptor, or the painter or poet if his art were worth living for. Indeed, here are living arts, each with its ideal conception to symbolize, each with its mission to stimulate, delight, and console mankind, and to raise men's minds out of money-grubbing grooves into a less selfish, less sordid, less commonplace atmosphere. It is significant that in each of these cases the artist is his own craftsman; he thinks

his own thought, clothes it himself, and spares no pains in the elaboration of the clothing. He keeps no ghost, and if he does he is not thought to be respectable. But the architect's ghosts are legion—on his premises and off them—and he is not one whit ashamed. In calculating the place and mission of the modern architect, one is reminded of what is happening in the bee-world just now. By the aid of an ingenious patent, ready-made cells are stamped out in wax (adulterated, of course) of the correct shape and size, and when placed in the patent hive the bees forthwith complete the cells and fill them with honey. And the very counterpart of this is happening in the human world; the royally-instituted architect makes the cells, and the decorators and manufacturers fill them with honey. You know quite well that the English people have not to thank the British architect for the poetry of their homes. You know that one of the noblest provinces of architecture, that of turning necessary articles of daily use into works of art, has fallen from the architect's hands. You know that all the pretty things that dignify modern life come from the "largest furnishing establishments in the world" in Tottenham Court road—from those homes of champagne and shoddy where the red sealing-wax "Early English" furniture, and the wood coal-boxes adorned with roses and daffodils and the cast-iron over-mantel china closets come from; where you may get a dozen very cheap high-class native oil-paintings at one counter, and a dozen very dear native oysters at another.

Again, we must confess that the other contemporary arts I have enumerated have been affected for the better and not for the worse by the influences of the day. Each has won new triumphs, each has found out new chances of appeal, new domains for display. But not so architecture, for while it has gained nothing it has lost nearly all. In respect of the use of iron for constructive purposes, and of patent sanitary appliances, which builders and sanitary engineers have devised for us, we score something. Yet, however blessed the iron joists and D-traps are, and however lucky we are to be able to use them, the architects of old, who knew them not, were infinitely more accomplished all-round men than ourselves; and I do not know that, after all, our houses are either more stable or more sweet and wholesome for body and soul to inhabit than the old homes of old England.

But further. The practice of these arts of color, sound, form and word directly conduce to the development of artistic genius; nor could you be a successful composer if you had no musical genius, nor an eminent literary man without literary genius. Yet you can be accounted an eminent architect, and reap all the honors of the profession, without possessing or feeling the want of artistic genius. In putting the case thus strongly, do not suppose that I am blind to the noble gifts and genius of certain architects working with us and shedding their helpful influence amongst us at the present time; and, but for my resolve to keep this paper impersonal, I would name them and speak of them with all the genuine admiration and respect I feel for them. Do not mistake me on this point; I speak of rank and file, and not of these. And I ask whether architecture as now practised ought not rather to be accounted as a "useful" than as an "ornamental"—or, as some would call it, a "fine"—art? I ask whether architecture can any longer be termed the "Queen of Arts," when all that remains of her is the skull and the feet and the palms of her hands? I ask if it be not true that architecture has ignominiously resigned her throne, lost her honors, and bartered the sceptre of pre-eminence with which she has held sway from time immemorial, and only reserved for herself the sovereign right of levying a tax of 5 per cent. on other men's labors? I ask whether it is not true that the engineer has (whether civilly or uncivilly it matters not, as the thing is done) robbed the architect of one-third of his domain on the one side, and whether the decorator and manufacturer have not between them robbed him of another one-third on the other side? I ask whether the architect of to-day is, or need be, anything more than a paper-draughtsman to sit on a stool and invent new sorts of doors and windows? I ask whether his business in life is not that of a designer of shells of houses for decorators and manufacturers to finish and furnish, and who varies this jackal occu-

pation by occasional jobs for an engineer, who hires him to do the "pretty" upon a bridge or railway station? Yes; and such of us who like to see iron skeletons clothed in shoddy ornament may, after refreshing our bodies, refresh our souls at the York or Bristol railway station, and realize at the same time the mission and scope of the modern architect and his art.

Now if you think that what I am saying is approximately true, you will agree with me that it is high time the position of the modern architect and the issues of his art were overhauled; and when this shall be undertaken, I know no better place for the investigation than under the roof of the house which contains the Royal charter granted expressly to a certain institute for the advancement of architecture and the various arts and sciences connected therewith. If it be for the better advancement of the arts and sciences that architects abstain from personal relations with them, then it must be granted that they are, with much self-denial and self abnegation, fulfilling the obligations of the charter under which they are enrolled. However this may be, I cannot help saying that, to my mind, every celebration of the Institute commemorates not the marriage, but the divorce of architecture from the arts and sciences connected therewith. I have laid before you evidences of this in what has already been said, and it would be easy to go on multiplying the proofs. Indeed, it is an undeniable fact that the arts and sciences which of old were ever indissolubly connected with architecture, have passed to the care and conduct of the specialist and the manufacturer. The British public goes to its shops and specialists for any matter connected with domestic art; and if you are a parson with wants, you go to an ecclesiastical shop, and while one shopman is fitting on your coat, or taking the shape of your parsonic head for a new stiff hat, you can be ordering of another shopman a sculptured reredos, an altar and font and lectern, and that sort of thing. Yes, and I saw a striking letter the other day, written by the head of a well-advertised carving establishment, which stated that, inasmuch as not more than half-a-dozen of the writer's architect clients could prepare their own detail in an artistic manner, he had started an office and staff of clerks to do for the architects what they could not do for themselves. And remember that the architects here referred to were of the Gothic school, which represents the best masters of detail. Even in the matter of building houses, the better sort of builder has his own staff of draughtsmen (or compiling copyists, as some would call them), who can invent new sorts of windows and doors, and draw convenient plans, and make pleasing combinations of colored materials after the approved fashion. The public may soon begin to inquire wherein the architects' clerks and the builders' clerks differ. The State, as you painfully know, has a very summary way of dealing with the architect, inasmuch as it entrusts its buildings to the engineers and officials of South Kensington, and maintains an office of salaried draughtsmen for carrying out public architectural works. And what is happening at Kensington, where engineers combine with ornamentalists to carry out the State's architectural works, may happen in other cases; for the public will see that, given a good builder, an engineer, and an ornamentalist, any building is possible. And the architect has only his own sloth and incapacity to thank for a state of things which in process of time will assuredly work his own extinction. The experts he has called into existence have silently undermined his position. He called in aliens to help him in his need, and the alien army is a standing menace to his position, and will in time dispossess him. Lacking science and lacking art, he is just nowhere if the scientist and the artist combine for his effacement. There is a good deal of what Mr. Ruskin would call professional "bow-wow-wow" talked at our conferences and in the journals about the rights and wrongs of the profession; but what cares the world about the architect so long as its wants are somehow supplied? Although we abuse it, the world is fair in this respect, it values us at its own valuation of our worth. It knows we keep ghosts, and it makes no nicely-drawn distinction between an "expert" and a duffer!

But in order to clear the way for some few practical observations I must arrange the subject under three heads: (1) What

is architecture, and what were the functions of the architect in old days? (2) When, and from what cause did the change from the old to the new system take place? (3) Is it possible for architecture under its present conditions to be carried out upon the old lines, and, if so, by what means? Here are three points, each of which would serve as a theme for a long lecture, so that my treatment of each must needs be brief and simply relative to the matter in hand.

As to the first point, although addressing a professional audience, I cannot define architecture as building erected after an architect's design. One might as well say that the snuffmaker was the final cause of the human nose! There is building which is, and building which is not, architecture; and I would define architecture as imaginative building; in other words, building which expresses the invention or imagination of the builder, and which appeals by this means to the imagination of the spectator. If it is to answer to the description of architecture, the building must have a soul as well as a body. The body is the structure answering to the primary purpose of its erection, and this body should be staple and convenient. The soul is that superimposed something extra to the body—that something which is provided beyond the demands of mere utility, and which is really the expression of the builder's thought and his mode of appeal to the sympathy and imagination of the spectator. In this definition you get the three cardinal virtues of architecture represented — namely, stability, which relates to science; convenience, which relates to good sense; and beauty, which relates to taste. Naturally, the primary purpose of a structure, combined with other like conditions, settles its character and the fit extent of its decoration; and yet, while it is quite fair to define the word architecture as the art of building nobly and ornamentally, you cannot gauge the value of a structure by the amount of its ornamentation. Dance, who built old Newgate, was an architect, and although his structure has dead black walls of rough-hewn granite, relieved only here and there with niches and statuary, and has a savage repellant air, it is imaginative building, and speaks directly to the imagination of the spectator of violence and doom in the true grim Northern manner. A mere builder would have put plain brick walls. And architecture all the world over has the same characteristic qualities—however different the types and the styles of the art represented, however different the scale of the structure, however different the culture and aims and methods of the builders—the architecture carries the impress of thought or invention, or imagination befitting an ornamental art. Architecture is truly a human art, a volume and record of human thought. As long as the structure remains you connect with it the memory of the men who built it. For instance, the monumental art of the west front of St. Albans Abbey is a more lasting memorial of its reputed father—our only British architect—than the cracked bell at Westminster. And so with other immortal specimens of other immortal artists, "soft-handed" or otherwise. As you look at the architecture of Egypt and Greece you associate it with its authors. The work is steeped in thought, instinct with invention, and—so far as its ornament is concerned—eloquent of pleasurable labor. It represents problems of proportions. Ideas are expressed with mathematical accuracy. In Greek art we have, as I need not remind you, the science of building united with accuracy of design and execution. The arts and sciences are here united perfectly. The tide of tradition is represented in full volume, and the designer is the exponent of traditions that commenced in Egypt, and flowed onward through the Greek and Roman and every other period till broken by the Gothic revival.

As I have just said, the architecture of the modern world answers, in all essentials, to the architecture of the ancient world, however different its aims, and character, and mode of appeal. With regard to the latter point, the Classic is a more intellectual art, and demands a more intellectual appreciation. The Greek architect is a man of complete culture, learned in philosophy and geometry, and he addresses his peers. This explains why it is some of us find the heights of Classic art cold, and the atmosphere that surrounds it bleak and grey. The modern architect, like the ancient, is the

right man in the right place; and, whether he be cultured or uncultured, prince or ploughman's brother, he is the most skilled man in the building crafts upon the job. The difference is that, being a Christian, he is no respecter of persons, and being a modern, he is no respecter of calculated academic rule, but speaks his thoughts simply and spontaneously, and addresses his art both to learned and unlearned, to rich and poor, to bond and free.

But now, as I turn aside to define the functions of the architect under the old system, I at once feel the ground shake beneath my feet. For who can forget the storm of 1874, after Mr. Fergusson's unfortunate deliverance in the *Quarterly Review* upon this very head? The story of that time affords, I think, a really valuable glimpse into the secret motives of the British architect. The veil lifts for a moment, and he stands revealed with the touchstone of his art in his hand. Directly the elevated position, the professional status and social level of the architect is threatened from below an army of "soft-handed gentlemen" rush to the rescue. Never in the annals of art (or the history of the Institute, which is the same thing) had so much power of eloquence, so much literary talent, and so much genuine enthusiasm been evinced. The British workman was supposed to be on the march to Conduit-street to demand enrolment as a Fellow of the Royal Institute, and to be, in this way, there and then constituted into an architect; and, although under the pressing exigencies of the case the parish beadle from the neighboring church, in all the majesty of his Sunday clothes, had been hired to watch the portals of No. 9, and although the Fellows had constituted themselves into a vigilance committee, in day and night relays, to guard their Magna Charta, something dreadful might have happened had the threatened invasion taken place. After all, however, the "unemancipated" British workman stirred not, but abode in his breeches, where I will return to him anon. Looking back at the pitiful affair (and the literature of the episode is innocently printed in the Institute's transactions "by order of the Council") I have only one remark to make, namely, that whereas the architects were preposterously alarmed lest the workmen should become architects, it never struck them to try themselves to become master-workmen, and so to gain the respect of the workshops by their own eminence in the crafts rather than by giving themselves airs because of their professional status and soft hands. Luckily for me, it is immaterial to our purpose to inquire as to the social status of the architect as a person, or whether he had soft hands or hard. One thing is certain about him —cultured or not cultured, hodsman's cousin or not—he contributed the requisite amount of knowledge and theoretical science, and did not retain experts; he was in direct contact with the work as it grew up; he saw how things were done, and was not the mere figurer of details at an office; he was the familiar spirit of the building, and not the distant dictator of its details. And besides having a general knowledge of handicrafts, he was master of at least one. Some architects were modelers, some carvers, some workers in marble, or in gold, or in ivory, and, plainly enough, we can infer that they worked in workshops, and not in offices or studios. "In Greece," Winckelmann says, "the best workman in the most humble craft might succeed in rendering his name immortal."

Let us turn for a few moments to Italian Mediæval art, for we know so much more about the architects of Italy than of those of any other country, and they afford us a ready type of men whose functions covered every matter pertaining to construction and ornament. The Italian architect was engineer, builder, painter, decorator, sculptor, modeler, metal-worker, goldsmith and the rest; or, at least you might expect that the same man could paint a picture, carve a subject, draw and model a bit of ornament, make a gold casket or an urn, design a dress or a fabric, build a church or a palace, or a bridge. Thus we see how wondrously the arts were interwoven and technical skill was diffused in Mediæval Italy. One craft overlapped the other; there was no hard and fast line of demarcation between them as with us, and no professionalism, and no Salvation Army of specialists behind the scenes. Naturally the poor Italian architect had never heard of the native Asian, African, and American styles so much in favor in

our classes of design; but had he professed to design any sort of building he would not have left it to the expert to fill it with plaster work, or marble, or wood inlays, or bas-reliefs and color devices; and his art would extend to the provision of gorgeous chests and furniture, and perhaps even to the dresses and portraits of his esteemed clients. Think of Da Vinci, with his superb power of color and form, of his magnificent designs and projects in art and mechanics, and set this man with his marvelous range, his almost superhuman grasp of mind and boundless ideal, against our puny selves poring over our D traps and ventilation, any quantity-taking, Metropolitan Building Acts, &c., &c.; and if, after instituting the comparison, you are satisfied with the scope and issues of the modern architect and his art, then I think you are eligible to be a Fellow of the Institute without further ado, and I will give myself the honor of proposing you on the first convenient occasion. Now, you cannot properly account for the high condition of Italian art in the middle ages by saying that the Italian people are a phenomenal people, with art in the blood. If so, art would be flourishing in Italy at this time, and it is not. The fact is, that whatever art you examine, of any period, or of any country, you will invariably find that the excellence of the work is only commensurate with the ideal. There is no luck, no chance about it, it is a simple matter of cause and effect; and if the members of the Institute had as high an idea of architecture, and of the various arts and sciences connected therewith, as they have of the privileges of the profession and of their professional status, English architecture would be different to what it is. It needs no prophet to foretell that so long as the modern architect contents himself with groveling views and consumes his soul in small things, so long will he grovel and do small things. In Italy, in the middle ages, there was a grand ideal to animate the artist and to sustain his art. Of course, many things conspired to favor art there and then, beyond the consanguinity with artistic races which doubtless had its effect. Italy was then what England is now—the world's emporium, the seat and center of the world's commerce. There was wealth, and the desire to spend it upon beautiful things. There was the ambition of cultured nobles; there was the inheritance of fine traditions; there was a lovely climate and a flowery land; there was the innate passion for beauty of a passionate and beautiful people. But what raised Italy to her high-water mark of art was the measureless value set upon execution. What Winckelmann said of Greece is equally applicable to Italy—the best workman in the most humble craft might succeed in rendering himself immortal. The designers themselves were masters in the crafts they dabbled in, and they had technical knowledge and technical skill. "Design" then meant something more than it at present does in an architect's office or in our classes of design. It meant the power to *do* as well as to *draw*. It meant executive power and technical skill. It meant that what the brain of the man could conceive that the hand of the man that conceived it could execute.

Coming to our second point we have to inquire when and from what cause the change from the old to the new system of architectural practice took place. And here we must come back to our own country again, first, because we are speaking of English art, and, secondly, because a similar change has not come over the architectural practice of other countries. I will begin by saying that the old system had lasted in the world generally from the building of the Tower of Babel to the time of the Gothic revival. Ever since English architecture was English architecture, it had been born and bred and fostered and propagated in English workshops. The Gothic revival meant not only confusion to architecture but death to the art of the workshop. I do not mean for a moment that the art of the workshop, or the craft carried on there, were of a high order before the inauguration of the new condition of things, but I speak of one system of design as opposed to the other system of design. How could the arts of design flourish then, when, from the king on his throne to the merchant on his stool, no one cared one dump for art? Why, the very life of art, its sinews, its flesh, and its bones is the living thought it contains, and the living interest it creates. If there were no de-

mand for literature, language would not be cultivated. If there were no dancers the piper would cease to play. Will the crafts develop their cunning if there is none to order and none to heed? It was not patronage only that was wanted, but employment. People, when they are uncomfortable about the results of the Gothic revival are fond of pointing to Gower street as a justification for the annihilation of traditional art. But you may depend upon it, had there been the demand for higher things there would have been the supply. However homely, or, if you like, however ignoble, the art done just before the new stimulus came, the traditions of the better times still lingered on in the workshops, and the bricklayer, the carpenter, and the plasterer who hung on were men with some notion of style and some love of detail. The early Queen Anne had its leanings towards the picturesque Elizabethan, and the houses of the period are singularly well adapted to English minds and English scenery, and their fittings are in nowise unworthy of the best traditions of the English workshop. I have purposely made this digression in order that I might insist upon the fact that so long as the traditional art remained in force, and the workshops were the nurseries of design, so long the old scope of architecture, and the connection of the architect with the crafts were maintained. And, while on this point, let me remark upon the significant fact that while certain architects still adhere to traditional art, English architecture gained no advantage by their adhesion; nor did they themselves strike oil, and for the simple reason that, like the Goths, they swamped the traditional art of the workshop with their new-fangled types and rolls of details prepared by the soft-handed clerks in their offices, and accomplished the complete strangulation of traditional art. So it comes to pass that the tale of honored names of English architects passes on from Pugin to Barry, Scott, Street, Butterfield, Shaw, Pearson, Bodley, and Philip Webb, and leaves them—shall I say?—inconspicuous in the crowd.

But I have yet to account for the decay of architecture before the Gothic revival, and also for the change from the old to the new system of architectural practice; and the explanation I offer for the one applies to the other. I have shown how low the arts had fallen at the beginning of this century through neglect, and I cannot see that you could expect that art should engage men's attention when you remember the vast number of social, political, and religious problems that were then agitating England. Professor Seeley's valuable book on the "Expansion of England" has helped me to see why the faculty for design died out with us in the 18th century, for he shows how entirely English interests were then centered in America and her other colonies. Think of the war ships that had to be built, the armies to be equipped, the colonies to be fought for and occupied; and, later on, think of the machine-looms and steam-engines to be invented and perfected, and the railways to be made! How naturally does the engineer spring into existence amid the demand for the useful arts! How naturally does the eye of the historian pass on to the record of that noble set of engineers and machinists and mathematicians — Davy, Watt, Cavendish, Arkwright, Herschell, Stephenson, and Brunell! And how natural that the men of genius should gravitate—not to the ornamental arts as in earlier days, but to the useful arts! Yes, one may well say that English science had produced a perfect vacuum long before the scientific investigator had discovered the way for himself, and that in an unsuspected direction.

And now, having considered the origin of the engineer, who is one of the cuckoo intruders in the architect's nest, let us turn to the origin of that still bigger bird—the ornamentalist or expert in the decorative arts. I said just now that the Gothic revival had inaugurated the change from the old system of architectural practice to the new. Before this revolution of taste took place, the architect was the leading spirit of the building he designed, but he did not stand alone. His designs or models for stone, brick, iron, wood, and plaster-work were backed by the traditional skill and types and methods of craftsmen, each of whom was more or less of an artist. The architect was only the prime minister; the workmen represented the departments. He was only the president, for the time being, of a little republic of art. From what we know of Wren, Inigo Jones, and the

Chambers and Adams, the architect was conversant with every branch of the work included in the structure. He supplied the plans and sketch elevations, and the leading details (as in John Thorpe's case), but the hundred and one odd details required for after-thoughts and emergencies might fall to the conduct of the workman, who, at all events, would be quite competent to deal with them if so required. Here, then, we have architecture carried out under the best auspices—where architect and workmen are in perfect sympathy in matters of taste, the designer has a fellow worker in the handicraftsman, one craft helps and overlaps the other, the executive and the theoretical go hand in hand like twin sisters, the structural and the ornamental proceed along the same lines, and we have building which deserves the name of architecture. The Gothic revival upsets all this harmony of procedure, for the whole of the traditions of the past must be sacrificed, and new types, mouldings, traceries, carvings, groinings, decorations, and the rest of it are introduced, about which the workman knows nothing and cares less. From henceforth you must look no more to the English workshops for the inception of types and evolution of ideas. The old *Téméraire* of English art having been sent to her last home, a bright new Venetian gondola takes her place, and rides proudly out to sea with seven Gothic lamps at her prow, an Oxford graduate and a few able enthusiasts to work the oars, fire off the guns, and take care of the cargo of sketch books and romantic literature on board. Naturally the gondola is first attracted to Venice, but as time goes on the taste of the crew changes, and you find them flying about in all directions, and bringing home valuable spoils in the shape of numberless new sets of doors and windows to offer at the feet of a grateful people. And the merit of the new types consists in this, that they are quite unique in England, and that the British workman cannot move a step, as he copies them, without full-sized details of every part. Now, my explanation of the origin of the specialist decorative artist is this. Having destroyed the old system of art, the Gothic revivalist found himself unable to construct a new system that would work; he had accepted a task which he was unable to cope with. He had a strong love of art, a true sense of the intimate relations of the lesser arts with architecture; but he found things too much for him, and, instead of raising an army of fellow-laborers in the workshops, he called into existence certain specialist assistants to aid him in the conduct of his practice, where he lacked time or ability to carry out the work himself. The mischief of the whole business has been that he was only a learner himself all the time he was carrying out works in various styles. He has been only a blind man leading the blind. He was up a tree all the time himself, and the specialist has been found an indispensable help in supplying his necessities.

I come now to my third point. Is it possible for architecture under its present conditions to be carried out upon the old lines, and if so, by what means? To the first division of this point my short answer is—No, and Yes. No, if the present conditions are to remain unchanged. Yes, if things change for the better. In dealing with the whole matter before us I do not want to arraign modern art for difficulties inherent to it, nor do I want to multiply the responsibilities of the architect. That some of the higher branches of an architect's work have been abandoned is undeniable; and I plead for the recovery of these at any cost. In claiming this I do not desire to extend the radius of the architect's proper work. I am even arguing for the lessening of his labors by bringing the handicraftsman into a more active participation in the work he has to do. This was the old system, and it is the only practical solution of the case. The question is, to what extent our present difficulties are inevitable or irremediable. I have no hesitation in putting at the head and front of our difficulties this of having to employ revived styles. Any suggestion that you or I can make which will indicate some way of mitigating our sufferings in this matter will therefore be a boon. We are in for the use of all the various phases of the various periods of architecture, extending from the 13th to the 18th centuries, in England and abroad; and when it is remembered that the giants of the past had all their work cut out to master the capabilities of one

style only, the vastness of our task is appalling. Every post brings us in a request from this quarter or from that for details of buildings which may each be of a different style. Add to this that one must keep touch with the progressive science of the day, and must be able to speak authoritatively of all the rival "sanitary specialities" and rival ventilating and warming schemes and electric lights, and hygeian rock and asbestos and American joinery, and the scores of dodges for minimizing art in the workshop, and girders and lifts, and "Acme" this, or "Imperial" that, and "Eclipse" or "Last for Ever" the other—to say nothing of having to pronounce off-hand upon Metropolitan Building Acts, and having to wade through surveyors' quantities and builders' accounts — is it any wonder that the architect gets so tired out with the business side of his work that he gladly leaves the problem of art production and ornament to the specialist decorator and manufacturer? You will observe, too, that at conferences and in presidential addresses and that sort of thing, where it is necessary to speak cheerily and respect the feelings of the profession at the same time the architect has invariably only one sovereign remedy to suggest—one patent salve is to heal all our disorders—and that is the specialist. The specialist, either inside or outside of the profession, is to ease everybody and everything all around! The proposal is that there shall be a sort of inner circle of the profession. The profession is to keep a paddock for the prize animals, who are to be warranted to have only one gift each, and who are to run around the paddock in a given groove all their lives. And all the sectionally-gifted persons are to make up one entire concrete architect, on the principle of making a quilt if you have enough patches to cover it. I grant you that, according to the present state of things, specialists must exist to do such things as these—to superintend the imitation of old work, to carry out decoration in a given style, Pompeian, Egyptian, Classic, or Gothic; to restore or build Gothic or Classic churches; Elizabethan, Jacobean, or Georgian houses, and the like. The question, however, arises here—are we to go on imitating the styles of the past? Specialists are necessary if we do go on in our present courses; but if we are to get out of the mists and on to the hill-tops again, we must train ourselves for our future liberty. If we want to perpetuate chaos and will-o'-the-wisp art, I do not know that we can devise a better means to that end than the establishment of representatives of the rival styles and the rival trickeries of the day. But surely we do not want practitioners of one accomplishment or one idea! Surely we do not want to ruin and degrade the noble art of architectural design by introducing into it that miserable division-of-labor system which (as Mr. Morris points out) has, in the case of our manufacturers, reduced the workman to a machine, effaced his individuality, taken away all the pleasure of labor, and destroyed the standard of excellence. The making of architectural design deserves a better system of procedure than the manufacture of a modern pin! Let us, then, listen—no, not for a moment—to the bewitching suggestions on this head. The disorder of modern architecture is too deep-rooted to be remedied by the quackery of a specialist. We will not allow the great factory and machine system introduced in the great art that has fallen to the care of our unworthy hands. Let us rather take courage and look forward to the time when the jumble of styles will be cleared away or reduced to system, and prepare ourselves for an all-round practice in our vernacular that is to be. Depend upon it that it will not be the one-eyed, or one-legged, or one-armed, or one-idead specialist practitioner that will then be sought for, but it will be the architect with the most individuality, the most culture, the most skill, the most efficient training, that will be sought for, and found most useful to the architecture of the future.

But you may well now remind me of my promised suggestion of the means I would propose to bring about the redemption of the old ideals of our art. First, I would say, let architects determine at all costs to recover lost ground. Secondly, let architects endeavor to render the types now in vogue more malleable for nineteenth-century use in our workshops by classification or otherwise, by which means new traditions may be established, and the standard of excellence raised to something of its old

pitch. In regard to the first point, some of us have grown too old in naughty slothful ways to hope ever to accomplish much in the personal manipulation of the handicrafts, but we are none of us too old to determine, God willing, that our younger brethren shall have better chances than we had at their age, better chances for modeling and drawing ornament, and for taking their share in the design of house-fittings and the like. None of us, morever, are too old to help to dignify the labor of the workman whose dusty clothes soil the best Sunday-go-to-meeting coats of the members of the Royal Institute of British Architects, as they accidentally come in contact with him in the builder's yard. We are none of us too old to help to establish new traditions for the workshop, by classification of types and features done in such a way that they may appeal to the workmen in a more practical, familiar, and loveable way than they do now.

May I divert your interest for one moment from that all-important matter, the modern architect and his art, and ask you to look at the British workman. What is his condition? What are the issues of his life's work? What have you done for him? We left him in the eighteenth century, a magnate according to his personal qualifications in his little parliament of art, the workshop, evolving architectural types, and putting his whole soul into his work. In those days he was an intelligent being, following his craft joyfully because he excelled in it, and knew what he was about, and had a felt-place in the world. You have scattered those workmen, you have dissolved these little republics of art that in old days held sway in every town and village in the land, and what have you put in their place? You have drowned the English handicrafts by opening up the sluices of a ceaseless tide of archaic types, and how has your eclecticism affected the British workman? Certainly you have with a vengeance directed his eyes to the wonders of old art, and you have given the charm of novelty to his every-day occupation; you have introduced him to a very Pandemonium of tit-bit types; you have shown him how various have been the doors and windows in the buildings of past days; you have muddled his ideas and confused his brain, but you have done nothing to form his taste or settle his standards; you have added not one single pet moulding to his tool-chest, nor helped him to pigeon-hole a single familiar feature; he has no lasting impression of any piece of work you ever gave him to do. Had *he* had the origination of the changeful types that have passed before his eyes instead of *you*, he might have retained the same vague sense of things that you have yourself; but, as it is, his memory is no more fixed about the patterns he has worked than the loom which turns out patterns mechanically. He is in for the deluge, and no soft dove comes to whisper hope in his ear. He is the slave of caprice, the plaything of fickle humors, the sport of mutable tastes and veering winds of fashion. What a long dreary jest his life has been, and how, in his sober moments, he must sigh for the blessed, irredeemably bad art of the bad days before the deluge! Yet, in this much-abused,much-misunderstood, much-enduring, unheroic, untrustworthy, misbelieving, self-seeking, wife-beating, drunken, conceited, shallow, *Daily Telegraph*-reading, school-of-art trained man, behold the martyr of the nineteenth century. The Gothic revival proved the winding-sheet of his peace of mind, and one thinks that it had been better for his mental, his social, his moral and religious state, had the modern Gothic architect never been born! Nay, we of the Architectural Association would almost have preferred that he had been left daubing stucco-walls and chasing those curly ornaments and smiling cherubs on tombstones, and making those moulded Jacobean pews that we find so fascinating when we go to study Gothic architecture in some tip-top Mediæval church!

Just think of all the sad, bad, and mad architecture that has passed under the British workman's hand, say, in these last thirty-five years. In 1850 he was rearing a Norman apse upon the ruins of an old chancel that had been destroyed in the interests of morality and purism. In 1855 he was building a thirteenth-century hotel with details cribbed from Salisbury Cathedral, and a bank adjoining it in the Ducal Palace style. This took him some time. Iu 1870 we find him titivating an old Queen Anne

house in a Gothic manner; and in 1880 he was titivating a Gothic house in the Anglo-Foreign "Early English" Queen Anne manner; and now, in this year of the architect's salvation, he is satisfactorily completing the memorial of the nineteenth century at the west end of St. Albans Abbey, under the reputed direction of our all-accomplished, soft-handed, "emancipated," and only true British architect, Sir Edmund Denison Beckett, Q. C. Now, I want to know if we cannot do something to regenerate the art of the builder's yard and to raise the workman's position, and, if no higher motive affects you, think how it is for the interests of the modern architect and his art that you look steadily into this matter and do your best in it? I am firmly persuaded that there will be no good architectural design and no good execution until the craftsman can be brought to participate with the architect in the working out of architectural ornamentation. It is just one of those things about art which marks its divine origin and inherent dignity. You can get faultless mechanical work out of machines, and can get good mechanical work out of human machines; but noble hand-labor is only found where the workman uses his intelligence, and where he is able to express the individuality of the individual. I would say, then, begin the work of regeneration by throwing away all your petty professionalism. Give the workman his rightful participation in your aims. Let him see into your great mind. Make him something more than the transcriber of your hesitating lines. Lift him nearer to your own level of knowledge so that he may know something of the essential qualities of the style he is working in, and may at least interpret your thoughts sympathetically, render in his own idiom the things you put before him, and find some way of escape for the soul within him. Thus, and thus only, will you get good architecture and good sympathetic workmanship. Thus, and thus only, will you effectually and fairly lift some of the crushing load of responsibility and labor from your own shoulders, and get the helpmeets God made for you. The old architect lived long and saw good days because he was thus helped. But Pugin, Scott, Street, and Burges died young, and you know that the doctors say it is worry and not work that kills. This single-handed system of architectural design, where every detail must be supplied from the office, was too much for them. More than this. They were men of singular love of good workmanship, and nothing worried them more than to see their work carried out unsympathetically, or to find their designs carried out to a wrong scale, or their mouldings worked from the wrong side of the sectional line!

I conclude this paper with two propositions which aim at the amelioraton of some of the evils I have here enlarged upon. The first is as to the selection and classification of the architectural types now in vogue. The second relates to the provision of technical education for architects and craftsmen. With regard to the first point, it is clear that no scheme of architectural design has ever been practised without a basis of workshop traditions. Shall we then— is it worth worth while to try and formulate our tentative styles and to systematize our distracted types, with a view to rendering things permanent and to assist the workmen? If so, you must have a grammar and an alphabet before you can form words and sentences. Now it so happens that never since the world began has so much architectural knowledge been accumulated as is now stored up in the brains and on the shelves of the English architect. Why, then, should not these experts be set to work to formulate and render into serviceable shape the leading mouldings and forms and features of the styles in vogue? Why should not the destroyers of old English traditions do penance and make reparation for their naughty deeds, and build up new traditions? Why should we not have a well-arranged series of details of arches, capitals, bases, plinths, friezes, cornices, staircases, doors, windows, &c., for workshop use?

The second proposition is to have a technical college for the instruction of architectural design, to be for the use of architects and craftsmen. If modern architecture showed itself in as attractive form to the public as English music does, the scheme would receive the attention which we who know our pitiful state think that it deserves. One thinks that if the scheme were started under proper auspices it could not fail to receive the support of the Royal Academy, of the Insti-

tute of Architects, of the Architectural Association, and of all other public bodies who have any care for the advancement of the various arts and sciences connected with architecture. If such a college were set on foot, one might feel perfectly secure about the architecture of the future, for it might be expected to bring about that harmonious cultivation of the crafts without which the practice of architecture is a delusion and a snare. Depend upon it the hope of English architecture must come from the workshop, and not from the architect's office. Cast aside, then, as unworthy and profitless, the notion of specialists within or without the profession, and this for your own sake, your heart's sake, and the sake of the art of the future.

Cast aside also the notion that the mere personal taste, learning, theoretical knowledge, or power of penmanship of the architect will avail anything for the real advancement of art, unless the craftsman who works out his ideas reflects his accomplishments and can sympathize with his aims.

What we want is not so much men who can design in many styles of more or less remote antiquity, or men who can sketch well, but men of aim who can lead the aimless, men who by their personal acquaintance with the handicrafts and personal participation in the production of ornamental art can build up new traditions for the workshop, restore the credit of English workmanship, and recover the lost ideal of the English architect.

ON THE INFLUENCE OF SAND ON THE STRENGTH OF CEMENT-MORTARS.

By H. ARNOLD, of Wilhelmshaven.

Translated for Abstracts of the Institution of Civil Engineers.

After observing that not only the quality of the cement used, but that of the sand also, is a very important factor in the composition of mortar, the author remarks that in the case of sand, beyond general vague directions that it must be "clean and sharp," no detailed classification of the characteristics of different kinds, with directions how to obtain in all cases a normal or standard sand of uniform quality, was generally available for practical purposes until the publication of the valuable results of experiments carried out at Wilhelmshaven since 1877, in building the second entrance to the harbor. Taking first the ordinary local (Dangast) sand, a standard or normal sand was obtained from it by washing and sifting in the prescribed manner. This mixed with cement in the regulated proportion of 1 cement to 3 sand, gave a mortar whose tensile strength after seven days' setting was 5.93 kilograms per square centimetre (84 lbs. per square inch), and after twenty-eight days 6.60 kilograms, which does not nearly approach the standard of 10 kilograms prescribed in all contracts. An experiment similarly conducted with Berlin normal sand gave as follows:

After 7 days	10.56	kilograms
" 28 "	15.10	"

being an increase of 43 per cent. in the interval, and 78 and 129 per cent. respectively higher than when Dangast sand was used. Specimens of Dangast sand and cement sent to the Royal testing factory at Berlin gave the following results:

	After Seven Days.	
Dangast sand	11.28	kilograms.
Berlin "	16.80	"
Difference	=49 per cent.	

while experiments at Wilhelmshaven with Berlin sand and the very same cement gave

	After Seven Days. Kilograms	After Twenty-eight Days. Kilograms.
with Dangast sand	9.06	9.74
" Berlin "	14.24	18.44
Difference	=57 per cent.	89 per cent.

The increase in strength from seven to twenty-eight days is therefore 32 per cent. for Berlin sand, and 7½ per cent. for Dangast sand, and from these and other

experiments it is abundantly proved that "the quality of the sand not only exerts considerable influence on the first setting of the mortar, but also materially influences the progressive hardening of it."

Further trials with the same cement, but with the various kinds of sand specified below, were made in order to test the influence of the sand itself.

1. Dangast ordinary building sand, weight, 1.61 kilogram per litre (100 lbs. per cubic foot); size of grain, very unequal and somewhat dusty; not very sharp.

2. Dangast sand No. 2. The same sand after further sifting; a somewhat smoother grain.

3. Dangast normal sand, prepared from No, 1 by washing and sifting. Weight, 1.506 kilogram per litre; grain, reddish and of uniform size; not very sharp. The microscope showed a rounding of the corners of the grains of quartz.

4. Wilhelmshaven common blue sand. Very sharp and extremely fine grain; contains hardly any soluble particles of mud and silt, and weighs 1.267 kilogram per litre.

5. Wangeroog sand. A somewhat coarser, but still a fine, very sharp clean sand, free from dust; weight 1.47 kilograms per litre.

6. Berlin normal sand. A sharp, clean, whitish quartz sand of uniform grain, and weighs 1.547 kilogram per litre.

Mortars made with these kinds of sands, and the same cement, in the regulated proportion of 1 of cement and 3 of sand, gave the following tensile strength:—

	After 7 days. Kilograms.	After 28 days. Kilogms.
Wilhelmshaven blue sand.......	7.15	9.40
Dangast normal "	8.78	11.10
" No. 2 "	8.98	11.04
" common building sand.	11.60	13.00
Wangeroog sand...............	13.58	16.74
Berlin normal sand.............	17.60	21.94

These results show that the strength of mortars similarly made with the same cements, but different kinds of sand, depends on the coarseness and size of the grains of sand; and that in sands of equal size of grain that is the best whose grain is the coarsest. (Compare Berlin and Dangast normal sand in the above Table, where the difference in the strength of the mortar is about 100 per cent.) In order to determine the influence of the size of the grain, comparisons were made with seven specimens of Dangast sand of various sized grains, and also with granite chips, the result always being in favor of the granite chips. This also came out of the experiments, viz., that in sands of equal coarseness of grain that is the best whose grain is largest. (Compare also in the above Table, Berlin normal sand with Wangeroog sand, where the difference is about 30 per cent.) It was further established that coarseness of grain is a more important factor in the quality of a sand than the size of grain; and also that "a coarse sand free from dust gave better results than a fine sand of equal sharpness of grain." And further it was shown that sand containing uniformly sized grains is not always the best, since the ordinary Dangast building sand, which is somewhat dusty, invariably gave more satisfactory results than the sifted and washed Dangast normal sand of uniform size of grain. The author concludes by saying that, although different kinds of sand give materially different results in similarly prepared mixtures of mortar, it will not be justifiable in ordinary masonry to alter the prescribed proportion of cement and sand, viz., 1 to 3, unless the exact quality of the sand employed is known; and he recommends—

1. That the normal sand of the Imperial Testing Station at Berlin should be declared officially to be the only prescribed normal sand as regards not only size, but also sharpness of grain, and that thereby a standard for the strength of cement and sand mortars should be established.

2. That in all notices regarding cement, a comparison of results with this normal sand and with local building sands should be set forth.

3. That both the seven days' and the twenty-eight days' tests should be published, in order to obtain a scale of increase of strength in cement and sand mortars.

OBITUARY.

SAMUEL HENRY SHREVE. — Samuel Henry Shreve, the widely known civil engineer died in this city on Thursday, November 27th. He was known to the members of his profession throughout the country as the author of a treatise on "Bridges and Roofs," the greater portion of which was first contributed to this magazine. It was afterwards translated into French. Another work on the "Theory of the

Arch" was nearly complete at the time of his death.

It is difficult to say whether his greatest skill lay in expounding the principles of correct engineering or in directing their application in the field. In both fields of labor he gained high distinction. He was one of the engineers of the Rapid Transit Commission; was consulting engineer of the Metropolitan Elevated Railway, and chief engineer of the Brooklyn Elevated Railway.

Mr. Shreve was born in Trenton, August 9th, 1829, being the eldest son of Samuel Shreve, and his wife, Mary R. Stockton. Three of his grand-parents were of the latter name. The Shreeves were among the original Lord proprietors of New Jersey, and were distinguished in the Revolutionary War. He was graduated at Princeton in 1848, and was graduated in 1850 from the Harvard Law School and admitted to practice. His mathematical tastes, however, led him to adopt the profession of civil engineer. He was a member of the American Society of Civil Engineers and of the Century Club.

REPORTS OF ENGINEERING SOCIETIES.

Engineers' Club of Philadelphia—Record of Regular Meeting—November 15th, 1884.—Vice-President J. J. de Kinder in the chair.

Mr. John Haug presented a description, illustrated by drawings and test specimens, of Mr. William Astor's new Steam Yacht, the first sea-going steamship built of steel in this country. She is 235 ft. long on load waterline, 30 ft. beam, and 20 ft. deep, and has two complete decks of $\frac{1}{4}$ in. steel plates. Her machinery consists of a compound engine with cylinders 34 and 60 ins. diameter and 36 ins. stroke, supplied with steam of 85 lbs. pressure by 4 oval boilers built of steel, and having 170 sq. ft. grate surface and 4,200 sq. ft. heating surface. Her hull and machinery have been arranged according to Lloyd's rules for a class of 100 A 1, and she has been specially surveyed while building.

Mr. Fairman Rogers described the Steam Yacht "Magnolia," built in 1882, by the Herreshoff Manufacturing Company, Bristol, Rhode Island. The following conditions were required to be fulfilled in her design: Light draft, not more than 4 ft., length and breadth such as to enable her to pass through the Erie Canal locks, which are 100 x 18 ft., flat floor, large accommodations for cruising and for a long residence of the owner's family on board, flush deck, no projection of steam drums, etc., above deck, except the smoke-stack. Low speed, not less than 8 miles per hour. Minimum head room below, 6 ft. 6 ins. Adapted specially for inland cruising along the Atlantic coast. She turned out to be entirely satisfactory. Her ordinary speed is 10 miles, maximum $11\frac{1}{2}$ miles.

The Secretary presented, for Mr. E. F. Smith, an illustrated description of a Floating Coffer-Dam, which is used in removing obstructions from the channels of the Schuylkill Navigation Co. Its dimensions are 32 ft. by 48 ft., and 20 ft. by 36 ft., formed by the union of two halves, which, when placed end to end, could pass through the locks of the canal. This form also enables its easy removal from around a pier after construction. It has water-tight boxes to float it, and to hold the stone to sink it. When in use it is surrounded by sheet piling, and the latter surrounded by puddled clay. It has been worked in from $7\frac{1}{2}$ to 11 ft. of water. It cost $1,128.32 in 1879, has been much used, and is still in good order; 7 men can shift it, complete, in four to six working days. Average cost of four shifts, $86.19, for labor. 5 to 10 per cent. of new sheet piling is required for each shift. A 6-in. Andrews centrifugal pump, run three minutes out of every ten, keeps it clear of water. Rock obstructions are removed by steam drill, and blasting by battery.

Mr. Theodore Bergner, introduced by the Secretary, exhibited his designs for Drawing Boards.

Mr. Robert P. Snowden presented an illustrated mathematical discussion of Formulæ for Frogs and Switches, with some notes as to putting in leads for the same.

The Secretary presented, for Mr. Graham Spencer, a description of the Kaolin Beds of Chester Co., Pa., and Newcastle Co. Del.—describing the methods of mining and refining the clay.

Mr. Allen J. Fuller exhibited a new form of Transit Rod, which is made so that the point of the shoe is in the same plane as the face of the rod There is an opening through the rod, from about 9 ins. above the point of the shoe to a height of 6 or 7 ft. At the top of this opening and on the face of the rod, is suspended a plumb-line, which hangs across a marked cross-piece near the lower end of the opening. The plumb-line indicates when the rod is plumb, after which it may be fastened to a hook on the underside of the cross-piece to prevent oscillation by the wind. The transitman sights to this line, which, with the dividing line on the face of the rod, forms a right line throughout its length, perpendicular to the point over which it is set. The rod is provided with short stay-chains, attached to pins, for driving in the ground, that will hold it when set over a tack center or stone monument. It is also designed to be used instead of plumb-bobs in taking horizontal measurements with the tape-line, to save time and insure greater accuracy.

The Secretary presented, for Mr. R. H. Soule, illustrated notes upon the West Shore Anthracite Engine, No. 24, 8-wheeled, of which class there are 30 on that road. This engine was designed by the late Howard Fry, Member of the Club, Supt. M. P. of that road. It is massive and utilitarian in appearance, all ornamental mouldings and brass work having been studiously avoided. The domes and stack are after English practice, and the cab is unusually large. In performance they have excelled the bituminous engines, which is principally due to the fact that the firebox of the anthracite engine rests on the top of the frames, raising the ashpan higher above the track than is the case with bituminous engines, the fire-boxes of which drop down between the frames.

Mr. W. Bugbee Smith described, with illustration, the method of removal of the West Philadelphia Stand Pipe, which was taken down in 1881, and the masonry and ornamental cast-iron work used in the construction of the Spring Garden Stand Pipe. The new pipe, which is of the same general design as the old, is 5 ft. in diam, and 156 ft. 6 ins. high, being 20 ft. higher than the old. The lower 53 ft. were of $\frac{3}{8}$-in. iron; the middle 50 ft., of $\frac{5}{16}$-in. iron, and the upper 53 ft., of $\frac{1}{4}$-in. iron. The longitudinal seams for the first 53 ft. were double riveted; all other seams single riveted. The bottom sheet was double riveted to a cast-iron flange. The weight of the pipe and flange was about 16 tons. The pipe was erected in one piece, by means of two poles, one on each side of the pipe, and was then lowered about 6 ft., and bolted to the top of the base casting. This operation took about 1 h. 40 m. The base casting is 5 ft. diam., 5 ft. 3 ins. high, and receives the 36-in. inlet pipe on one side. It is secured to the foundation masonry by four $2\frac{1}{2}$-in. bolts, which are keyed on the under side of large plates below the masonry. When the pipe had been secured, the ornamental masonry was built about it, to the height of 87 ft. To erect the ornamental iron work, which consists of columns in 10-ft. sections and a spiral stairway, a carriage supporting two sheaves was placed upon the top of the pipe, where it turned freely. A rope ran up one side of the pipe, over the sheaves and down the other side, by means of which the columns, etc., were hoisted into position. The work was done upon a movable platform which surrounded the pipe, and rested on the rings which joined the tops of the columns. As each section of columns was secured in position, the platform was hoisted ready for the next section.

St. Louis Engineers' Club.—The club is now in excellent working condition, and there seems no reason why it should not so continue. The success of the past year is due to a generally awakened interest in the matter, which can be largely traced to the work of the Committee on Programme, who provided for so valuable a series of papers for the meetings. It is perhaps not too much to say that no local Engineers' organization in this country has done better work in the past year.

The Committee on Programme for the next six months reported through their Chairman, Prof. C. M. Woodward. The report was accepted and the Committee discharged, the duty of executing the scheme devolving upon the incoming Executive Committee. The report is as follows:

St. Louis, December 3, 1884.

Mr. President:

Your Committee on Programme for the coming season beg leave to submit the following report:

Your Committee are aware that they have an important duty to perform. The arrangement of a programme includes the fixing of the dates on which the Club shall regularly meet during the next six months; the acceptance and often the assignment of subjects; and above all the securing of the active co-operation of those members of the Club able to present valuable papers.

They are happy to state that they have found no lack of excellent material, and that, as will be seen below, several papers will be held in reserve.

The programme we submit is as follows:

December 17, "Earlier Floods in the Mississippi," by J. A. Ockerson, U S. Assistant Engineer; January 7, "Economy in Gas Engines," by J. Sobolewski, Inspector St. Louis Gas Co., and "Protection Against Fire and Means of Extinguishing the Same," by C. T. Aubin, Engr. to Board of Underwriters; January 21, "The Use of Compressed Air," by C. Shaler Smith, Engr. Ills & St. Louis Bridge Co.; and "An Improved Crane," by Frederick Shickle, of Shickle, Harrison & Co.; February 4, "Eliminations of Errors in Field Work," by Wm. Bouton, City Surveyor, and "Mill Creek Sewer," by Wm. Wise, Chief Assistant Engr. Sewer Dept.; February 18, "Street Pavements," by Thomas H. Macklind, Chief Assistant Engr. Street Dept., and "Improvement in Switches," by Hubert Taussig, Engr. in charge of St. Louis Depot Yards; March 4, "Experiments in Hydraulics," by Henry Flad, Pres. Board of Public Works," and "Treatment of Wood for Street Pavements," by T. D. Miller, City Gas Inspector, and T. J. Caldwell, Special Expert on Wood Preserving; March 18, "Pile Driving and Related Work," by C. V. Mersereau, U. S. Assistant Engineer," and "The Use of Diagrams of Crank Effort in Designing," by W. H. Alderdice, Assistant Engineer U. S. Navy; April 1, "Amsler Integrators," by M. L. Holman, Chief Assistant Engr. Water Dept., and "Construction in Wood and Iron," by K. Tully, Architect and U. S. Assistant Engineer; April 15, "Steamboat Shafts," by H. W. Baker, U. S. Assistant Engineer, and "The Efficiency of a Pair of Holtz Machines, one Acting as Generator, the other as Motor," by F. E. Nipher, Prof. of Physics, Washington University; April 29, "The Utilization of Fuel by the Generative System," by Wm. B. Potter, Professor of Mining and Metallurgy, and "Dynamometers," by Chas. F. White, Supt. of Shops, Manual Training School; May 13, by Frank H. Pond, of Pond Engineering Co., and "The Theory of Car Starters," by J. B. Johnson, Prof. Civil Engineer, Washington University; May 27, "The Theory of Ammonia Refrigerating Processes," by C. M. Woodward, Dean of Polytechnic School, Washington University, and "Report of the Committee on Smoke Prevention."

Several papers have been half promised and are held in reserve for cases of unavoidable failure, and for additional meetings should the Club see fit to continue semi-monthly meetings into the summer. Your Committee recommend that this Programme, when adopted be printed as a circular and distributed among the members of the Club, and other interested persons.

ENGINEERING NOTES.

Cost of Carriage by Rail and Canal.—A canal from the Westphalian coal district

to Emden being projected, the author compares the cost of carriage upon canals and on a single line mineral railway with a few stations and a small staff. Assuming eight trains of sixty loaded wagons per day to the port, of which twelve are returned loaded, and a cost of £6,000 per kilometer for building the line, as actually incurred for similar lines in the district, he calculates the cost per train-kilometer as follows:—

	d.
Repairs and renewals of locomotives...	1.20
Fuel...	2.40
Cleaning, oil, &c....	0.54
Repairs and renewals of wagons.......	2 88
Lighting and heating of guard's van....	0.22
Drivers' wages, including mileage......	1.41
Guards' and brakesmen's wages, including mileage ...	2 46
Inspection, &c., of rolling stock........	0.13
Station-service...	3.12
Permanent-way, repairs, and signalmen.	4.32
General management...	1.56
Interest on capital account for line, locomotives, and wagons, at 4 per cent...	14.52
Total...	34.56d

or $\frac{34.56}{3.60} = 0.096d.$ per ton-kilometer = 0.16*d.* per ton-mile.

The carriage on the Elbe canals costs 0.35*d.* per ton-mile, and on the canal from the Belgian coalfields to Paris 0.29*d.* in spring and 0.34*d.* in autumn 1883, without paying interest. Estimating the cost of the Ems canal at £16,000 per mile, and adding 4 per cent. of this, the carriage on the canal would cost about 0.4*d.* per ton-mile. The items in the above estimate are calculated from the actual returns of similar German lines.—*Abstracts of Inst of Civil Engineers.*

Protecting River Banks from Caving.—A novel system of protecting river banks against the consuming action of an everflowing current is being applied near Memphis, Tennessee, along the Mississippi, where a caving bank rises straight up from the water's edge from 10 to 50 feet. There is an incessant lapping and chafing by which the bank is slowly worn away and undermined, and, as a consequence, it breaks down piece by piece, and is dissolved and carried away by the river. To check this steady but slow disintegration is the problem which United States engineers are trying to solve satisfactorily. The idea of a blanket placed along the slope of the bank from high-water mark to the bed of the river naturally suggested itself, and the contrivance adopted, a willow and pole mattress, represents the blanket theory. The woven webs are some 50 feet wide and from 200 to 1,000 feet in length, with flexible willows worked in for woof and poles and wire for warp. These are made on boats having a length equal to the width of the mattress, and as the mattresses are completed they slide away into the water. The sunken mattresses, it is said, prevent undermining below the low-water line, and the grading down of the over-hanging bank stops the undermining above that line. The space between the upper edge of the mattresses and the top of the bank is protected with willows and stone. All this mattress-grading and stone-covering is embraced in the term revetment. The work already done by the American engineers and the staff of workmen is described as being of the most substantial character, The appropriation of $200,000 secured from the last Congress for this work will, it is estimated, be sufficient to place mattresses along the river front from Wolf River to the foot of Beal or Linden Streets. The work will probably be completed before the next rise of the river. It remains to be seen whether it will hold the bank and prevent it from further caving. Next spring, when the floods turn the Mississippi into an inland sea, the practical test will be made, and if the mattresses hold the banks successfully against the impinging and undermining current, the mattress revetment theory will be sustained.

IRON AND STEEL NOTES.

In an article in *Dingler's Journal* "On Utilization of Slag," Mr. A. Frank recommends the application of magnesium chloride for the decomposition of slags containing sulphur and phosphorus. The fluid slag is run into a solution of about 1.06 sp. gr. and agitated; the sulphides are decomposed with the evolution of hydrogen sulphide; so in basic slags uncombined lime produces calcium chloride and magnesia, which indirectly induces a concentration and more easy solubility of phosphates present. The magnesia thus produced can be removed by washing and settling. On heating in an oxidizing flame the slag powder obtained with the still adhering magnesium chloride, or with a further addition of chloride, a partial higher oxidation of the ferrous oxide and similar compounds results, the new compounds formed being less prejudicial to the manure obtained. Instead of beginning with fluid slag, solid slag finely ground can be heated with the magnesium chloride solution under high pressure. In place of magnesium and ammonium chlorides, the sulphates can be used along with free hydrochloric acid.

By a modification of Rocour's process for working up phosphorized slags, described in *Ding. Polyt. Journ.*, the slag is melted in a cupola, whereby a matt is obtained containing 20 to 25 per cent. of phosphorus. It is then mixed with powdered anhydrous SO_4Na_2, and heated to redness. Most of the phosphorus is changed into sodium phosphate, whereas a portion of Fe and Mn is converted into phosphates, sulphides and oxides. The mass is treated with water to recover sodium phosphate by crystallization. The insoluble residue is mixed with Na_2SO_4 and charcoal, and heated in a reducing flame. The Na_2SO_4 is first converted into Na_2S, and then by double decomposition sodium phosphate and FeS and MnS are formed. The mass thus yields another crop of sodium phosphate crystals. The residue, after roasting to destroy the sulphides, can be used as an iron ore rich in Mn. The sodium phosphate is employed for artificial manure. Another method to work the phosphorized matt is to fuse it in a Bessemer converter with dolomite or lime. Alkali can be added to promote the fusing of the

metal slag which is formed. Before the complete dephosphorization the slag is decanted, and a fresh portion of lime added to obtain the dephosphorization according to the basic process. The slag contains P_2O_5 and only little Fe and Mn. It is powdered, and used either directly as manure or after treating with SO_4H_2 as superphosphate. The second method yields the P as a product of less marketable value; but as the metal has been converted into steel, its value is said to make up the difference.—*Engineer.*

RAILWAY NOTES.

Railway Progress in New South Wales.—In 1883 the total earnings of the New South Wales railways were £1,931,464; total working expenses, £1,177,188; and the net earnings £753,676. The total cost of construction amounted to £14,382,102; the cost of rolling stock, workshops, machinery, furniture, &c., was £2,520,912; and the total capital expended £16,905,014. But for heavy reductions of charges, the receipts would have been increased by £100,000.

Steam on London Tramways.—A satisfactory trial has taken place of one of the fifteen steam tramway locomotives now being constructed by Messrs. Merryweather & Sons, of Greenwich, for the North London tramways. These engines have cylinders 7½ inches diameter by 12 inches stroke, and are each capable of drawing three loaded cars at a speed of eight miles per hour, and at a stated working cost of 30 per cent. less than horse-power. It is expected that the whole of these engines will be running in the course of the next two months.

The extension of the Brighton electric railway line having now been in active operation for six months, a few particulars may be interesting, as showing the capabilities of a light line of this description; the details of construction have been already described in our columns, and need not, therefore, be repeated. The car mileage amounted to 15,600 miles, or 100 miles per diem, the number of passengers in round numbers, 200,000; this figure would be increased but for the fact that at certain times the would-be passengers exceeded the capabilities of the car, which seats thirty persons only. No accident has occurred to either the general public or to passengers by the car. The consumption of gas in the gas engine has been 300,000 cubic feet, or 13 cubic feet per passenger per mile. The total cost of traction, including interest and depreciation on engine, dynamo, and motor, cost of gas, oil, and attendance, has amounted during that period to 15s. 6d. per day—100 miles run—say 2d. per mile. The car service has only been stopped for one day through the tires of the wheels giving out, owing to the heavy pressure of the holiday traffic, there being at the time no second car available.

ORDNANCE AND NAVAL.

Improvements in Torpedo Appliances.—A noteworthy improvement in torpedo appliances on board torpedo craft has just been perfected at Portsmouth after a course of practical experiments under way in the harbor at Spithead. Hitherto the attack of outrigger charges has been delivered from the front by means of a spar, 35 feet in length, which was run out over the stem of the boat, and exploded from the conning tower after the charge had been submerged to the requisite depth. From a tactical point of view this method had the obvious disadvantage of arresting the way of the craft at the critical moment, and of seriously retarding her retreat after having accomplished her purpose. The cause, however, which has brought about the improvement was less tactical than mechanical. It was found that when the pole was lowered for action it almost invariably snapped off short by reason of the resistance of the water when running at high rates of speed. Wood was superseded by steel tubular booms, but without any advantage, the metal, besides being more cumbrous and expensive, proving just as brittle and uncertain under the like conditions. In these circumstances recourse was had to a device which was suggested by Captain Noel some ten years ago, but which was at the time reported against as impracticable. This was to deliver the attack from the side by means of a swinging pole, which would give way gradually under the pressure of the water. One of the new poles has now been fitted (according to the *Times*) to No. 13 first-class torpedo-boat at Portsmouth, under the superintendence of Mr. Gowings, by whom all the initial difficulties suggested by the officers of the *Vernon* have been satisfactorily overcome, and the gear will become a service fitting for all torpedo craft as well as the swift boats which are now in course of construction by Mr. J. S. White, of East Cowes. The new pole is made of wood, and is fixed by a swivel aft. Its length is 45 feet, or 10 feet longer than those hitherto in use. It has an arc of safety ranging from 30° before to 30° abaft the beam; and by reason of an ingenious contrivance, though the officer in charge might, in the hurry and confusion of the moment, move the firing-key before the charge had traveled to a safe distance from the boat, it will not explode until the indicator shows that the limit of danger has been passed. It was also necessary to provide against the chance of the head of the pole being forced under the boat carrying it through coming against the protective nets and booms of the enemy, and the torpedo being fired while in that position. This danger has been met by preventing contact being made after the charge has been sunk below 20 feet. It has likewise been thought expedient to adopt precautions against the risk of premature explosion by the production of "an earth current," by the overflow of the sea.

Artillery Experiments at Shoeburyness.—Some interesting artillery experiments were made on May 27, at Shoeburyness. The principal feature of the day was the trial of a new compound armor plate constructed by Messrs. Cammell & Co. It was 10 feet 8 inches wide by 9 feet high, and 19 inches thick,

$6\frac{3}{4}$ inches being steel and the remainder wrought iron. It was arranged that the 80-ton gun, with a charge of 450 lbs. prism powder and a Palliser shell of 1,700 lbs., should be fired at the plate at a distance of 120 yards, and that should this shot, as was anticipated by some, not succeed in breaking it, four other shots from an 11-inch breech-loading gun of 43 tons should be fired at the four corners of the plate. The plate was fixed by sixteen bolts to a backing of $11\frac{1}{2}$ inches of teak, constructed on a frame identical to that designed for Her Majesty's ship *Camperdown's* target at the water-line, the only difference between the *Camperdown* and the target erected being that the teak backing in the former is 19 inches thick, whereas in this it was only $11\frac{1}{2}$ inches. The plate also in this case was 19 inches instead of 16 inches. The gun having been loaded, the company present retired under shelter and the shot was fired, striking the target, as was calculated, with a velocity of 1,594 f. s., and with a total energy of upwards of 30,000 foot tons. The result of the experiment was that the projectile penetrated about 8 inches into the plate and was broken to atoms. The armor plate was radially split into five different pieces, one of which was entirely detached. Behind the backing nine of the sixteen bolts were found to have been broken, and the iron frames representing the ship's side were bent and torn. To account for this result it was claimed, no doubt with some truth (says the correspondent of the *Times*), that the framing of the target to which the plate had been attached was too weak in its structure, and from want of support it had buckled considerably from the impact of the shot. The fractures showed that the welding of the steel face to the iron back was perfect throughout, and was an excellent specimen of manufacture so far as soundness was concerned. After the satisfactory results which had on former occasions been obtained in firing against a compound armor plate with a granite backing, the experiment on May 27 was no doubt disheartening. Consequent, of course, upon the shattered condition of the plate, it was not possible to carry out the remaining portion of the experiments with it, and the rest of the day was taken up with some interesting practice with the various guns which had been mounted. Shots were fired from a Hotchkiss quick-firing gun constructed for sea forts, but owing to the manufacture of the cartridges not being very satisfactory the practice was only moderately good. Experiments with the Hotchkiss quick-firing gun for naval service, mounted on a pedestal, were made, and it was found that the gun was capable of being fired with aim once in every $4\frac{1}{2}$ seconds. One hundred of these guns have been ordered for the service, as well as one hundred of Nordenfelt's quick-firing 6-pounders; they are intended to be used as an auxiliary armament to ships. A 12-pounder breech-loading gun was next shown, mounted on a hydraulic Woolwich field carriage, which has been constructed in competition with one made by Messrs. Armstrong. The trail of the Woolwich carriage is lighter than Messrs. Armstrong's; the recoil averages on ordinary ground nearly 6 feet. Three of these carriages have been constructed for experiments at Okehampton, and eighteen of Messrs. Armstrong's similar carriages have been ordered and will be served out for practice. Messrs. Armstrong's 18-pounder breech-loader, which has been made to take the place of the old 16-pounder, was shown. It was mounted on an Armstrong steel field carriage, with a brake, which, however, was found not to work very satisfactorily. After luncheon a 5-inch breech-loading sea-service gun was shown, also a 6-inch breech-loader and an 8-inch breech-loader, the former being mounted on an Armstrong carriage with friction compressor, and the latter on an Armstrong converted carriage with a hydraulic buffer. The practice of these three guns was at 2,000 yards, at 6 feet by 6 feet targets. The skill with which they were laid was highly creditable, and the action of the fuses was satisfactory.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

SELECTED Papers of the Engineering Society of the University of Michigan; First and Second Series.

An Ephemeris of Materia Medica, Pharmacy and Therapeutics. By E. R. Squibb, M. D.

Abstracts of Papers in Foreign Transactions and Periodicals. Edited by James Forrest, Secretary of the Institution of Civil Engineers.

The Journal of Microscopy and Natural Science.

Transactions of American Institute of Mining Engineers, Vol. 12.

Report of the Board of State Engineers of the State of Louisiana for 1882–83–84

Sound Signals. By Arnold B. Johnson. New York: D. Appleton & Co.

The Magazine of Art for 1885.

Cassell's Family Magazine. New York: Cassell & Co.

Professional Papers of the Signal Service. No. XIV., Charts of Relative Storm Frequency of a portion of the Northern Hemisphere.

PROPOSED PLAN FOR A SEWERAGE SYSTEM AND FOR THE DISPOSAL OF THE SEWAGE OF THE CITY OF PROVIDENCE. By SAMUEL M. GRAY, City Engineer. Providence: Providence Press Company.

This report is an exceedingly well printed and well-illustrated book of 146 pages of text, and many good maps. As most of the volume is a carefully prepared treatise on Sewerage Systems and treatment of Sewage in general, it will prove to be a valuable reference book for the practical engineer.

MANUAL OF GEOLOGY—THEORETICAL AND PRACTICAL. By JOHN PHILLIPS, LL. D., F. R. S. Edited by Robert Etheridge and Harry Govier Seeley, F. R. S. London: Chas. Griffin & Co. Price $6.50.

This work appears in two parts. Part I. is devoted to Physical Geology and Palaeontology, prepared by H. G. Seeley, F. R. S.

By far the greater portion of this volume relates to Physical Geology, and includes one of the most satisfactory treatises on Lithology in the English language.

The illustrated examples are mostly those of British Geology, and, with the exception of a few new ones prepared for this edition, they can hardly be regarded as embellishments.

So much is presented in this treatise that is not in other accessible books that students of Geology cannot well afford to be without it.

Applied Mechanics. An Elementary General Introduction to the Theory of Structures and Machines. By James H. Cotterill, F. R. S. London: Macmillan & Co.

This may be regarded as a complete elementary treatise on practical mechanics.

The several sections treat, respectively: Statics of Structures, Kinematics of Machines, Dynamics of Machines, Stiffness and Strength of Materials, and Transmission and Conversion of Energy by Fluids. These five sections are divided into twenty-one chapters.

Examples for practice are given at the end of each chapter. The illustrations are numerous, and tolerably good.

Students who desire a work presenting all the subjects of Applied Mechanics in a form easily mastered by moderate mathematical attainments will find their wants well supplied by this book.

The Seaman's Guide to the Law of Storms. By W. H Rosser. London: Norie & Wilson. Price $1.00.

A compact little essay on the Law of Storms is here presented in twenty-eight pages of text, which are made also to include the few necessary wood cuts. It may be regarded as a text book for seamen. A collection of examination questions is furnished at the end of the volume.

Although not designed for general reading, this book is well calculated to satisfy the want of the general inquirer who seeks to become familiar with the practical side of the growing science of Meteorology.

Electricity in Theory and Practice. By Lieut. Bradley A. Fiske, U. S. N. New York: D. Van Nostrand. London: F. & N. Spon. Price $2.50.

This book is written with the object of forming a connecting link between the many works written on the theory of electricity and those written on its practical applications. The author states in his preface that practical men find great difficulty in mastering this subject, because they have to study the theory of electricity from books devoted wholly to theory and the practical applications from those devoted wholly to practice, and he endeavors in the present work to remove this difficulty by considering the theory of electricity in connection with its practical applications. The book opens, after an elaborate table of contents, with a chapter on magnetism, in which only the leading facts and phenomena are touched upon, and then the author proceeds to the consideration of frictional electricity. There is nothing novel in the treatment of this part of the subject; we notice the customary engravings of the various machines, better executed than is ordinarily the case, and a very well written description of the Holtz machine. The idea of potential, which is usually so incomprehensible to students, is very lucidly explained, and the chapter on voltaic batteries is extremely full and complete. The discussion of Ohm's law in the following chapter leads us to the consideration of quantitative measurements, and this is followed by an explanation of the practical units in which currents are measured, and examples illustrating their use and showing how the current varies according to the grouping of the cells.

Electro-magnetism is very clearly dealt with in chapter eight, and the convention of magnetic lines of force is rendered very easy of comprehension by excellent diagrams. An account of electrical measurements is preceded by a description of the sine and tangent galvanometers, and we must point out that in his discussion of the principle of the sine galvanometer, Mr. Fiske has made use of ambiguous terms, which would probably leave a wrong impression on the mind of the student. He refers to the couple tending to pull back a deflected magnetic needle into the meridian as being measured by the product of the earth's magnetic force, the strength of one pole and the distance between them, whereas he should have been careful to point out that it is the horizontal component only of the earth's magnetic force that enters into the couple. It is a defect in the chapter on magnetism that there is so little information in it on the subject of the earth's magnetism. Several forms of ampere meters and voltmeters are excellently described in this chapter, and the manner in which the engravings are executed is beyond all praise. The account of telegraphy in the following chapter gives a description of all the systems of working commonly in use, and is a most interesting and instructive article.

The chapter on the electric light possesses no particularly novel feature, and we are somewhat disappointed in not finding some practical information on the subject of installations and methods of working.

Electric machines receive a fuller treatment than is to be found in most text books; there is a splendid folded engraving of the Weston machine, and also smaller ones of the Brush and Siemens machines. This chapter also contains a discussion of the characteristic curves of dynamos, which is a valuable addition. These curves, first suggested by Dr. Hopkinson, offer a graphic representation of the relations of the strength of current to the electromotive force, and therefore to the magnetization of the magnets. Under the heading of electric distribution of power, the author discusses the problem of the transmission of power in the form of electric currents, and gives a series of tables taken from the *Electrical Review*, London, showing the amount of horse-power lost per thousand yards, in conductors of different sizes, with currents of different strengths. There is also a full account of the various sytems of electric distribution, by which power may be distributed with a comparison of their relative advantages and disadvantages This

chapter may be considered as the principal feature of the book, for in it the author departs from the ordinary routine of the text book, and applies the theory which has been studied in the previous chapters to the consideration of problems of a more practical nature.

The book concludes with an account of electric railways, in which the two systems for propelling cars by electricity, viz., firstly, by a storage battery carried by the car, and, secondly, by a conductor which transmits the electrical energy to the motor, are dealt with. The difficulties that arose in the solution of the problem of applying electricity to locomotion, and the way in which they were surmounted, are fully explained and the student is thus made conversant with all that is at present known on the subject. As a scientific text book this work is sure to find considerable favor amongst students and teachers; the reasoning is at all times clear and explicit, and the general arrangement of the book leaves little to be desired. To the student who is desirous of becoming an electrical engineer, it will be a most valuable assistance, as it goes beyond the point at which most books leave off, and shows how theory is applied in practice. We have already had occasion to note the excellent engravings with which the book is illustrated, and we will only add further that it is got up in a manner which we regret to say is seldom seen in this country. —*Engineering*.

ATTACK AND DEFENCE OF COAST FORTIFICATIONS. By CAPTAIN EDWARD MAGUIRE, Corps of Engineers, U. S. A. D. Van Nostrand. Price $2.50.

As Captain Maguire shows, the changes of the last generation have not lessened the importance of fortifications as the main defence of coasts and harbors, however much they may have enlarged the role and increased the importance of auxiliary defences, such as submarine mines and movable torpedoes. More powerful guns will be required to keep hostile vessels off at longer range, and heavier works will be required to accomplish the same result. Stone must give place to iron in casemated batteries, and earthern parapets at least forty feet in thickness should be provided for open unarmored batteries which are still useful in localities sufficiently high above the sea level, say the level of the maintop of war vessels, and in no case less than twenty-five feet, in order not to be commanded by the turrets of armored ships. Depressed carriages must be used in such batteries to secure protection for the gunners while loading, and mortar fire must be developed to the utmost, to assail armored vessels at their weakest point by a vertical fire. Magazines must be sunk quite below the *terreplein* to protect them against the penetrating power of projectiles. Machine or rapid-firing guns in abundance must be added to the armament of all open batteries to worry the gunners, as well as the man at the wheel, and the commander in the conning tower, while heavier guns are piercing or racking off the armor. Channel obstruction, fixed and floating, becomes increasingly important, owing to the great speed of war ships, which would, without it, carry them speedily out of the range of slow-firing, heavy coast guns. These sevearl necessities of the modern defence are clearly set forth by Captain Maguire. Submarine mines can never, he argues, replace guns. The mine has but one life, while the activity of the gun is limited only by the supply of ammunition, and the mine must be operated by the expert, while the guns can be fought by the multitude and without very great training. Captain Maguire's book does not profess to be anything more than a compilation, but it is a very useful compilation, bringing together a mass of facts which have outgrown the received text-books, and are only to be found scattered in the current periodical literature, and presented in a variety of languages. The German, Austrian and French text books treat the subject of modern coast defences in a very general way, and the information they give is limited. The English text book devotes thirteen pages to a brief review of the subject, and that from a purely English point of view. Professor Mahan, in his Permanent Fortifications, gives but little attention to coast defences, and his work is in any case out of date. The latest text book that covers this subject in any measure is the new edition of Professor Wheeler's text book on Military Engineering. In this the subject of coast defences is treated generally in a chapter of nineteen pages. None of the works to which we have alluded consider naval operations against coast defences and the fighting strength of fleets, subjects which are of the first importance in determining the character, location and armament of coast works. Hence this work of Captain Maguire will be found to be an excellent supplementary and companion volume to the received text books on the subject of the art of fortification, such as that of Professor Wheeler, which is, of course, much wider in its scope. Professor Wheeler's purpose is to give a clear statement of the general principles and rules observed by engineers in the construction of modern fortifications, thus guiding the student to the intelligent study of the development of the art of fortification, and the history of the various systems employed in the past. As it is a book for class-room instruction details are omitted, so far as may be, explanation and instruction being left to the instructor. At the same time Professor Wheeler's work is one which may be read or studied with profit by those who, without making arms their profession, are desirous of keeping themselves informed as to the principles and progress of the military art, which, in its relations to the defence of States, becomes of interest to every citizen. It is not to be expected that either Professor Wheeler's work or that of Captain Maguire will reach the circulation of dime novels, but the more widely they are read and studied the firmer foundation we shall have for an appeal to popular judgment on the subject of providing against the possibilities of national humiliation through foreign aggression. Our national lack of reverence displays itself, among other ways, in indifference to, or ignorant contempt for, the opinions of those whose life-long study of a subject of national moment entitles them to be listened to with respect

when they call attention to our public danger and its remedy. As an intelligent and enlightened member of the Democratic party, Mr. Dorsheimer well said, during the last session of Congress, from his place in the House, "You may say the conditions of Europe are impossible to America. But these very streets we look upon have heard the cannon of Bladensburg. They have beheld the bayonets of General Ross. History repeats itself. It is bound to do so again. It may come upon us as a thunderclap. And, for one, I do not desire to be identified, within or without partisan boundaries, with any party that squarely sets its face against the defence of our country from the probable insult or the possible invasion of a foreign enemy."—*Army and Navy Journal.*

MISCELLANEOUS.

Righting a Chimney.—There are various ways of righting a tall chimney which has inclined out of the perpendicular. The method pursued recently in the case of the chimney of Messrs. Pattee & Perkins' machine shop at Holyoke, Massachusetts, displays several novel features. The chimney in question is 80 feet high, 8 feet square at the base, and 6 feet at the top. Three "harnesses" were put on the base, the first one being placed under the cornice, and the other two below the first. The chimney leaned to the north of west, and was 42 inches out of the perpendicular. Two lever jackscrews were placed under the girders of one of the harnesses on the west side and six jackscrews under the harnesses on the north side. The earth was then removed on the south and east sides to below the foundation of the chimney. Then the earth was carefully combed and loosened with iron rods under the south-east corner. Water was next forced into the loosened earth by means of hose. Finally the jackscrews were turned up slowly and carefully, and the chimney was forced to an upright position after repetitions of the loosening, puddling, and turning of the screw. When the work of righting was finished, the earth on all sides was puddled, and the chimney is now said to stand pointing towards the zenith.

According to an article "On the Decomposition of Cements by Water," by H. Le Chatelier, in the *Chemisches Centralblatt*, he has studied the progressive decomposition of cements by water. Hydrated cements when treated with excess of water give up lime; it has hitherto been supposed that the dissolved lime was free lime, and it was determined in this manner, hence the varying results obtained in different laboratories. These amounts are proportional to the water used, the calcium salts ceasing to be decomposed when the water contains a certain percentage of lime. The free lime may, however, be determined by solution by using very little water at a time, and only removing it on becoming saturated—1.3 grms. CaO per liter. In this manner no calcium compounds will be decomposed, calcium ferrite, the least stable compound of all, only beginning to decompose when the solution contains about 0.62 grms. CaO per liter. It was found in this manner that the slowly hardening cements always contain a large amount of free lime, whereas the quick setting are almost free from it. By the progressive action of water each of the constituents is decomposed in turn, giving a particular amount of lime per liter in the water, which amounts remain constant for each lime compound decomposed. The question cannot be completely answered, as in the solution of the lime there are certain stopping places, corresponding to which there have been as yet no lime compounds prepared synthetically. The author is therefore inclined to the opinion that silico-aluminates and silico-ferrites are formed in hardening, although he has not as yet succeeded in preparing them artificially.

New Form of Primary Base Apparatus; by T. W. Wright.—In Wright's *Treatise on the Adjustment of Observations* (D. Van Nostrand, New York) there is a description of a new form of primary base apparatus. The measuring bar is of metal packed in melting ice. The length of the bar will remain unchanged throughout the measurement, as its temperature is constant, being that of melting ice. The same temperature can at any time be had at which to find the length of the bar in terms of the official standard of length. The apparatus might be constructed as follows: The measuring bar, a bar of steel 25^{mm} in diameter and 6^{m} in length, placed in a circular cast-steel tube $\frac{1}{8}^{m}$ in diameter, made stiff by bracing, but as light as possible. Along the top of this tube slots of about 75^{mm} in width would be cut to allow the introduction of ice around the bar. The hole for drainage would be at the center of the tube on the under side. For supports during the measurement two trestles placed $1\frac{1}{4}^{m}$ from the ends would be best. Effects of flexure would be got rid of by having the graduation marks showing the length of the bar placed on the neutral axis of the bar. The reading microscopes, alignment apparatus, sector and level for determining the inclination of the bar during measurement, such as those made by Repsold for the U. S. Engineers. The mode of measurement the same as with the Repsold apparatus. The amount of computation necessary to reduce the measurements made in this way would be small in comparison with that required with the forms of apparatus at present in use. The scheme is entirely practicable in the United States, at least where ice is to be had everywhere at all seasons.—*American Journal of Science.*

Correction.—On page 362 of our last volume (Nov. No.), in 9th and 10th lines of the second column, there is a fraction whose denominator is printed 3^{o}. It should have been 3^{a}.

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VAN NOSTRAND'S

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THE REYNOLDS PUMPING ENGINE.

By JOHN W. HILL, M. E.

Contributed to Van Nostrand's Engineering Magazine.

By permission of the Hon. Mayor and Board of Aldermen of the City of Decatur, Ill., and of the contractors, E. P. Allis & Co., of Milwaukee, Wis., I am permitted to publish the following *résumé* of the results of a recent test trial of one of a pair of novel pumping engines recently erected for the water supply of that city.

The engines (two in number) are horizontal, of the single cylinder, condensing type, the steam and water ends in line, and the main shaft mounted in bearings on the top of air chamber, between the steam and water ends, a working lever or half beam turning on a center at the lower end and below the center line of engine, takes motion through a pair of short links connected with the steam end cross-head. Besides the pins on the beam to which the short links are connected, another pin, set back to describe an arc tangent to a center line from crank shaft to center of pin, takes the inner end of the main connecting rod, while the other end is attached to the crank. The crank is bent in the shaft midway between the two bearings, and to each of the overhung ends of the main shaft a heavy fly-wheel is keyed. Although the piston rod of the steam end and the piston rod of the water end are in line, each has its own cross-head, traveling on flat ways or guides in the engine framing, and connected together by horizontal struts; the space between the cross-heads and the struts being occupied by the upper end of the working lever and the short links which connect it with the steam end cross-head.

The pins receiving the links are overhung on each side of the working lever, and the pin receiving the inner end of the main connecting rod is supported in jaws central to the overhung pins, and over the center line of engine.

Owing to its novelty it is difficult, in the absence of a skeleton view or elevations of the engines, to make an easily comprehensible description of it. But it is sufficient for present purposes to have it understood as a horizontal direct-acting, single-cylinder, condensing pumping engine, with crank and fly-wheel mounted on the air chamber of pump, above the center line of engine, and connected with a pin on the working lever, which pin describes an arc tangent to a line drawn from the center of pin to center of crank shaft. The steam cylinders and heads are steam-jacketed, the condensation from the jackets being trapped into the hot well.

The pumps are of the double-acting piston variety.

The exhaust steam is led from the cylinder into a surface condenser with vertical tubes, around which the suction

is drawn from the well to the pump, the shell of the condenser being a part of the suction pipe. The circulation is, therefore, the water of supply on its way to the pump. From the lower end of the condenser the condensation is drawn off through a single-acting air pump, driven by an arm on the working lever shaft; a hot well in the line of the overflow pipe from the air pump receives the boiler feed water, from which a single-acting plunger pump, driven by an arm on the working lever shaft, forces it into the boilers.

The valve gear to the steam cylinders is of the well-known Corliss type, with Mr. Edwin Reynolds' improvements. The engines are finished with the Reynolds' style fly-ball governors, which regulate the cut-off and speed of engines automatically.

The speed at which the governors regulate ranges from fifteen to forty revolutions, according to the adjustment of a counterpoise spring which balances more or less the normal weight of governor balls. Each engine is complete within itself, and has no working connection with its duplicate.

The engines, with the exception of a slight difference in the diameters of the steam cylinders, are precisely alike in style and dimensions, and, by mutual agreement of the city and the contractors one engine was tested for duty under the terms of contract. For this purpose engine No. 2 was first selected for the trial, but upon discovering a leak in the steam trap connected with the jacket, engine No. 1 was tested instead.

The engines are of the following dimensions:

	Inches.
Diameter of steam cylinder No. 1.....	22.0625
Diameter of steam cylinder No. 2.....	22.1250
Diameter of pump cylinder No. 1.....	11.
Diameter of pump cylinder No. 2.....	11.
Diameter of steam piston rods........	3.625
Diameter of water piston rods........	3.1875
Stroke of steam and water pistons.....	36.

The engines take steam from two five-flue boilers, set with separate furnaces, and with steam and water connections common to both.

The terms of the contract, under which the engines were furnished, guarantee a duty of eighty million foot-pounds of work per hundred pounds of coal, upon an evaporation of ten pounds of water (to steam) per pound of coal. The economy depended, therefore, not upon the weight of coal burned under the boilers, but upon the weight of steam delivered to the engines, and as there could be no material variation of the quantity of steam consumed from hour to hour, under constant conditions of steam pressure, water load and speed, it was decided to limit the trial to sixteen hours' duration, during which time the steam and water gauges, vacuum gauge, water levels in boilers, temperature of feed water, and revolution counter were read at intervals of fifteen minutes. The temperature of the circulating water to and from the condenser was read hourly. Indicator diagrams were taken at regular intervals of fifteen minutes from 9.00 A. M. to 6.30 P. M., and afterwards at random to the termination of trial.

During the trial the connection between the hot well and the boiler feed pipe was closed, and the discharge of the air pump wasted through the overflow pipe. The feed water for the boilers was drawn from the force pipe and weighed in uniform charges of four hundred pounds, in a tight cask mounted upon an accurately adjusted Fairbanks platform scale. The suction of the boiler feed pump was provided with a vertical supply pipe, with a deep tin funnel at its upper end, into which the water from the weighing cask was discharged.

The rate at which the water flowed from the weighing cask into the suction pipe of the feed pump was the rate at which the water was supplied to the boilers, and with a uniform level of water in the boilers was the rate at which steam was consumed by the engine. The glass water gauges in the boilers were fitted with sticks graduated to quarter inches, and these were read and reported regularly every fifteen minutes, and frequently between the regular observations to govern the rate of delivery of feed water from the weighing cask.

In view of the fact that the contractor assumed no responsibility for the boilers, and based his guarantee of duty upon a hypothetical consumption of coal, it was held that he was entitled to saturated steam, and that from the water weighed to the boilers should be deducted the water of entrainment, if any was carried over in the steam.

The setting of the boilers was such as to prohibit superheating of the steam, and a material entrainment was, with reason, suspected before going into the trial.

The main steam pipe was tapped near the engine for calorimeter purposes, and samples of steam were drawn for tests of quality at least twice during each hour of the trial.

The great uniformity of steam pressure, load and speed, and of the rate of firing and water levels in the boilers, rendered more frequent tests of the condition of the steam unnecessary.

The scale upon which the condensing water and water of condensation were weighed, was graduated to quarter pounds, and would weigh to eighths.

The cask of wood was tight, and the thermometer used was a signal service instrument of James Green's manufacture.

The steam gauge and water gauge at the engine were read as found, and were compared with a new test gauge subsequent to the trial with the corrections noted.

The engines pump from a well connected with the Sangamon River by 105 feet of 24-inch pipe, and 190 feet of 22-inch pipe. At the junction of the 22-inch and 24-inch pipes a filter is interposed, which, however, is not effective, owing to the manner of connecting the filter with the river.

Previous to the trial, levels were taken of the surface of water in the Sangamon, of the upper edge of a spike driven in the side of pump well about one foot above the surface of the water, of the engine-room floor, of the center of pump cylinder, and of the center of water-pressure gauge from which to estimate the rate of inflow to the pump well through the inlet pipes, the suction lift as measured from the center of the pump cylinder to the surface of water in the well, and the head represented by the elevation of the water pressure gauge above the center of pump cylinder.

The distance from the upper edge of spike in the side of pump well to the surface of the water was measured once every three hours of trial; similarly the variation of the level of water in the river was noted.

Although under the terms of the contract the actual consumption of coal was not a factor in estimating the duty, the coal was weighed to the boilers for the purpose of determining the evaporation during the trial, and of stating to the city the actual performance of the boilers.

As the engines are usually operated, the condensation from the cylinder jackets and heads is trapped to the hot well and pumped back into the boiler; but, for the purpose of estimating the consumption of steam in the jackets the condensation was caught in a gauged vessel, its temperature and rate of delivery noted, and the jacket water wasted during the trial.

The trial for duty began at 8.00 A. M., Nov. 19th, and terminated at midnight, same date, with the following results:

Engine counter at 8.00 A. M.	261,420
Engine counter at 12.00 P. M.	298,800
Revolutions in 16 hours	37,380

The piston travel for the pump per revolution of engine is six feet, by scale measurement, and the total piston travel for trial was

$$37.380 \times 6 = 224,280 \text{ feet.}$$

The area of the circular side of pump piston is 95.033 sq. inches; of the pump rod, 7.9798 sq. inches, and of the annular side of pump piston, 87.0532 sq. inches; and mean area of piston, from which the duty is estimated, 91.0431 sq. inches.

The contractor "expressly guarantees a duty of eighty million foot-pounds per one hundred pounds of coal consumed, based upon an evaporation of ten pounds of water per pound of coal consumed, the duty test to be made with a water pressure of eighty-five pounds, the engines (two) pumping three million gallons in twenty-four hours."

The contract provides that each engine shall "have a steam cylinder twenty inches diameter by thirty-six inches stroke."

The actual diameter of steam cylinders agreed upon by the city and the contractor, subsequent to the contract, was twenty-two inches, which diameter was exceeded slightly, as shown in the dimensions given of steam and water ends.

Assuming the diameter of steam cylinder at 22.0625 inches, and the water

pressure gauge as substantially correct, then the relative pressure by gauge to be pumped against during the duty trial under the terms of contract was,

$$\frac{85 \times 382.879}{314.16} = 103.59 \text{ pounds.}$$

An average of sixty-five readings, at regular intervals, of the water pressure gauge was 102.148 pounds. But upon test of the gauge subsequent to the trial it was found to be in error between the pressures at which it was worked—3.25 pounds—making true pressure by gauge 105.398 pounds.

The vertical distance from the upper edge of spike in the side of well to the engine-room floor was 13.58 feet; from floor of pump house to center of pump cylinder, 2.78 feet; and from center of pump cylinder to center of water pressure gauge, 4.97 feet. From which the head pumped against, including suction lift, was,

Average vertical distance from upper edge of spike to water in well during duty trial	1.41356 feet	0.6125 pounds
Vertical distance from upper edge of nail in wall to center of pump cylinder	16.36 feet	7.088 pounds
Suction lift, not including frictional resistances	17.77356 feet	7.7005 pounds
Vertical distance from center of pump cylinder to center of water pressure gauge	4.97 feet	2.1534 pounds
Average pressure by water pressure gauge	102.148	
Error of gauge	3.25	
True water pressure by gauge	105.398 pounds	243.258 feet
Total observed head, including suction lift	266.001 feet	115.252 pounds
Add for extra frictional resistance of water passages into and out of pump	1.000 pound	2.308 feet
When the total head becomes	116.252 pounds	268.309 feet

During the sixteen hours of duty trial there was weighed to the boilers 73 charges of 400 pounds each of water.

(In weighing the water to boilers the tare of the barrel was carefully taken each time a charge was delivered to the feed pump, the weights then advanced 400 pounds, and water drawn from the main until the scale balanced. Each charge of 400 pounds represented all the water above the outlet nipple in the cask.)

Of the total water weighed to boilers, part was lost by leaks over the grates in both boilers, part was drawn off for tests of quality of steam, and the remainder passed through the steam cylinder and jacket of the engine.

In reporting the trial to the City of Decatur, no attempt was made to account for and credit the boiler leaks and the whole quantity of water, less that drawn off for calorimeter purposes, was assumed to have passed through the engine.

From the calorimeter data a mean of nineteen tests gave as average:

Water heated ....pounds	200.
Steam condensed......... ..pounds	10.079
	Fahr.
Initial temperature by thermometer	54.84
Initial temperature by calculation..	54.844
Final temperature by thermometer..	107.76
Final temperature by calculation....	107.917
Apparent range of temperature.....	52.92
True range of temperature.........	53.073
Maximum range by thermometer with 10 pounds of steam	54.
Minimum range by thermometer with 10 pounds of steam..............	51.5
Thermal units per pound of steam..	1161.057
Temperature of steam at observed pressure, 64.18..................	1208.78
Difference	47.723
Latent heat at observed pressure....	894.95
Percentage of water entrained in the steam.........................	5.3325

From which is obtained the following distribution of the water:

	Pounds.
Weighed to boiler..................	29,200.
Drawn off to calorimeter...........	202.35
Entrained in the steam.............	1,546.8
Delivered as steam to the engine....	27,451.35

The contractor guaranteed a duty of eighty million foot-pounds per hundred pounds of coal, upon an evaporation of ten pounds of steam per pound of coal; or for every thousand pounds of steam delivered to the engine there should be developed not less than eighty million foot-pounds of work. Under these conditions the duty by the conventional method was,

$$\frac{224{,}280 \times 91.0431 \times 116.252}{27.45135} = 86{,}471{,}762.42 \text{ foot-pounds.}$$

The calculated displacement of the water piston is 28.377 gallons, and the weight

of water per gallon, 8.34 pounds, and the duty, based upon the discharge of pump, was,

$$\frac{37,380 \times 28.377 \times 8.34 \times 268.309}{27.45135} =$$

86,465,600.4 foot-pounds.

In the following table are given the average of observations during the trial:

Steam gauge at boilers	pounds	65.648
Water level at beginning and end of trial, boiler No. 1	inches	5.5
Water level at beginning and end of trial, boiler No. 2	inches	6.25
Water level, boiler No. 1	inches	5.154
Water level, boiler No. 2	inches	5.422
Steam gauge in engine room	pounds	64.18
Vacuum gauge	inches	26.445
		Fahr.
Temperature of water to condenser		45.444
Temperature of water from condenser		48.987
Temperature of water in hot well		87.531
Temperature of air in engine room		59.288

From 9.00 A. M. to 6.30 P. M. the indicator diagrams were taken coincident with the readings of engine counter and water pressure gauge, regularly every fifteen minutes, the averages from which are given in the following table:

Initial pressure above atmosphere	lbs.	63.592
Terminal " " "	"	0.152
Counter pressure absolute	"	5.439
Effective vacuum	inches	18.520
Mean effective pressure front	lbs.	30.624
Mean effective pressure back	"	29.163
Apparent cut off in decimal of stroke		0.1176
Expansions by volumes, estimating clearance at 3 per cent		6.978
Expansions by pressure		5.461

The considerable percentage of water entrained in the steam, which was evaporated after cut-off by heat from the jacket, partially accounts for the discrepancy in the expansions by pressure and by volumes.

In order to compare the work of the steam end and the water end of the engine, I have estimated the duty for both ends, for the period of time during which the indicator diagrams were regularly taken. The duty of the steam end I have estimated as follows:

	Sq. In.
Area annular side steam piston	371.958
Area circular side steam piston	382.958
Moment of load, front side of piston 371.958 x 30.624	11,390.842
Moment of load, back side of piston 382.279 x 29.163	11,148.402
Ft. lbs. of work per revolution of engine 3 x (11,390.842+11,148.402)=	67,617.732

During this interval of time the engine made 22,198 revolutions, and there was delivered to the boilers approximately 17,455.5 pounds of water, of which 136.975 pounds were diverted to the calorimeter; of the remainder, 923.597 pounds were entrained in the steam, and 16,394.928 pounds were delivered as net steam to the engine.

From which the approximate duty per 1,000 pounds of steam, of the steam end, was,

$$\frac{67,617.732 \times 22,198}{16.394928} =$$

91,551,388.02 foot-pounds.

During the same interval of time the head pumped against was:

	Pounds.
By gauge on force pipe	102.263
Error of gauge	3.250
Distance from center of water pressure gauge to surface of water in well	9.8539
Add for extra frictional resistance of water passages into and out of pump	1.0000
Total head	116.3669

From which the duty per 1,000 pounds of steam of the water end was,

$$\frac{91.0431 \times 116.3669 \times 22,198 \times 6}{16.394928} =$$

86,066,092.31

foot-pounds, and the efficiency of the engine, as represented by the work of the water end,

$$\frac{86,066,092.31}{91,551,388.02} = 0.94008$$

leaving about six per cent. of the total power to overcome the frictional resistances of engine. The discharge of condensation from the jacket for sixteen and one-half hours was 1,014.75 pounds, at an average temperature of 189.3 Fahr. The corresponding discharge for the duration of trial is

$$\frac{1014.75 \times 16}{16.5} = 984 \text{ pounds,}$$

representing an expenditure of

$$100 \times \frac{984}{28,997.65} = 3.3934 \text{ per cent.}$$

of the gross steam delivered to the engine.

The engines pump directly into the mains and no facilities offer for an actual

measurement of the discharge of pump. The pump was carefully measured for length of stroke and diameter of piston with dimensions previously given. From which the calculated displacement per revolution of engine is

$$\frac{91.043\times72}{231}=28.377 \text{ gallons,}$$

of which quantity I have estimated an actual delivery of 94 per cent., or 27.242 gallons per revolution. During the trial for duty the engine made 37,380 revolutions, representing a delivery of 1,018,305.96 gallons, equivalent to a delivery of 1,527,458.94 gallons in 24 hours.

The contract required two engines alike in style and dimensions, "each capable of pumping 1,500,000 gallons of water in twenty-four hours, against a pressure of one hundred and forty-five pounds per square inch" of pump piston. All the water lifted from the well by the pump came from the Sangamon River through 105 feet of 24-inch pipe and 190 feet of 22-inch pipe, to the well.

The rate of flow per minute during the duty trial, based upon the estimated discharge of pump, was

$$\frac{1{,}018{,}305.96}{16\times60}=1{,}060.74 \text{ gallons,}$$

the friction head for which, in a straight, cleaned pipe, free from obstruction, 24 inches diameter, and 105 feet long, is 0.014 foot, and the velocity head

$$\frac{1060.74}{60\times7.48}=2.363 \text{ cu. ft. disch. per}$$

second, and

$$\frac{452.39}{144}=3.1416 \text{ area sq. feet of}$$

24-inch pipe. Then,

$$\frac{\left(\frac{2.363}{3.1416}\right)^2}{64.4}=0.00878 \text{ head due velocity of}$$

flow, and total head for 105 feet of 24-inch pipe, discharging 17.679 gallons per second,

$$0.014+0.0078=0.02278 \text{ foot.}$$

The friction head for a flow of 17.679 galls. 2.363 cu. ft. per second through 190 feet of 22-inch pipe, is

$$0.0134\times\frac{24^5}{22^5}\times\frac{190}{100}=0.03935 \text{ foot,}$$

and the head due velocity of flow

$$\frac{380.13}{144}=2.63 \text{ sq. feet area 22-inch pipe.}$$

Then,

$$\frac{\left(\frac{2.363}{2.63}\right)^2}{64.4}=0.01252 \text{ foot, and total head,}$$

for 190 feet of 22-inch pipe, discharging 17.679 gallons per second, 0.05187 foot, and total head for both pipes, 0.07465 foot.

The actual condition of the pipe from the river to well is unknown; it may be heavily charged with silt or sand, it may be open at some of the joints, it may be incrustated, or it may be out of line. At all events, the actual head on the pipe at river and during the trial was 0.923 foot, which head, with a 22-inch pipe for entire distance, assuming the pipe to be straight and free from obstructions, represents a daily delivery to the well of over 7,000,000 gallons.

The estimate of an actual discharge of 94 per cent. of the calculated piston displacement is justified by the probable flow through the inlet pipe.

The contract provided that the engines shall be "made so as to be operated singly or together."

Subsequent to the duty trial of engine No. 1, both engines were operated for one hour, against a pressure by water gauge of 80 to 85 pounds, at an average speed of 26.85 revolutions per minute for engine No. 1, and 26.583 revolutions per minute for engine No. 2, the automatic cut-off gear controlling the speed of the engines.

The engines were also jointly operated for one-half hour, against a pressure of 142.15 pounds per square inch of water pistons, the maximum pressure attainable with the springs found on the "pop" relief valves attached to the force pipe.

The following table contains the general data and results of the duty trial:

Duration of trial .. ....hours	16.
Steam pressure in engine room...............pounds	64.18
Vacuum by gauge .....inches	26.445
Water pressure by gauge..lbs.	105.898
Total head (including suction lift) pumped against..pounds	116.252
Revolutions of engine per minute........................	38.9375
Piston speed of steam and water pistons...........feet	233.625
Duty in foot pounds per 1,000 pounds of steam...........	86,471,762.42
Percentage of useful effect developed by water end	94.008
Percentage of steam required to maintain the jacket......	3.8934

The boilers from which steam was obtained for the engine during the duty trial were two in number, of the following dimensions:

Diameter of shells...............inches	48.
Length of shells ....feet	20.
Number of flues.....................	5.
Diameter of flues..............inches	{ 3–14 / 2–12
Heating surface shells (2)............	251.828
Heating surface flues (10).............	691.128
Heating surface heads (4)............	15.578
Heating surface, total....... sq. feet	958.034

The boilers were set with independent furnaces, of the following dimensions:

Length of grate bars................	4ft. 2in.
Width of grate bars................	4ft. 7in.
Area of both grates............sq. ft.	38.08
Grate to boiler at front........inches	20.
Grate to boiler at bridge wall .inches	25.

Chimney of brick, circular in section, 4ft. 6in. diameter, and 91ft. 6in. high from surface of grate at front.

Ratio heating to grate surface.........	25.191
Ratio grate surface to cross section of flues............................	3.986
Ratio grate surface to cross section of chimney............................	2.391

The water supplied to the boilers during the trial was 29,200 pounds, of which 1,557.09 pounds was entrained in the steam, and 27,642.91 pounds evaporated into saturated steam.

The coal burned during the trial was 6,000 pounds, representing an evaporation per pound of coal of

$$\frac{27,642.91}{6000} = 4.60715 \text{ pounds,}$$

from a temperature of feed of 48.937 Fahr.; corresponding to an evaporation from and at 212 Fahr. per pound of coal of 5.534 pounds.

The weight of ash and clinker weighed back at end of trial was 795 pounds, representing 13.25 per cent. of the total weight of fuel charged to the boilers.

The coal was of fair quality, and under good boilers, well set, would show an evaporation quite fifty per cent. above that developed during the trial.

The most important lesson to be learned from this trial of a pumping engine is the fact that a high duty can be had from a single-cylinder condensing engine. Of a list of duties before me the highest for this class of engine is 68,387,200 for single cylinder, beam crank and fly-wheel condensing engine, by Hubbard and Whittaker, for the Brooklyn Waterworks, reported 1860; and 65,824,581 for the same class of engine, by Mr. D. C. Cregier, for the North Side Pumping Works, Chicago, reported 1874. If the duty of single-cylinder condensing, crank and fly-wheel pumping engine can be raised to 85 or 90 millions, with the corresponding diminution in first cost over compound engines, it will certainly meet a want now felt, especially in works where the quantity of water pumped and revenue is small.

The *Scientific American* says:—"The quality of movement of the particles of iron under pressure or percussion is a remarkable one, whether the change in arrangement is made while the iron is hot or when it is cold. Red-hot iron can be pressed to fill a mould as clearly and exactly as so much wax could be, and the grain of the iron will certainly follow all the contour of the mould. Thus the heads of pick-axes and articles of a similar form can be shaped by pressure, the metal that is removed to make the hole for the helve being forced to form the projection of the adze-like head. Cold iron can also be moulded into form by pressure—a method largely practised to finish drop forged iron articles. The heading machines for making rivets, bolts, and wood screw blanks shows some surprising results in the compression of iron; a No. 6 1 in. screw requires a piece of wire slightly more than 1½ in. long to form it. Yet the total length of the screw blank, headed, is just 1 in. Of this the countersunk shaped head is ⅛ in. by $\frac{5}{16}$ in. widest, or top, diameter. Now, it has been proved by experiments with shorter bits of wire that less than $\frac{5}{16}$ in. of the extra $\frac{8}{16}$ in. is required to form the screw head. What becomes of the remaining more than $\frac{3}{16}$ in. in length of the original 1½ in. that makes the 1 in. screw blank? There can be but one answer—the iron is driven upon itself; in other words, $\frac{3}{16}$ in. of wire is compressed into ⅞ in.—measuring under the head—so that 1 in. and $\frac{1}{16}$ in. of wire is compressed into ⅞ in. in length without increasing the diameter of the wire." If this question is followed up, it will be found that the wire has increased in diameter, and that the specific gravity of the iron is but very slightly increased.

ON THE STRENGTH OF MATERIALS UNDER REPEATED STRESS.

By Prof. MANSFIELD MERRIMAN.

Written for Van Nostrand's Engineering Magazine.

It has been generally recognized for a long time that repeated varying stress is more dangerous than steady stress, and requires a higher factor of safety, or, in other words, that the allowable working unit-stress to be used in designing a bar should be less for repeated than for steady stress. The experiments of Wöhler and Spangenburg have also furnished the means of grading these working unit-stresses according to the range of strain, in a more satisfactory manner than mere judgment can do. These experiments were made by subjecting bars of iron and steel to repeated stresses ranging between certain limits. When the range was great it was found that the number of repetitions required to produce rupture was small, and that as the range was decreased the number increased. The unit-stress which would produce rupture after an enormous number of repetitions were thus determined for different ranges, the word enormous meaning several millions. The discussions of these valuable experiments have established the following laws:

1. Rupture may be caused by repeated applications of a unit-stress less than the ultimate strength of the material.

2. The greater the range of stress the less is the unit-stress required to produce rupture after an enormous number of repetitions.

3. When the unit-stress ranges from zero up to the elastic limit the number of repetitions required to produce rupture is enormous.

4. A range of stress from tension into compression, or *vice versa*, produces rupture sooner than the same range in stress of one kind only.

5. When the range of stress in tension is equal to that in compression, the unit-stress which produces rupture after an enormous number of repetitions is a little greater than one-half of the elastic limit.

In order to deduce a formula for the unit-stress s which will cause rupture after an enormous number of repetitions, let us consider a bar in which the stress ranges from p to a greater value P. These stresses p and P are the total stresses in the bar; if A be the area of its cross-section $\frac{p}{A}$ is the least, and $\frac{P}{A}$ is the greatest unit-stress upon it. The quantity s whose value is required should be the same as $\frac{P}{A}$. Let u be the ultimate strength and e the elastic limit of the material, which, like s, are given in pounds per square inch. Let f be that stress per square inch which causes rupture when p is equal to P, one being tension and the other compression.

According to the above laws the unit-stress s which will cause rupture after an enormous number of repetitions ranging from p to P is a function of the difference $P-p$, or of the ratio $\frac{p}{P}$. Thus

$$s=\Phi\left(\frac{p}{P}\right)$$

This function will be established by assuming an empirical expression with undetermined constants, and then finding the values of the constants from the experimental laws. Thus let

$$s=le+m\frac{p}{P}+n\left(\frac{p}{P}\right)^2,$$

in which l, m and n are constants to be determined. If p and P are both tension or both compression, the sign of the ratio $\frac{p}{P}$ is to be taken as positive, but if one is tension and the other compression, it is to be taken as negative. Now, if $p=P$, there is no range of stress, and the case corresponds to that of a steady load for which s is the ultimate strength u. Again if $p=0$, the case is that of the third law for which s is the elastic limit e. Lastly, if $p=-P$, the case is that of

the fifth law for which s has the value f. For these three cases the assumed function becomes

$$\begin{array}{ll} \text{For } p=\mathrm{P}, & u=le+m+n, \\ \text{For } p=0, & e=le, \\ \text{For } p=-\mathrm{P}, & f=le-m+n, \end{array}$$

From these three equations we find the three values

$$l=1 \qquad m=\frac{u-f}{2} \qquad n=\frac{u+f-2e}{2}$$

and the expression for s hence is

$$s=e+\frac{n+f}{2}\cdot\frac{p}{\mathrm{P}}+\frac{u+f-2e}{2}\cdot\left(\frac{p}{\mathrm{P}}\right)^2$$

This may be written in the form

$$(1) \quad s=e\left\{1+\frac{u-f}{2e}\cdot\frac{p}{\mathrm{P}}+\frac{u+f-2e}{2e}\left(\frac{p}{\mathrm{P}}\right)^2\right\}$$

which is often more convenient in discussion and computation.

This formula satisfies the three imposed conditions. If the ratio of p to P is one, the value of s is u as for quiescent stress. If the ratio is 0, the value of s is e as required by the third law. If the ratio is -1, the value of s is f as the fifth law demands. To ascertain its reliability for intermediate values of the ratio, a comparison with experiment is necessary.

For German Phœnix iron Wöhler found the ultimate strength u about 55,000 the elastic limit e about 31,000, and the vibration strength f about 16,500 pounds per square inch. For these values formula (1) becomes

$$s=31{,}000\left(1+0.62\,\frac{p}{\mathrm{P}}+0.15\frac{p^2}{\mathrm{P}^2}\right)$$

He also found that this iron when subjected to a tension ranging from 25,000 to 45,500 pounds per square inch did not rupture after four millions of repetitions. For this care the ratio of p to P is $\frac{25000}{45500}$ or $\frac{5}{9}$, and the formula gives $s=43{,}000$, which is a fair agreement with the observed value 45,500.

For Krupp's axle steel Wöhler found $u=105{,}000$, $e=50{,}000$, and $f=29{,}000$. For these values the formula is

$$s=50{,}000\left(1+0.76\,\frac{p}{\mathrm{P}}+0.34\frac{p^2}{\mathrm{P}^2}\right)$$

He also found that this steel when subjected to a tension ranging from 36,400 to 83,200 pounds per square inch did not rupture after twelve millions of repetitions. For this case the ratio is $\frac{7}{16}$, and the formula gives $s=70{,}000$, which is considerably lower than the observed value.

For soft spring steel Wöhler found $u=115{,}000$ and $e=52{,}000$. The value of f was not determined, but by analogy with the preceding it may be taken as 27,000 pounds per square inch. For these values formula (1) becomes

$$s=52{,}000\left(1+0.84\,\frac{p}{\mathrm{P}}+0.36\,\frac{p^2}{\mathrm{P}^2}\right)$$

In the following table the first and second columns give the values of p and P, both tensile unit stresses, for which bars did not rupture when the stress was repeated over thirty million times. The third column gives the ratio of p to P, and the fourth the value of s computed from the formula. As p and P are here unit-stresses the values in the second and fourth columns should be the same, and the fifth column contains their difference. The agreement between the observed and computed values, though not close, may be regarded as fairly satisfactory.

p.	P.	$\frac{p}{\mathrm{P}}$	s.	Diff.
68,640	104,000	0.66	89,000	−15,000
62,400	93,600	0.67	89,500	− 4,100
58,240	83,200	0.7	91,500	+ 8,300
51,600	83,200	0 5	78,500	− 4,700
31,200	72,800	0.36	69,000	− 3,800
26,000	72,800	0.43	74,000	+ 1,200

Unfortunately no experiments were made by which the formula may be tested for the values of the ratio between 0 and -1. Further experiments on repeated stresses are much needed to perfect our knowledge of the subject. Wöhler's researches give three definite values of s, namely the values u, e, and f, corresponding to the ratios 1, 0, and -1. If the ratios be taken as abscissas and the values of s as ordinates, three points are determined not in a straight line, and formula (1) is the simplest curve which can be passed through those three points and is hence an approximation to the true law.

For wrought iron the value of f is about $\frac{1}{2}e$, and the value of u about $2e$, and the formula is

$$s=e\left(1+\tfrac{3}{4}\frac{p}{P}+\tfrac{1}{4}\cdot\frac{p^2}{P^2}\right)$$

which gives the unit-stress required to rupture the bar when the applied stress ranges from p to P. For security the same factor of safety as for quiescent loads should be introduced, namely 4. Then the working unit-stress is

$$s_w=\frac{e}{4}\left(1+\tfrac{3}{4}\frac{p}{P}+\tfrac{1}{4}\frac{p^2}{P^2}\right)$$

and under this stress it may be expected that the strain will be safely applied a very enormous number of times within the given range. For example, if e=27,500 pounds per square inch, the following are valves of the rupturing and working strengths of wrought iron for given ratios of the limiting strains.

For $p=P$	s=55,000	s_w=13,750
For $p=\frac{1}{2}P$	s=39,520	s_w= 9,880
For $p=0$	s=27,500	s_w= 6,875
For $p=-\frac{1}{2}P$	s=18,920	s_w= 4,730
For $p=-P$	s=13,750	s_w= 3,440

The first of these corresponds to the case of dead load where there is no range of stress, the second to the case where the minimum stress is one-half the maximum, both being tension or both compression, and the third to the case where there is no stress under dead load. The fourth and fifth correspond to the case of stress alternating from tension into compression, or *vice versa*, the fourth where the range in one kind is double that in the other, and the fifth where these ranges are equal. It is hence seen to be important that the minimum, as well as the maximum strains, should be computed for members of bridge trusses, and in all cases where pieces are to undergo repeated varying stress.

THE SOLAR TEMPERATURE QUESTION.

By DeVOLSON WOOD, M. A., C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

The attempted proof by Mr. Gilman —that the intensity of heat emitted by a spherical incandescent body varies inversely as the square of the distance from its center—in the January number of this Magazine, at first sight appears so plausible that it would be almost a graceful act to admit its truth. But, unfortunately, it not only contains some statements the truth of which may be questioned, but it involves principles conflicting with others which are supposed to be well established.

In regard to the proof, Mr. Gilman states that he "has referred me to Captain Ericsson's experimental demonstration." I find no such proof. The principle was *assumed* by that eminent engineer, as stated on page 362 of the November number, in accordance with Newton's assumption, as given in the *Principia*. Although Newton's name carries great weight on all questions upon which he wrote, yet it will not be presumed that his views are to be unquestioned. In regard to heat there was comparatively little known in his day.

Next Mr. Gilman says "after a time the flow of heat from the body will be uniform and each point of the surrounding space will also acquire a constant temperature." Here appears to be a misconception. The temperature of space is a matter about which we have little, if any, definite knowledge. If the ether in planetary space be perfectly diathermanous, heat will pass through it without imparting temperature to it. The temperature may be, and probably is, exceedingly low, while any quantity of solar heat passes through it. The *quantity* of heat passing the concentric spherical shells will be the same, while the temperature may be lower than we are able to measure. Mr. Gilman, however, assumes that the intensity, or temperature, will be directly as the quantity of heat passing a given area—say one square foot—of the spherical shell. This being erroneous his demonstration fails.

The question of temperature is complex, but it is unnecessary to discuss it here, in order to show still further the fallacy. Thus, we assume that it will be admitted that the law of the inverse

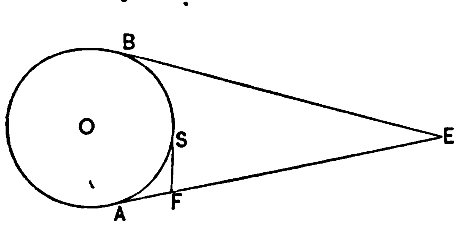
squares would be applicable if the radiant be considered as a mere point. Let o be the center of the sun, S a small portion on its surface, and E the position of the

earth. If now, a be the intensity of the solar heat at E, the intensity at any distance x from S will be

$$a\frac{(S\,E)^2}{x^2}$$

If $x=o$, this becomes infinite. But if the entire surface be involved and the inverse squares be reckoned from O a finite value would be found for the temperature at S. In other words, according to Mr. Gilman's mode of computation, we will find a less temperature at S when the entire surface of the sun is the radiant, than we will, according to the recognized law when only a small portion of it is the radiant. This is a reduction to an absurdity—both hypotheses cannot be true, and the latter is too thoroughly established to be easily shaken.

Mr. Gilman and Captain Ericsson both assume that heat passes radially outwards as if from the center of the sun; whereas every point on the sun's surface is the center of a hemisphere, through which it radiates heat. Captain Ericsson, on page 363 of the November number of this Magazine, on the hypothesis of radial emission finds that only 0·0658 square inch of the solar surface furnished heat to the 3,130 square inches of his pyrometer; whereas, in fact, it received heat from nearly one-half of the sun's surface.



It is plain from the figure that if the pyrometer be at F it will receive heat from about one-fourth as much surface as if at E, the arc AS being about one-half AB. The nearer a body is to the sun the less will be the hot surface to which it will be exposed. So that while we may admit that the heat received from the small portions of the sun exposed to the pyrometer, varies inversely as the squares of the distance from them, it also varies directly as the sum of those small portions; and since this sum diminishes as we approach the surface of the sun, the law of the inverse square from the center fails. We trust that it will now be seen in what sense the law of Pettit and Dulong is at variance with inverse squares. We repeat what we said in a former article, that the law of the inverse squares does not, in any case, determine the temperature of the radiant; the law of Pettit and Dulong does in some cases at least. If what we have said be correct, it follows that the temperature of the solar surface cannot be determined by applying the law of the inverse squares, and it will no longer be claimed that its temperature exceeds 3,000,000° F., unless deduced by other methods.

[*Note.*—The following remarks bear upon this subject. Professor Zöllner remarks (*Phil. Mag.* 1878, Vol. 46, p. 355), "That there is no proportionality between the heat radiation of a body and its temperature, I have already made clear in a criticism on the method applied by Father Secchi to the determination of the temperature of the sun." Then follows a quotation from Soret. "M. Soret has recently proved by interesting experiments that in fact the heat radiation of a body increases much faster than its temperature, and that consequently the hypothesis made by Father Secchi in his actinometric determination of the temperature of the sun 'the radiation of a body is proportional to its temperature' was inadmissible." Father Secchi found the absolute temperature of the sun to be 2,900,000° F., in regard to which M. Soret remarks, "To control the accuracy of this reasoning, let us apply it to the determination of the temperature of zirconium heated at the oxyhydrogen-lamp. We shall find T=45,990° C., a figure absolutely inadmissible, for the temperature of a body heated in the oxyhydrogen-flame is at its utmost 2,500°C.(1,400°F)." It follows from this experiment that the temperature of the solar surface is less than $\frac{1}{20}$ of that found by Father Secchi, or less than 145,000° F. Professor Zöllner found in a computation illust

ing a theory which he had developed that the absolute temperature might be between 26,000° F. and 34,000° F., but he cautions the reader against accepting these as definite values. (*Phil. Mag.*, 1873, vol. 46, pp. 299–301). Rossette concludes that the absolute temperature is not less than 5,500° F., nor much more than 11,000° F. (*Phil. Mag.*, 1879 (2), p. 548).]

THE BLACKSMITH'S ART IN ANCIENT AND MODERN TIMES.

Read before the Society of Architects by ALFRED NEWMAN.

From "The Building News."

The recent movement in favor of the adoption of wrought-iron work for architectural and domestic purposes, in preference to the hard geometric productions of the foundry, has caused one to wonder why this beautiful art has become superseded by cast iron. As far back as the beginning of the 18th century, castings were intermixed with the wrought work, and, the evil having commenced, the wrought work became gradually exterminated, and castings came entirely into vogue. The florid designs and general pretentious effect producible at a moderate cost captivated the general public, and the smith's art gradually fell into disuse. There are several other reasons why it fell into disuse—notably an Act of Parliament which was passed prohibiting the use of those elaborate and ornamental signs which had made the streets of London so picturesque for a long time, and in which each tradesman outvied his neighbor. The disuse of signs was a really serious blow to the smith, almost as serious as the actual introduction of castings. The gradual general use of coal as a means of fuel did away with fire dogs, grids, spits, and many other items which formed an important part of the smith's business. The rigid register stove and kitchen range now took the place of the formerly picturesque fireplace. These causes, together with the indifferent taste which was growing during the last century, and which even extended far into this, caused many to look upon wrought-iron work with indifference. The smith became a mere laborer; he had lost the love of his art, and the task of now re-instilling an artistic spirit is one attended by great difficulty. Moreover, many architects have so long been accustomed to specify for castings that the cost of wrought iron appears exorbitant in comparison. Furthermore, the enterprising northern firms, whose books of patterns lie close at hand, save many, who care not for detail, a deal of trouble, and, as a consequence, those beautiful gates which are still met with in many of our suburbs are elbowed by work in the worst possible taste. Not unfrequently a classic villa is surrounded by heavy cast-iron railing of so-called early English Gothic style—a style which has been invented by the large manufacturers, and which is quite devoid of the tastes of the period it endeavors to represent. This railing is frequently painted a gaudy red, the effect in many instances being heightened by a liberal display of gold. From the few specimens left us of the architecture of the past centuries, the English smiths had acquired infinite taste, and a perfect mastery over the material on which they worked. The brackets for hour glasses which are still to be seen in many of the country churches, the hinges and the latches, the handles, the font covers and altar rails, &c., are alone sufficient to prove this. In old prints of London mansions, such as Arundel House, Burlington House, Powis House, Montague House, and York House, also, we must not forget to enumerate those fine railings in Great Ormond Street still remaining, which, I am pleased to say are religiously preserved by the owner. No. 5 Bloomsbury Square, Isaac D'Israeli's house, has some fine balusters of 1720; we have good pilasters at No. 45 Lincoln's Inn-fields, and a beautiful panel there. In Westminster Abbey, a well-known example of early ironwork, and some interesting old sword rests in All Hallows, Barking, Church, in

the City, deserve to be better known. Those sword-rests are of Charles the Second's time, and are beautifully worked. Canterbury, Gloucester, Bristol, and York cathedrals boast of excellent work, and in Haddon Hall, Ham House, and other mansions, there are good examples of early domestic ironwork still remaining. Many examples are met with in the provinces of magnificently-wrought gates. All those can only prove how we have deteriorated by the universal use of cast iron. On the Continent, as well as here, a reaction has taken place, perhaps with greater enthusiasm than has been exhibited in this country. For instance, in the Place des Sablons in Brussels, the railings surrounding the gardens have been executed by the leading smiths of Belgium, each having contributed a part in which to display his skill and taste. The result is an effect which must strike the most uninitiated. The smiths here have endeavored to outvie each other by an extraordinary display of the most intricate and difficult treatment, showing a splendid illustration of the mysteries of their craft. This work has been executed by the Government, who have by this means encouraged the spirit of emulation amongst their native smiths. I hope that our Government may pursue a similar policy in some of our public works. But it is in you, gentlemen, the power of fostering this art lies. Your clients are led by you, and to you the public look for that which is best in taste and artistic in treatment. Germany appears to have lost the art more completely than ourselves. Beautiful as the work of the past has been, for the remaining examples in the collection in Munich show the most unique and beautiful in Europe, the modern attempts of Germany in this direction are decidedly failures. And during a recent trip to the principal cities of Germany, I did not discover a single firm who had produced work exceeding in better taste, or, indeed, as good as that which emanates from Birmingham. The liberal use of stamped rosettes, malleable iron, and other tricks for cheapening handwork, have completely destroyed all individuality in the work, and their wrought iron has become cast, as far as taste or effect is concerned. As to French work, it has degenerated into a treatment of polished hoop iron, and is innocent of the hammer of the smith. The Italians, whose splendid reputation in the past would lead one to hope for better things, now devote their skill to the reproduction of sham antiques, and that, too, in the thinnest and flimsiest of material. I have met with no less than three gentlemen, recently, who brought from Italy real antique lanterns, and I have every reason to believe factories for these exist in the neighborhood of Vicenza, the principal city of the iron industries in Italy. In Vienna the movement is taking a right direction, and some really splendid work has been done. Here, again, the State has lavishly introduced wrought-iron work in their public buildings, as the Votive Church; whilst the old gates at the palace of Prince Eugene of Savoy, and some of the balconies in different parts of the new town, show the greatest taste. In the State collection here are also some fine examples of forgings, and also in the St. Stephen's Cathedral. Of Spain, one can only say that the modern Spaniards ignore the forge. In Holland, notably the Hague, no building is erected without the introduction of wrought iron, and much of it is excellent in workmanship and design. At the Exhibition of Amsterdam, recently, there were some really good exhibits of native work. In Hamburg I found some excellent smiths. There is a great taste also arising in America, notably for domestic purposes, and I may remark that the American purchaser shows the greatest appreciation for merit, and has a very sharp eye for defects. And now a word for a land far off, where one would hardly seek for this particular branch of industry—I speak of Japan. I have had the pleasure of seeing some exquisite specimens of ironwork of the choicest possible design and marvelous in workmanship—fire-dogs and irons, lanterns, vases, sword hilts, and other kinds of beautiful objects. Some lanterns from Japan, that I saw recently, show such a Gothic feeling that it causes one surprise as to how they could have emanated from Japanese hands. Having now given a brief glance at foreign work, let us consider what has been done to improve the smith by technical schools of design, or by the much-abused city companies. By the way,

there is a blacksmith's company still extant; but what they have done to foster their art and mystery I have never been able to discover. Now, as to the technical schools, if forges could be erected, superintended by experienced smiths, in conjunction with art classes, I am sure many young men would learn this craft, and not only improve their means, but produce a supply of mechanics which are really becoming needed. Now, as to the English mechanic, we have never had to find fault with his actual work. In accuracy and finish he is all that can be desired, and soundness of his work is undeniable. In this respect an English workman can hold his own against all comers; but when you expect taste, idea or originality, he is most disappointing. He is completely devoid of those qualities. In past times this cannot have been the case. The great skill and taste, together with quaint conceit necessary for producing, for instance, a suit of armor, indicates the high qualities of the smith's calling. There is one great reason, too, which causes a lack of good hands—namely, the gradual but undoubted prejudice against apprenticeships. When it is considered that a strong lad can earn 15s. a week in a factory, his parents or guardians are very loth to apprentice him at all, and a premium is a thing of the past. This lad will destroy his chance of ever becoming an efficient mechanic, and he himself ends in joining the already large army of mediocre workmen who change from shop to shop, and are always out of work. It is also singular to remark that the largely-increased use of tricycles and bicycles has absorbed many of the best smiths we possess. As art workmen they are preferred by the manufacturer from their great appreciation of the niceties of the work. Beautiful as are the designs of many of our architects, in nine cases out of ten it is next to impossible for a smith to execute them, except at a great cost, whereas a similar effect might be obtained by a slight knowledge of the material in which the design has to be made. Junctions are frequently shown where it is impossible for a smith to weld, which induces pinning or riveting, which is not the natural manner in which the smith would execute his work. I may here remark that I shall be most pleased if any member of this society, desiring any practical information, would visit my forge, where I could better illustrate my meaning in this respect. Again, many gentlemen draw their information from the study of Mediæval work, which, I may say, in most instances was executed in a way now no longer necessary or desirable. In Mediæval times a man took his pig of iron, and by a most slow and laborious process drew it down to the size necessary for his work. Thus, in making hinges they were not welded as we do, but were made out of one piece; but now, by means of steam rollers, we are enabled to get ribbons and bars of any size, and so by welding pieces together we can secure the same result without the heavy labor attached to the same process. There are some purists who object to this, and think it necessary to pursue the same process as of old; but it is idle, as the welding implies the perfect junction of the parts. Gentlemen retaining this opinion would find it impossible to distinguish between the old and the new way. As regards the elaboration of the craft, such as flowers, &c., so often exhibited as masterpieces to the eyes of the uninitiated, these are merely manipulations of sheet iron, and could be executed by any decent silversmith, and have nothing to do with the smith's work or legitimate wrought-iron work. There is also a great deal of good smithing that has been spoilt by coming under the hands and files of excellent fitters, who carefully smooth away all traces of the smith's hammer, and produce a piece of ironwork more fitted for a pattern founder than an artistic piece of ornamentation. This over-zealous love of finish is a failing that we have constantly to battle with. Now, as to the use of malleable iron which I observe so liberally displayed in the work of firms professing to execute wrought-iron, so far as artistic effect is concerned, cast iron might just as well be used. Its only merit is that it is of a less friable nature. Many orders are secured by firms intending to intermix the decorative parts with this material, which enables them to estimate it at a price which it is impossible to compete with; in fact, hand work and ornamentation produced by this material possesses all

the monotony of cast iron, and should be eschewed on this account. A great deal of ironwork that is spoilt is attributable to the system of inclusive contracts. The quantity surveyor, an inestimable man in most respects, but from whom an artistic or approximation is hardly to be expected, specifies for the amount which he considers sufficient for the smith and his items, such as builders' ironmongery, &c., often includes many pieces of work, such as knockers, handles, &c., which would, if not allowed for, add considerably to the expense of the joiner and stone mason. I am often asked to put 50 ft. or 60 ft. of railings and balusters, with a pair of gates, for £50—a price which would hardly allow for supply of cast iron. Now, another serious reason which prevents much good work from being done, and actually encourages these shifts and contrivances I spoke of elsewhere, is the great idea possessed by many architects of getting a price from several firms, on the competition system. Now, very frequently a person whose tender is the lowest, produces a hard and unsympathetic interpretation of the design, and with a commonplace foundation, and is sometimes an unscrupulous worker, who would sacrifice everything approaching to taste to enhance his profit. This, in fact, is the only course left to him, he having ousted those who might have taken a positive interest in the work. Another very potent reason for the non-encouragement of this system is that many trifling details or additions which might considerably enhance the design, and which can only be ascertained during the progress of the work, must be passed by through the inelastic nature of the contract. We find this peculiarly hard when we have to turn out unsatisfactory work, or bear a loss, and though I am not here pleading for *carte blanche*, I am asking for the same recognition of the smith as of the stone, brick, or wood carver. Now, to recur to the use of wrought iron, as regards domestic work, a serious injury has been done to wrought-iron work by designs in the so-called early English and Gothic. Domestic examples of Gothic work exist, and are only found in churches, where trefoils, quatrefoils, triangles, circles, fleur-de-lis, &c., were used as symbolizing Christianity; but it is not to be supposed in Gothic times domestic architecture was treated in this form; hence people have a natural horror of seing a modern corona decorated with these devices in a dining room. But I may say that the few remaining examples of domestic Gothic architecture are entirely free from those ecclesiastical symbols. Talking of ecclesiastical symbols suggests ecclesiastical metalwork, which may be purchased where clerics order their vestments. The corona, the font covers, the hinges, the handles, the altar rails, the pulpit lights, &c., made by the gross, are temptingly arranged in a row, and may be purchased for those of high or low tendencies, much in the same way as mourning may be ordered for overwhelming bereavement or mitigated grief. Why are not these designed by the architects to suit the churches for which they are intended, instead of being left in the hands of dealers. One enterprising firm advertise weekly their list of ecclesiastical abominations, and wind up by stating, "and eagle lecterns from £45." Our old friend, the eagle, having descended so low, I think it high time that he should be abandoned altogether. In spite of what has been recently urged against specialists, it can hardly be expected, gentlemen, that with the anxieties, responsibilities and labor inseparably connected with large building operations, architects can find time to fathom the practical depth of ironwork, which plays a somewhat unimportant part in the ornamentation of the edifice, yet without this practical knowledge the results are seldom, if ever, satisfactory. To observe a junction which would be impossible for the most accomplished smiths to weld, to put rivets where a practical man would put collars, and collars where they would be next to useless, is painful to the initiated, and pleasing in effect to the general critic. The design has to be carried out, but it is a thing of shreds and patches—in fact, not a legitimate construction, and what no smith would attempt if he were freed from the bondage of carrying out the design literally. One of the great charms of Mediæval work is the utter absence of the forced treatment, and it is this which raises the controversy as to who made the designs for these works, the smith or the architect. If made by the smith he

was a man possessed of infinite taste and fancy; and were they made by the architect—well, then the architects of old must have had a practical knowledge of something far beyond anything that can be acquired or expected of the architects of to-day, heavily handicapped as they are by the hurry and competition of the age. It is to be hoped, gentlemen, and it rests with you more than with any of my craft, that we may arrive at a higher stage of perfection in this art than has been yet attained. It would be rather humiliating to suppose that you had only attained the standard of past workers in this age with all the advantages we possess. I hope we shall not be content with achieving the highest qualities of Mediæval work, but that we may yet far exceed them.

THE ORIGIN AND REMOVAL OF AN OBSTRUCTION IN THE DANUBE.

By E. GARTNER.

From Abstracts of the Institution of Civil Engineers.

During the construction of the North-Western Railway Bridge across the Danube, in 1870, and after the caisson for No. 2 Pier (which was attached by chains to two floating stages or platforms) had been sunk in its proper position, and the pneumatic foundation of the pier had commenced, a very strong flood accompanied with much ice came down the river and broke one of the mooring chains. The staging consequently shifted its position and, together with the attached caisson, would have been carried down stream, and probably have greatly injured, if not destroyed, the Tabor and Northern Railway bridges (both wooden structures), had not the engineer in charge knocked out the bolts of the chains and thus set free the caisson, which sunk and took up a position oblique to its original site, and at its nearest point about 17 feet distant from it.

The two stages subsequently sprung a leak and sunk, and orders had been given for their being fired into and destroyed—for they could not be approached on account of the ice—when another flood occurred which carried them, fortunately, clean through the Tabor and Northern Railway bridge openings, though for a time the latter bridge was endangered. It was at first proposed to lift the caisson and its appendages bodily, and a special contrivance, a drawing of which is given, was designed with this object, but little was accomplished. A diver was also engaged to recover the chains by which the caisson had been suspended from the floating stages, but the force of the current prevented his working. This method of removing the obstruction was therefore abandoned, and its complete destruction by blasting with dynamite resolved upon. It should be mentioned that during the above operations the construction of the bridge was steadily proceeded with, and was completed in eighteen months, or three and a half months within the contract time.

The first experiments with dynamite under ice proved that charges of even 9 lbs. might be used without seriously endangering the stability of No. 2 pier, which, as before stated, was at its nearest point 17 feet distant from the sunken caisson, but also showed that, owing to the intense force of the current below, special precautions were necessary in laying the charges, and these took too much time. A stop was consequently put to the blasting operations, and the matter referred to a committee.

The Committee proposed that the river-bed should be dredged out and the caisson buried at such a depth as to comply with the order of the River Commissioners, viz., to have 10 feet of water clear for navigation. This necessitated the excavation of a pit about 25 feet below the surface of the water, and the construction of a special dredging machine to work at such depth. The work commenced in the autumn of 1872, and by the middle of November the required depth was almost

reached, but it could not be maintained as the pit continually filled up, and, after about four times the actual cubic capacity of the caisson had been dredged out and other difficulties encountered, this plan was abandoned. A mechanical destruction of the caisson by cutting it up into pieces was then proposed, but rejected on account of the cost, and finally recourse was had again to blasting with dynamite, and special experiments were made as to the most suitable strength of the charges and the best methods of laying them. Captain Lauer of the Engineers, was placed in charge of the operations, and he designed a special blasting vessel, with arrangements for laying the charges exactly and expeditiously in position, as this was found to be the chief difficulty in the previous experiments owing to the strength of the current. This vessel was 80 feet long and 12½ feet broad, and could be fixed in any required position by hauling on chains attached to two bow and two stern anchors.

A wooden platform projected about 16 feet over the stern, and at the end of this was a movable framework or grating to guide the rod carrying the charge, the upper end of which passed through a ring or staple attached to the cross-piece of a timber frame fixed to the platform, to which also was fastened a pulley, over which ran the chain for lowering or raising the rod.

The guide-rod was made of iron gas-pipe 2½ inches inside diameter, and 1½ inch thick. The lower end was split up for about a foot to receive a stick 5 or 6 feet long which was there securely fastened, and at its lower end carried the box containing the charge, which was fired by electricity from the blasting-vessel. The rod was then drawn up, a new stick and fresh charge attached, and it was then let down again and so on. At first, charges of ½ lb. only of dynamite were used, but afterwards it was found that charges of 1 lb. and even 1½ lb. could be used with safety, and the work accordingly made great progress.

Altogether two hundred and twenty-three charges were fired, involving a consumption of 187 lbs. of dynamite. The quantity of material removed was 53 cubic yards, which gives $\frac{187}{53}=3\frac{1}{2}$ lbs. per cubic yard. The work occupied the officer in charge, with seven or eight men, twenty-two days, viz., from 4th to 25th November, 1873, and, inclusive of salary and wages, purchase of dynamite, &c., cost about £320, or say £6 per cubic yard.

As dynamite readily freezes at 50° Fahrenheit, and during November a continual low temperature prevailed, it was necessary to soften it by warming. A full description with sketch, is given of the warming apparatus, and also of the fuse for firing the charge. Of the two hundred and twenty-three charges fired—

					Lbs.
91	contained	½ lb.	dynamite	=	45½
114	"	1 "	"	=	114
18	"	1½ "	"	=	27
	Total..............				186½

DANCHELL'S ELECTRIC RAILWAY.—There is on exhibition at the Westminster Aquarium a working model of a new electric railway for which very considerable advantages are claimed by the inventor, Mr. Danchell. The feature which distinguishes it from previous attempts of the same kind is that the locomotive and vehicles run between a pair of rails placed vertically one over the other, the lower carrying the weight and the upper serving as a guide to prevent the vehicles from falling over sideways, and also, in some instances, increasing the adhesion of the locomotive. Each vehicle has two wheels, one before the other. These wheels are not flanged, but are kept on the track by eight guide rollers, two running against each side of the upper rail and two against each side of the lower rail. The electric motor carries on its spindle a small friction pulley, situated between the two driving wheels of the locomotive. These wheels are carried in sliding bearings and can be set up to obtain a sufficient pressure against the friction pulley, so that the rotation of the motor is imparted to them. For lines with steep gradients, a small wheel is fixed over each driving wheel in such a way that it has a constant tendency to jam itself between the wheel and the upper rail and thus increase the adhesion of the locomotive. The rails act as the conductors for the current, which enters the motor from one of them and leaves it by the other. It is claimed for this railway that it will run with very small friction, and that exceedingly high speeds are possible without danger, but it is difficult to find any argument for putting an electric locomotive on a single rail which is not equally applicable to a steam locomotive, while the safeguard afforded by an overhead rail is as available with one kind of carriage as another. An engineer who put forward such a plan with any existing form of traction would expose himself to derision, and we fail to see that the use of electricity as a propelling power alters the conditions of stability under which a railway works. As to the electrical part of the scheme it appears devoid of novelty, and whatever advantages exist in the Danchell railway are to be found in the use of a single track and a steadying overhead rail.

EVAPORATION OF WATER FROM AND AT 212° FAHRENHEIT.

By RICHARD H. BUEL, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

In order to reduce the results of boiler trials to a common standard, for the purpose of comparing them with each other, it is usual to calculate, from the evaporation under the actual conditions of test, the equivalent evaporation, or the evaporation which would have been obtained if the temperatures of feed water and steam had each been 212° Fahrenheit.

Thus, if E represents the number of pounds of water evaporated per pound of combustible at a pressure P, from a temperature T; H, the number of British thermal units per pound of steam evaporated under these condition; and L, the number of British thermal units per pound of steam evaporated from and at 212°; the equivalent evaporation per pound of combustible will be

$$E \times \frac{H}{L}.$$

The quantities, H, and L, are to be taken from a table of the properties of saturated steam; H, being a variable depending upon P, and T; and L, a constant. Most of the steam tables in use are professedly based on the experimental results obtained by H. V. Regnault, and published in the "Mémoires de l'Académie Royale des Sciences de l'Institut de France. Tome XXI. Paris, 1847." Hence, one might reasonably expect that the quantity L, would be the same in all modern steam tables; but this is not the case. It may be interesting and useful to examine the variations, especially if this examination shall throw any light upon the actual value of the constant L, as determined by Regnault.

In the "Report of the Committee appointed to Test Steam Boilers at the American Intitute Exhibition, 1871," the results are reduced to equivalent evaportion by the aid of a constant, L=966.6.

In the "Reports of the committee of the Franklin Institute on the Horse Power of Steam Boilers, Philadelphia, 1874," and in the boiler tests made at the 27th Exhibition of American Manufacturers, Franklin Institute, 1874, the value of the constant, L, was taken as 966.

In the Centennial boiler tests, Philadelphia, 1876, the reductions to equivalent evaporation were made with a constant, L = 965.7.

Results of equivalent evaporation obtained in boiler tests are frequently calculated to three decimal places; so that the value of the constants used in the calculations is a matter of some importance. This may be illustrated by an example.

In the report of a certain boiler trial, it was stated, that the evaporation produced by the consumption of 12,114.5 pounds of combustible was equivalent to 131,673,893 British thermal units; and using a value of L=965.7, the equivalent evaporation per pound of combustible was found to be 11.255 pounds. If L=966, this equivalent evaporation becomes 11.252 pounds; and for L=966.6, the equivalent evaporation is 11.245 pounds.

The values of L, as given in several well-known steam tables, all of which tables are professedly calculated from Regnault's results, are shown in Table I. It appears from this table, that Regnault himself makes L=965.7; and from this fact, it might be inferred, that any one who presented a table containing a different value of L, did not correctly report Regnault's results. But a distinction must be made between the results calculated by Regnault, and the actual results shown by some of his experiments. Regnault, in his report, describes in detail the manner in which he conducted his various experiments, gives all the observations, and explains his method of calculating the results from the observations. After calculating the several results of a series of experiments, he determines the equation of a curve which, in his opinion, agrees most closely with the series of results. By the aid of the equations thus determined, he calculates tables intended to show the results of his several series of experiments; and it is from these tables, apparently, that most of the mod-

ern steam tables have been deduced. Whether or not the calculated results are more accurate than the results of observation, will be briefly discussed.

Referring to Table I, it will be seen that, with one exception, all of the steam tables quoted give the same value for the total heat of evaporation above 32°; and

TABLE I.

BRITISH THERMAL UNITS IN A POUND OF STEAM EVAPORATED FROM AND AT 212° FAHRENHEIT, AND THERMAL UNITS IN A POUND OF WATER AT THE TEMPERATURE OF 212° FAHRENHEIT.

Authority.	Latent Heat of Evaporat'n.			Heat of Water above 32° Fahrenheit.	Total Heat of Evaporation above 32° Fahrenheit.
	Internal Latent Heat.	External Latent Heat.	Total Latent Heat. L.		
H. V. Regnault, in "Mémoires de l'Académie Royale des Sciences. Paris, 1847." C. T. Porter, in "Treatise on the Richards Steam Engine Indicator. New York, 1874."	—	—	965.7	180.9	1146.6
W. J. M. Rankine, in "A Manual of the Steam Engine and other Prime Movers. London, 1866."	—	—	966.1	180.5	1146.6
R. H. Buel,* in Weisbach's "Heat, Steam and Steam Engines. New York, 1878."	893.894	72.175	966.069	180.531	1146.6
B. F. Isherwood, in "Experimental Researches in Steam Engineering. Philadelphia, 1865."	893.666	72.034	965.7	180.9	1146.6
W. P. Trowbridge, in "Heat as a Source of Power. New York, 1874."	893.34	72.36	965.7	180.9	1146.6
R. H. Thurston, in "The American Cyclopædia. New York, 1876."† (1)	892.9	72.3	965.2	180.9	1146.1
(2)	—	—	966.6	180.	1146.6
(3)	—	—	965.988	180.612	1146.6

* Also in "Appleton's Cyclopædia of Applied Mechanics. New York, 1880." And in Röntgen's "Principles of Thermodynamics. New York, 1880."

† The quantities in lines (1) and (2) are copied from two tables that are contained in the article quoted; and the quantities in line (3) have been calculated by the writer from two formulas in the article for the latent and total heat of steam at any temperature.

that the different values of L, in Table I, are due to variations in the number of British thermal units required to raise the temperature of a pound of water from 32° to 212°. Regnault made forty experiments on the specific heat of water, at temperatures varying from 225.86° to 374.648° Fahrenheit, using a calorimeter in which water at a high temperature was mixed with cold water, and the resulting temperature noted. From these experiments, he concluded that the mean specific heat of water was 1.005, between 32° and 212°; and 1.016, between 32° and 392°; and from these two assumptions, he determined the constants of a formula for calculating the heat in thermal units required to raise the temperature of a given weight of water from 32° to any higher temperature, T. By means of this formula, he calculated a table showing the specific heat of water from 0° to 230° Centigrade. It seems probable, as will presently appear, that the data selected by Regnault to establish his formula were not justified by the experimental results.

At a meeting of the Royal Society of Edinburgh, Dec. 15, 1851 (see Transactions of that Society, vol. XX.), Prof. Rankine read a paper entitled: "On the Computation of the Specific Heat of Liquid Water at Various Temperatures, from the Experiments of M. Regnault. Correction of M. Regnault's Experiments for the Effect of Agitation."

The nature of the correction made by Prof. Rankine is explained by the following extract from his paper:

Table II.

Regnault's Experiments on the Specific Heat of Water at Different Temperatures.

Original temperature of water in Calorimeter, Fahrenheit Degrees. T_1.	Final temperature of water in Calorimeter, Fahrenheit Degrees. T_2.	Temperature of water admitted from boiler into Calorimeter, Fahrenheit Degrees. T_3.	Ratio of Mean Specific Heat of water between T_3 and T_2 to its mean Specific Heat between T_2 and T_1. As Calculated from Experim't. By Regnault.	As Calculated from Experim't. By Rankine.	As Calculated by Formula. Calculated by writer, from Regnault's Formula.	As Calculated by Formula. Calculated by Rankine, from his Formula.
53.546	69.386	226.022	1.00384	1.00375	1.00594	1.00409
47.102	63.86	228.722	1.00680	1.00655	1.00629	1.00414
55.328	79.354	319.532	1.00871	1.00779	1.01170	1.00959
48.11	75.092	342.842	1.01140	1.01019	1.01333	1.01055
55.346	83.642	367.718	1.01581	1.01419	1.01533	1.01248

Table III.

Heat of Steam and Water, in British Thermal Units, at Different Temperatures.

Temperature of Steam, Fahrenheit Degrees. T.	British Thermal Units per Pound. Rankine. In water, above 32° Fahrenheit H.	Rankine. Latent heat of Evaporation. L.	Rankine. Total Heat of Evaporation above 32° Fahren. H+L.	Regnault. In water, above 32° Fahrenheit H′.	Regnault. Latent heat of Evaporation. L′.	Regnault. Total heat of Evaporat'n above 32° Fahrenheit. H′+L′.	Regnault. Total heat of Evaporat'n above 32° Fahrenheit. As Calculat'd from Formula.
32	0.	1091.7	1091.7	0.	1091.7	1091.7	1091.7
50	18.	1079.19	1097.19	18.0036	1079.1	1097.1036	1097.1
68	36.	1066.68	1102.68	36.018	1066 68	1102.698	1102.68
86	54.018	1054.152	1108.17	54.0468	1054.26	1108.3068	1108.26
104	72.036	1041.624	1113.66	72.0918	1041.66	1113.7518	1113.66
122	90.054	1029.096	1119 15	90.1566	1028.88	1190.0366	1119.06
140	108.108	1016.532	1124.64	108.2466	1016.46	1124.7066	1124.64
158	126.18	1003.95	1130.13	126.378	1003.68	1130.058	1130.04
176	144.252	991.368	1135.62	144.5076	991.08	1135.5876	1135.62
194	162.378	978.732	1141.11	162.6858	978 3	1140.9858	1141.02
212	180.522	966.078	1146.6	180.9	965.7	1146.6	1146.6
230	198.72	953.37	1152.09	199.1538	952.92	1152.0738	1152.
248	216.918	940.662	1157.58	217.4508	940.14	1157.5908	1157.58
266	235.188	927.882	1163.07	235.7946	927.18	1162.9746	1162.98
284	253.512	915.048	1168.56	254.187	914.4	1168.587	1168.56
302	271.872	902.178	1174.05	272.6316	901.26	1173.8916	1173.96
320	290.268	889.272	1179.54	291.1338	888.48	1179.6138	1179.54
338	308.754	876.276	1185.03	309.6936	875.16	1184.8536	1184.94
356	327 276	863.244	1190.52	328.3164	862.2	1190.5164	1190.52
374	345.87	850.14	1196 01	347.0022	848.88	1195.8822	1195.92
392	364.5	837.	1201.5	365.76	835.74	1201.5	1201.5
410	383.256	823.734	1206.99	384.588	822.24	1206.828	1206.9
428	402.048	810.432	1212.48	403.4916	808.92	1212.4116	1212.48
446	420.912	797.058	1217.97	422.4744	795.42	1217.8944	1217.88

"The discovery by Mr. Joule of the fact, that mechanical power expended in the agitation of liquids is converted into heat as the visible agitation subsides, renders a certain correction necessary in calculating the results of experiments on specific heat in which such agitation has occurred.

"Of this kind are the experiments of M. Regnault on the apparent specific heat of liquid water at different temperatures. Water at a high temperature, T_3, was emitted from a boiler into a calorimeter containing water at a low temperature, T_1, and the resulting intermediate temperature of the whole mass, T_2, was used as the means of calculating the ratio of the mean specific heat of water between T_3 and T_2 to its mean specific heat between T_2 and T_1. Now, the upper part of the boiler contained steam at a high pressure, so that the hot water was expelled with great force. The vis-viva thus communicated to the water, having been converted by fluid friction into heat, ought to be allowed for in computing the results of the experiment."

After determining the formula for calculating the results from Regnault's data, Prof. Rankine arranges several of the experiments in groups each consisting of experiments in which the temperatures were about the same, and calculates the mean specific heat for each group. From these results, he determines the constants of an empirical formula. Table II. gives the principal data of the experiments selected by Prof. Rankine, and the specific heat, as calculated from these experiments both by Regnault and by Rankine. From this table, the reader can judge, which of the two empirical formulas—Regnault's or Rankine's—most nearly represents the results of the experiments, and whether or not the assumptions on which Regnault's empirical formula is founded are justified by the experiments.

Having calculated a table of the quantities of heat required to raise the temperature of a pound of water from 32° to higher temperatures, T, Regnault states that the differences between these quantities of heat and the corresponding total quantities of heat above 32° per pound of steam (which latter quantities are given in a preceding portion of his report,) constitute the quantities of latent heat per pound of steam, at the different temperature. Table III. contains the latent heat of steam at different temperatures, as given in Regnault's report, and other quantities of heat, as indicated by the several headings. This table is designed to still further illustrate the fact that the calculated results in Regnault's report are, in some cases, less reliable than the experimental results.

In table III., the column headed H+L, contains the total quantities of heat in steam at different temperatures, as calculated from Regnault's formula; and the last column of the table contains the results of Regnault's calculations of the same quantities by the same formula. Wherever there are differences between corresponding quantities in the two columns, these differences are due to errors in Regnault's calculations, or to misprints, as any one can easily ascertain, by observing that Regnault's formula requires the successive differences to be $0.305 \times 18 = 5.49$ British thermal units.

The column headed L′, contains the quantities of latent heat, as given by Regnault; and the column headed, H′, contains the heat of water, on the same authority. From these two columns, the column headed H′+L′, has been calculated, and it will be seen that the quantities in this column do not in all cases agree with the corresponding quantities in the last column of Table III.

For the reasons stated in this article, it seems proper to conclude:

1st. That in reducing the results of boiler tests to equivalent evaporation, the reduction should be made with the constant, L=966.1, to agree with the results of Regnault's experiments.

2d. That the formulas given by Prof. Rankine, for the specific heat of water and latent heat of steam, at different temperatures, more accurately represent the results of Regnault's experiments than the formulas given by Regnault himself.

THE producers of petroleum on the western shore of the Caspian Sea, it is said, have been seriously contemplating laying a pipe-line entirely across Persia to the Persian Gulf. If this were done, they claim that they would have the Asiatic market to themselves. This pipe-line would have to be something more than seven hundred miles long to reach the coast; and as it would for a long distance pass through a territory of savage Kurds, and other Nomadic tribes, it is feared that it could not easily be kept in operation.

EMERY WHEELS AND EMERY WHEEL MACHINERY.*

By W. O. ROOPER.

From "Iron."

Emery is a mixture of corundum and oxide of iron; corundum itself is alumina, with a little silica. Sapphire and ruby are corundum in its purest form, slightly tinged with iron oxide. In a less pure state corundum is found in many places, and is then valuable only for commercial purposes. The emery beds of most importance are those situated in Turkey and Asia Minor; considerable quantities of emery are found near Smyrna and Ephesus. The material takes its name from Cape Emeri, a promontory in the Isle of Naxos, where there are considerable deposits. In 1871 large beds of corundum were discovered in North Carolina, their existence having been betokened by the presence of numerous water-worn pebbles in the beds of the adjacent rivers. These beds have been worked commercially, and the corundum extracted, crushed, and used for grinding and polishing. Emery is obtained from surface workings, and the presence of the emery is generally indicated by the dark red color of the ground above it. The prospectors strike steel rods sharply into the ground where they expect the existence of emery, and by examining the points of the rods can easily detect its presence. The mineral is broken up into lumps weighing about 1 cwt. each. If hard to break, a fire is lighted around the refractory lump, and on cooling, blows are resorted to again. The lumps are then carried to the coast by railway or on muleback, and the material reaches England chiefly as ballast; thus freight is an insignificant item in the cost. The Turkish and Greek Governments generally let the right to raise and dispose of the emery to English capitalists. The specific gravity of emery ranges from 3.75 to 4.28, and the color from dark grey to black; there is an impure variety of corundum blue grey to brown in color. Much of the commercial emery is artificially colored, and is often mixed to a small extent with ground iron slag; some buyers insist upon the adulteration. Analysis of emery shows that it is composed of from 60 to 80 per cent. of alumina, 8 to 33 per cent. of iron oxide, together with a small quantity of lime water and silica. Taking sapphires as a standard at 100, corundum from the Carolina mine has an abrasive power of from 90 to 97, and the best Naxos emery from 40 to 57 only. This would make it appear as if corundum were far superior to emery for grinding purposes. However, this is not the case, on account of the peculiarity of the fracture of the latter. Every grain of emery breaks with a rough conchoidal surface, and presents numerous sharp points, be the lump large or small; even flour emery exhibits this markedly when examined under the microscope. Corundum on the other hand, although breaking into pieces with many sharp points, exhibits also plain and, curved surfaces. It is owing to this that corundum is not more extensively employed in the manufacture of grinding wheels. Thus, emery will cut its way into the material operated on, while corundum will grind and tear, and only perform the work in consequence of its being of a harder nature.

The next point to consider is the preparation of emery on its arrival in this country. It is first treated in the emery mills; stone crushers reduce the large lumps to pieces about the size of a hen's egg, and these in turn are reduced in a stamping mill, or are ground in a horizontal revolving mill with cast-iron circular grinding plates. Naturally the life of these plates is not long, and frequent renewals are necessary. The fragments of emery are then passed between chilled cast-iron rolls and ground or crushed again, then sifted through sieves. During these operations a portion of the emery is separated from the lumps in the form of dust, which collects on the top of beams and other places. This emery dust is all carefully preserved, and, under the name of flour emery, is useful for

* Paper read and discussed at a meeting of the students of the Institution of Civil Engineers; awarded a Miller prize.

polishing. Levigation is another process employed for separating the coarse grains from the fine emery. Six cylinders, about 3 feet high, are ranged in a row, and are in communication with one another at the top by means of metal pipes. Water mixed with emery from the rolls is introduced into the first vessel. The heavy grains sink, while the lighter ones, remaining in suspension, pass over into the next vessel. The process is repeated throughout the range, the last vessel containing the finest emery. There are now in this country a fair number of emery mills, but in America the trade is of greater magnitude and more importance. Emery crushers generally manufacture emery cloth as a branch of their business, as also the flour emery. The chief use of flour emery is for making knife polish, of which enormous quantities are sold. By far the largest demand for grain or crushed emery is for the manufacture of emery grinding-wheels, which of late years have become familiar objects in most engineers' shops. Emery-wheel makers in this country do not, as a rule, crush emery. During the last ten years many classes of emery wheels have been introduced.

The emery wheels best known in England now are the "silicate," or "grey" wheel, the "vulcanite," or "black" wheel, "shellac," or "red" wheel, the "union" wheel, and the "tanite" wheel. The silicate wheel was invented by Mr. Frederick Ransome, Assoc. Inst. C. E., who has perfected the method of manufacture. He employed silicate of soda as the base of the wheel; emery wheels were made for some time by the Ransome Patent Stone Company, in conjunction with their artificial stone and grindstone business. The process consists first in the preparation of the silicate of soda. Beach flints, mostly brought over from France, and used in preference to English flints on account of their being associated with less chalk, are placed in a steam-jacketed boiler; caustic soda is added and steam turned on. After some hours the dissolution of the flints is complete, very little solid residue being left. The liquid is then allowed to settle and run into a tank, where it is evaporated down to a consistency much like that of treacle, but as clear as sherry. This will remain fluid if properly made. Emery of the required degree of fineness is united with this silicate of soda in a mill and a thorough mixing obtained; this "pug" is then pressed into moulds by hand pressure. The disc produced would naturally harden by itself in time, but in order to save time it is baked, and is then complete and ready for use. The manufacture of black or vulcanite emery wheels requires heavy and costly machinery. The base is india-rubber, oxidized oil, or other similar material. This is intimately mixed with the emery in steam-heated rolls (horizontal hollow cylinders); afterwards calendered to exact dimensions, and squeezed into moulds in a powerful hydraulic press. The discs are then cured in the same manner as vulcanized india-rubber goods, after which they are ready for use. What is known as the red wheel is made with shellac as a base, and although the process is now considered old-fashioned, excellent wheels for some classes of work are the result. Shellac is heated in an iron pot; the emery, which has been previously heated is introduced, and the whole is well mixed by hand with iron spoons. The pug is then put into moulds and squeezed in a hydraulic press; afterwards the discs are baked at a low temperature. The tanite and union emery wheels are made in America; the methods employed in their manufacture are kept secret and jealously guarded. In the tanite wheel a solution of leather, and in the union wheels oxychloride of magnesium, are the binding materials. The tanite wheel has the reputation of being the best made in America.

Emery wheels have been described as circular files which never grow dull. As previously mentioned, the emery grains throughout the wheel retain their cutting power; and if these grains are cemented together with a proper binding material, an emery wheel can be worked until 90 per cent. of its original weight has been worn off. A file is useless before 5 per cent. of its weight has been used up, and when being worked is driven by manual labor at a speed of 90 feet per minute. An emery wheel penetrates without difficulty into the hardest metals, some wheels even cutting chilled iron or hardened steel more easily than soft iron, and it is

driven by steam at a peripheral velocity of about one mile per minute. In using emery wheels successfully, it must be borne in mind that as the wheel wears away the revolutions per minute must be increased, so that as nearly as possible the same surface speed may be maintained upon the periphery of the wheel. The economy of emery wheels in the operation of fettling or trimming castings is very marked, and this relates not only to economy in tools, but also in the saving of time. American foundries, and many English ones, are well supplied with emery fettling machines. An emery wheel keeps its size and shape far longer than a grindstone, and very little practice is required to use it successfully. It is a common thing to see a grindstone, 24 inches to 18 inches in diameter and 18 inches wide, slowly rotating in a trough, while men push and shove their tools against it to make any impression. It has been stated by an eminent firm of engineers in London that an emery disc as a tool-grinder lasts out six 4-feet grindstones, and only requires trueing once a month, against the weekly trueing up of a stone. They calculate the cost of keeping an emery wheel tool grinder in order as one-fifth of that formerly expended upon the maintenance of grindstones, and the time occupied in grinding tools one-half of that formerly required. In grinding tools, if very large quantities of metal have to be removed, it is best to grind them quickly, and to ignore their temper, rehardening them afterwards; but if only slight amounts, then the grinding should be gentle, and their temper will not suffer. Carpenters' planes can be ground by an emery wheel with ease after a little practice.

In this country many persons have failed in the use of the emery wheel, owing to insufficient knowledge. Experienced men, who will take many precautions in fixing a lathe or planing machine, will erect an emery machine anyhow, and then allow it to be used by anyone. A very simple machine is sufficient, but it must be rigid, on a good foundation, having long bearings protected from dust, and must be so steady when in use as to be absolutely without tremor or vibration. A wheel should be mounted true and between two washers, screwed up moderately tight, never by driving the wheel on to a square spindle with a mallet, and tightening up the nut with a long-handled spanner. Care should be taken to run the wheel so that the nut tends to tighten itself upon the spindle. Attention should be given to keep the emery wheel true. In large factories one man should have charge of all the emery wheels, and should make his rounds at intervals to true the emery wheels with a diamond tool. This tool consists of a small black diamond set in the end of a ½-inch steel bar, about 8 inches long, fastened on a handle. In trueing, the rest is fixed close to the wheel, and on a level with its center. The diamond tool is then held firmly on the rest, and the point is gradually worked backwards and forwards until the projecting parts are removed and the wheel runs true. The difference in working with a true wheel and one running out of truth can hardly be imagined by those unaccustomed to emery-wheel grinding. In the latter case grinding is a disagreeable duty, and the jar and vibration render the task of holding on the work very irksome. It is not by heavy pressure that the most and quickest grinding will be effected; the article merely requires holding up well and constantly against the wheel. Intelligent users soon get accustomed to feel the bite. The most economical speed for emery wheels is about 4,000 to 5,000 feet of circumference per minute. Some wheels are driven faster than this, and with good results; but owing to the jar which occurs if the wheel runs out of truth in the slightest degree, and to the necessity of such rigid foundations, a speed of 5,000 feet is seldom exceeded with success. There is, however, no doubt that the faster a wheel is run the more is the work accomplished. Too much pressure on certain classes of emery wheels induces heat; and the melting of the india-rubber or other substance used as the cementing medium produces a glaze upon the cutting surface. By changing the metal being ground, as, for example, copper after iron, this glaze may generally be removed. In the case of wheels made with silicate of soda, or oxychloride of magnesium as the cementing medium, pressure only raises the temperature of the work, and does not injuriously affect the wheel. Experi-

ments have been made with a view of ascertaining the utmost speed at which an emery wheel would run without bursting, but all wheels tried have stood the utmost test applied. On the rare occasions when wheels have burst, the cause has invariably been traced to unsteady bearings, improper adjustment of the wheel in the first instance, or an article being jammed between the wheel and the rest. But it is well known that far fewer accidents arise from the bursting of emery wheels than from the bursting of ordinary grindstones. It must not be forgotten that as different shapes and varieties of steel tools are necessary for different operations upon a lathe or planing machine, so different classes of emery wheels are required for different purposes, and it is the best economy thus to apply them. One class of wheel should be kept for one class of work whenever practicable. Wheels can be made hard or soft, rough or smooth, slow cutting or quick cutting; as a rule, fast-cutting wheels have a rough surface, and slow cutting a fine one. As in the case of an ordinary grindstone, the faster a wheel cuts the faster it will wear away. A wheel can be made so hard, that after six months' usage no wear to speak of will have taken place; but the amount of work such a wheel would have turned out would be infinitesimal, compared with that which a softer wheel would have done, and the money and time lost in working the hard wheel would have purchased many soft ones.

In the machinery for mounting emery wheels, first comes the tool grinder, a common object in large works, and invaluable for putting a fine cutting edge on tools, grinding them to template, or reducing them quickly. The emery wheel, generally of a fine grade, is mounted in a small cast-iron trough cast on a pedestal, extra long bearings are provided, and a cap which will completely cover the upper half of the wheel, excepting 3 or 4 inches where the rest is fixed, a little above the center of the wheel level. A continuous water supply is provided by means of a small pump, and a tank within the pedestal. The cap serves to keep the water from flying off the wheel whilst rotating. The emery surfacing machine has only been introduced lately. It is probably an English idea, although modifications have been introduced from America. A cast-iron box, with a top movable in a vertical direction, has long bearings across it. In these runs the spindle, upon which the emery wheel is fixed. The movable top of the box has a hole cast or ground in it, just large enough to admit a small portion of the periphery of the emery wheel to protrude, so that the wheel is otherwise entirely boxed in. A small fan revolves at the bottom of the box, and collects all the dust and cuttings which follow the wheel, thus helping to keep the surface of the table clean. By means of a screw adjustment the table can be raised or lowered, and a greater or less amount of wheel exposed. The article to be ground is passed backwards and forwards on the table top over the protruding part of the wheel. As the table is exactly true, only the projecting parts of the article can be touched by the emery wheel, and thus flat surfaces are rapidly attained. A steel key, which would take a fitter half an hour to complete, can be ground true by an emery wheel in five minutes. The top of the table should consist of a ground chilled plate. Slide bars for locomotive engines, after being case-hardened, are generally ground true in a special machine. The bar is cramped down in a horizontal trough which moves backwards and forwards under the emery wheel. This, with its spindle, runs at right angles to and above the trough. The bearings are supported by horns from the machine standards. The emery wheel spindle, in addition to its rotatory motion, has a cross travel, so that during the forward and return journeys of the slide bar every portion of it comes into contact with the emery wheel. The cloth has a reciprocating motion like a planing-machine table. Dust from the wheel and slide bar gets into the bearings and soon wears them down, so that great attention is necessary. Mr. W. Adams, M. Inst. C. E., of the London and South Western Railway at Nine Elms, put in some metallined bearings which have answered admirably, and have run without oil or renewal for four years. During that time his machine has surfaced seven thousand and forty slide bars, and the wheel which was twenty-four inches in diameter at starting, has only worn down to twenty-two inches.

The lateral travel of the machine is 3½ inches, and it moves across once to about every thirty revolutions of the spindle. Crank pins, after case-hardening, are trued up at the Nine Elms works by means of emery wheels. A pin is placed in an ordinary lathe, and driven at a low speed. A 14-inch by 2-inch emery wheel is fitted up solidly on the slide rest, and is driven off a wide drum on a counter shaft, the small pulley on the emery-wheel spindle being provided with flanges to keep the driving strap on. Case-hardened and chilled rolls are trued up in the same manner, and this method of removing the superfluous metal from such hard material has proved very successful, not the least advantage being that no stoppages for fettling tools are necessary. In railway and repairing shops, where new ends are brazed upon old boiler tubes, emery wheels are useful for removing the scurf from the joint, and reducing it to the required diameter.

Pulley grinding is another common use to which emery wheels are put. Cast-iron pulleys are often ground just so that their hard surface is removed, and a tool can enter without being injured immediately. Wrought-iron pulleys are almost invariably ground in a large surfacing lathe, the emery wheel running on the rest and being driven from an overhead drum. This is no doubt an economical way of getting up wrought-iron pulleys, and through its adoption lighter iron can be used for the rim of the pulley than if it were going to be turned true with a tool. The long knife emery-wheel sharpener is for grinding and sharpening the knives used by paper makers, tobacco cutters, and bookbinders. The knife is fixed in a slide rest, which has a self-acting rectilinear motion in front of the emery wheel (generally here a large-sized one), which runs in long bearings at the back of the machine with its spindle parallel to the knife. As the knife is only ground by the periphery of the wheel, a certain concavity dependent upon the size of the wheel is produced upon the edge of the knife. This concavity is so small that it is ordinarily disregarded. In an American machine recently introduced this defect is remedied by using an emery cup wheel, fitted on to the end of the shaft, which runs at right angles to the knife, and thus performs the grinding with its face and produces a flat surface. The ordinary fettling machine for foundry and yard work consists of a heavy vertical cast-iron box, 2 feet by 3 feet by 2½ feet high, with two long bearings on the top, and a steel 1¾-inch spindle running in them. The driving pulley is often keyed on the spindle between the bearings, and two heavy emery wheels overhang on each side. Sometimes one emery wheel is fixed in the middle of the machine between the bearings and pulley outside. Such a machine is invaluable for dressing heavy castings, and is also useful in girder work for taking off the arisses left by the saw, and for cutting the bevel of the T-iron stiffeners. A smith takes from three and a-half to four minutes to cut the bevel of a 3-inch by 4-inch T, and a man can grind off the bevel with an emery wheel in from one to one and a-half minute, saving at the same time the wages of a striker, and the cost of repairing sets. Smaller fettling machines are made for lighter work, and these are generally bolted down to the bench, or on to a stand made for them. Agricultural and other engineers who have a variety of small articles to get up find emery wheels very useful.

Coming to lighter work, the emery tape machine is a handy tool. It consists of an upright frame 5 feet high, with a stout spindle running in one long bearing 12 inches from the ground. At one end of this spindle are fixed the fast and loose pulleys for driving, and at the other end the emery-band driving pulley, 2 feet or more in diameter, and flanged, of a width corresponding to the width of the largest emery band it may be desired to use. Guide pulleys to hold the band up to its work are provided on the side of the machine, and an adjustable flanged pulley for tightening the band at the top of the frame. The band consists of an endless tape prepared exactly in the same way as emery cloth. It is made in various widths. The driving pulley is generally urged so that the emery band has a surface speed of 3,000 feet per minute. Implements of irregular forms can be readily polished on this machine, and the band can be worked either wet or dry. Emery-wheel saw sharpeners are now familiar objects in

most saw mills, and their economy over the old process of sharpening by hand is obvious. In conclusion, it may be well to mention that, in order to ensure success in the use of emery wheels, plenty of power must be provided. No experiments, it is believed, have been made with the object of ascertaining the exact power required to drive any particular size or class of emery wheel; but it may be assumed that a 14-inch emery wheel will take as much power to drive it properly as a large lathe. Small as is the use of emery wheels at the present time, it will not be rash to predict that in a few years every engineer's shop in this country will be well supplied with emery-wheel machinery.

DRAINAGE UNDER DWELLINGS.

By S. FLINT CLARKSON.

From "The Builder."

However desirable it may be to avoid drains under dwellings altogether, back drainage is impossible very frequently for terrace houses—those houses put close together, which would always line the streets in the central portions of our towns. Anything which was desirable for other drains in less important situations might be considered absolutely necessary for those under dwellings, which should, of course, be as near perfection as possible.

1. *What to avoid.*—By pointing out briefly the bad qualities of brick drains, such as used to carry away the refuse matter from dwellings, what are good qualities in drains generally may be perceived without an effort. New brick drains are rarely constructed now-a-days, but plenty still exist under and around about old houses, so that they are not as yet mere matters of antiquarian interest.

In brick drains (and in drains of rubble stone equally) the materials were porous, absorbing liquid foulness, and giving it out in foul air when stirred, half dry, or dry. The bottoms were too frequently of bricks laid flat in mortar; the bricks grew loose, and the bed of the channel became a row of little cess-pits. In true barrel drains the round bottom was usually covered with cement, but it was applied with difficulty, and frequently not very smooth. With any slight disturbances the coating cracked, parts peeled off without anybody knowing where. Renewal was out of the question. There were thus always little pools above the porous bricks. Rats worked their way between flat covers and side walls, or enlarged any crack in a barrel. The bad air in the drain found its way into the building, and the rats, too. With bricks, barrels were not made less than 9in. diameter, which we know is too large for an ordinary house drain. Little streams of water turned through large flat-bottomed drains were shallow and slow; and in 9-in. barrels, with rough insides, there was but little improvement. The solid matter was left behind by the liquids; flushing, applied with the most extreme rigor, could not cleanse such drains. They were (1) of porous materials; (2) not smooth inside; (3) with joints too frequent, and soon becoming imperfect; (4) too large; (5) difficult to cleanse.

2. *Stoneware drains.*—All the defects noted above may be avoided if good stoneware pipes are used as they should be. They are manufactured in many places in the United Kingdom, and are not expensive, not nearly so costly as brick drains cemented inside would be. Well-burnt, hard, glazed stoneware pipes absorb no moisture; the vitrified glaze renders them as non-porous (1) as an old-fashioned brown drinking-mug; they do not corrode; they are quite smooth inside (2); once well cleansed the surface is what it was at first.

The fewer the joints the more perfect the drain. If one could be put down all in one piece we should do well, but nature has apparently not arranged for this. In the stoneware drains joints occur at every two feet, and if properly made they are very lasting (3). [These numbers are those of the defects in the

brick drains noted above; the contrast is thus pointed out, and the way in which a defect is overcome.] The length of 2 ft. is convenient for making, firing, conveying and handling; a length of 4-in. pipe weighs about 15 lbs., and a length of 6-in. about 26 lbs. In each batch of pipes there are failures which must be cast aside; they should never be sent out from the works. They may be of insufficient thickness, rough on their surfaces, too brittle, fired too much or too little, not cylindrical, or otherwise defective in form. The pipes are made thicker as the diameter is increased; a 4-in. pipe is $\frac{3}{8}$ in. or $\frac{1}{2}$ in. thick, a 6-in. $\frac{5}{8}$ in., a 12-in. 1 in. If too thin or brittle, the broken pipes will saturate the soil around them with foul matter; if rough on the surface obstructions will occur; in either case the drain will be blocked up. If they are not truly cylindrical in form (or not truly oval in the case of oval pipes), one pipe will stand above another at the joint and stop the flow. An ordinary pipe is constructed with a projecting rim or socket at one end—a faucet into which the plain end of the next pipe fits as a spigot. The inside of the faucet and the outside of the spigot have parallel grooves to give a key to the material introduced to form the joint.

The lowest pipe is laid first with the socket at the highest end. The plain end of the next pipe is placed in that socket, and the space between it and the socket is filled in with a mixture of cement and sand. Clay should not be used for drains under dwellings; there the joints should be as air and watertight and as indestructible as possible. Before the cement has had time to harden, the interior of the pipe is wiped out very carefully. *If this is not thoroughly done, a ridge, or small lumps of cement, will stick up at the joint.* Long hairs, threads, pieces of cloth or cotton stuff will attach themselves to such projections, soil will then cling, and a stoppage be managed sooner or later. To guard against such ridges or knots joints have been treated somewhat as in iron water mains, that is to say, strands of gaskin have been put around the upper pipe, so as to make it fit tightly in the socket, and then cement packing, put to fill up the rest of the socket, cannot reach the interiors of the pipes. Some lodgment results, however, and consequent imperfect cleansing, if the whole space between the two pipes is not solidly filled up with something as hard, or nearly as hard, as the pipes themselves.

3. *Some defects in stoneware drains.*—When pipes are ordered hurriedly and arrive too late, there is sometimes a wish to use those which have come, and not to wait further for the special pipes which ought to have been ordered before. When the changes cannot be made at the junctions, pipes of one diameter should always be joined to pipes of another diameter by diminishing pipes, and in no other way. Patched junctions are painful shows of inefficiency; obstruction comes sooner or later when the filling-up breaks down into the pipe. Right-angled junctions cause trouble; a branch should discharge through a junction at an angle approaching the line of flow of the drain which is entered. When bends are required, but have not been supplied, straight pipes will be used with apologies, "so as to get the work done," unless there is interference. If the curve is of short radius, the spigot ends will actually leave the sockets on their outer sides. Speaking generally, very bad stoneware drains will be of porous pipes, rough on their insides, broken and pieced with cement; some joints gaping, others leaking; some badly made with bad cement, some with projections of the cement inside, not sealed over, laid to curved and irregular lines, with right-angled junctions; the curves made of straight pipes, without diminishing pieces at change of size, and occasionally with larger pipes inserted in the run of smaller ones; without inspection chambers; put on new-made or yielding ground; parts running up hill, and the rest laid to flat and irregular gradients.

4. *Iron pipes and subways.*—Some architects, in certain parts of their best work, use iron pipes with yarn and lead joints, similar to those in water mains, in preference to any stoneware drains. In Paris they are always used, not buried in the ground but exposed to view. In America they are common, and compulsory in some places for drains under dwellings. They are enameled inside, or treated by the Bower-Barff (Rustless Iron) process. Mr. John J. Stevenson, the architect of the new mansions at

Kensington Court, has taken great pains there, and used all the most modern sanitary appliances. He has kindly lent me a drawing showing the system pursued. Heavy cast-iron pipes, 5 in. diameter, are laid in perfectly straight lines under the houses. They are lined with Dr. Angus Smith's composition, a preparation of tar, which gives a smooth and apparently indestructible surface. Joints occur at every six feet, and are thus one-third of the number in a stoneware drain; there are no difficulties with defective cement or the careless use of it. Being much more costly than stoneware pipes, the iron pipes are only used under the houses.

Similar iron pipes were put by Mr. E. C. Robbins on wall brackets in a subway—a kind of sub-basement—in the Museum of Building Appliances, in Maddox Street, Regent Street.

5. *Lines and levels.*—Long straight lines are always preferable. It is more easy to get the levels right and to see that they are so; there are no checks to the flow, which is a very important point with water-borne solid matter; they can be more readily tested at first, and from time to time, and more readily unstopped. At the junction of the straight lengths of the pipe drains, and at any bends, small inspection chambers are put; at the bottom of these a length of half-pipe forms the channel. When the cover is off the character of the flow of the drain is seen at once.

Having determined the lines which the drains are to follow, it is then necessary to settle the inclinations at which they shall be laid, in order that they may convey all effete matter quickly to the sewers, and be self-cleansing. Of course, if there was too much fall, and the slightest check, the solids would remain and the water run away. But too much fall is the rarest thing; not being able to get enough is what we are wont to grumble about. The fall is strictly limited by the depth of the sewer below the lowest floor, and the necessity of keeping the drain well under the floor at the upper end; 2 ft. under the finished floor is considered desirable though we have sometimes to make ourselves contented with less. The least fall approved for 6-in. drains is 1 in 40, that is, 3 in. fall in each 10 ft. of horizontal distance. More is valued if it can be managed, certainly 4-in. drains should have more. It is desirable to have a flow of at least 150 ft. per minute with a shallow stream of water. When drains are laid to flat gradients some special means of flushing them must be used daily.

If the pipes are laid upon yielding ground they will not keep level; some will tip one way and some another, the joints will snap, and sometimes the pipes also, resulting in hills and dales, leaking joints and stoppages. A bed of cement concrete, carefully leveled on the top to the proper fall—a bed of artificial rock, in fact—laid along the whole length will give the pipes a fair chance. In this concrete grooves will be made to receive the lower parts of the pipe sockets, and the whole of the length of each pipe will then rest on an immovable bed.

Refilling the trench must be managed without disturbing the pipes. The hollows under them being very carefully filled up with concrete, it must also be put at the side of the pipes, with a thickness of 6 in. on each side, and then 6 in. over the top. Such a covering of cement concrete is usually stipulated for in by-laws for drains under dwellings, sealing up the pipes altogether as an additional precaution against evil results from defective jointing. It also serves to protect the pipes from displacement by impact on the surfaces above them.

6. *Disconnection from sewer; ventilation; connection with sewer.*—After the house drain has left the house, and before it reaches the sewer, a break is made, and the drain runs past an open space. On the side of this air space, next the sewer, is a water-trap with a good seal, intended to prevent any bad air in the sewer from reaching the air space. If, however, this trap is neglected, or pressed upon a good deal from the sewer, tainted air will not enter the house, but will find its way out of the air space. From the air space fresh air enters the drains under the house, and a current is kept constantly moving through them by arranging ventilating pipes at the higher ends, which shall run up to the top of the building.

Some disconnecting traps are large shaped pieces of stoneware, which shut off the sewer at one end, and receive the house drains at the other. A pipe car-

ried up at the house end supplies the fresh air above the trap, when a grating at the surface of the pavement is objected to. With other traps the construction of a manhole is contemplated. This is a little chamber built up under the pavement of an area, through which the drainage is carried in half pipes of enameled ware. The trap is a siphon or U trap put on the side of the manhole next the sewer. A grating at the surface is sometimes put when there is plenty of space; more usually a flue is constructed and filled in with a ventilator having small mica valves, which rise to admit air into the flue, but refuse to let the air come out. In time of storm there might be a set in the wrong direction—the long upright pipe at the back of the building might carry a rush of air downwards, and it would find vent at the induct and cause annoyance. Protected by these mica valves, the flue is unsuspected, and a moment of rest allows the pent-up air to go upwards according to its wont. The manhole makes inspection of the drains easy; an air-tight iron cover is often put over it.

The ventilating pipes, at the upper ends of the house drains, are of lead, or of galvanized cast iron, well caulked at the joints, and all 4 in. in diameter, or as large as the branches they start from. The soil pipe serving the water-closet is usually extended upwards; being joined at its foot to the house drains without any trap, a current of air passes steadily through drains and pipes. Long branches must have special ventilating pipes; short ones will be cleared of air by the discharges, and supplied with freshened air from the main drain. These upcast exhaust pipes must not finish near windows or cisterns, nor be stopped at the eaves, so that they discharge under the open joints of slating; nor must they stop just above the tops of chimney flues, nor be carried into the flues themselves. If they are, bad air will reach the insides of rooms. Wires, or a perforated finial, or an approved cowl, must be put to keep out birds.

The drain should be connected with the sewer in the upper half, above the line of flow, at the haunch just above the springing. The custom at one time was to put the mouths of the house drains below the water level in the sewers, but this is given up now; the intention was to prevent sewer air entering the drains. Connections must join the sewers obliquely in the direction of the line of flow of the sewer. The pipe-sewer junction blocks invented by Mr. Cockrill are a considerable improvement. Oblique junction blocks and bends are used for brick sewers. Flap-traps are railed at, and still used. The hinged valve allows a passage out from the house drain, but not into it; the flap closes by its own weight when the flow has passed through.

7. *Inspection, flushing and cleaning.*—Other connections with house drains for sinks, baths, rain-water pipes, &c., the traps to them, and the ventilation of pipes and traps, form a branch of our subject not forgotten, but very extensive. The construction, maintenance, cleansing, and the efficient ventilation of sewers might seem another branch. Everybody is interested in it, architects specially so. It is well, however, for everybody to have his own province and do the best he can in it; and architects are content with a province which extends, in large towns, as far as the walls of the sewers, but not beyond.

When the drains are completed, disconnected, connected, and ventilated, they must be examined keenly before they are used, so that if by chance there are defects, they may be remedied. If the lower end of the house drain is plugged, and the pipes were filled with water and left for a few hours, and the level of the water in the testing-bend has not sunk, it has been proved that pipes and joints are sound, that there are no vents for bad air, or cracks through which moisture will run away. The levels of straight drains can be tested by actual measurement, and the effectiveness of the gradient proved by floating down something in a good flush, and noting the time. At cast-iron terminals, with air-tight brass plugs placed in a back area at the upper end of a drain, various tests for soundness and level can easily be applied. With drains in use water mixed with lime is poured in at the end. By the amount and character of the discoloration of the effluent water, before and after flushing, the condition of the insides of the pipes will be judged.

The beautiful arrangement shown by Mr. Hawksley (470, Class 22) for testing house drains and soil pipes with a plumber's force pump and gas-pressure gauges, shows when there is any leakage, and localizes the leakage, too. The traps act as plugs; the ventilating pipes and the end of the house drain next the disconnecting trap must be thoroughly plugged up. The smoke test calls attention to important defects; little holes may, it is true, be plugged up by some chance at the moment when the test is applied. Straw burned in the drain may send smoke all along it, or smoke may be generated in a vessel and forced in by a machine. These appeal mainly to the sight. The peppermint test—a favorite one, on account of the ease with which it is applied—appeals to the sense of smell, as does sulphur burned in a shovel at the mouth of the disconnection chamber. Ether, oil of mint, and other strong smells have been suggested. The difficulty in actual life is in getting anybody to look for defects periodically.

SPEED ON CANALS.

By FRANCIS ROUBILIAC CONDER, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

II.

Mr. J. Evelyn Williams stated that to any one engaged like himself on river and canal engineering, the paper was one of much interest. Comparing the speed on canals with that on the open sea, it was obvious that in a deep open seaway the void formed by the vessel freely filled up again by the water closing in under the stern.

In a contracted shallow channel the movement of the vessel set in motion the entire sectional capacity of the canal, and the void at the stern was filled, in a great measure, by the water flowing backwards through the narrow passage left between the vessel and the sides of the canal; therefore the fulcrum or thrust of the propeller was diminished, and a considerable amount of power was expended without useful effect. The author stated that with a speed of from 8 to 9 miles per hour on the Clyde, a wave was caused 8 or 9 feet in height. Now a wave 8 or 9 feet in height would represent a speed of 14 knots per hour, and require about 24 H.P. per square foot; but as the ends of a vessel were beveled off, this theoretic wave would be very much reduced in height, and the power expended in propulsion would be only a fraction of that due to the wave stated. In short, since the resistance of water to lateral displacement, varied as the square of the velocity with which it was displaced, it followed that the resistance would vary as the square of the sines of the angles formed by the bow and stern with the keel. Suppose a modern fine-lined vessel with a speed of 16 knots per hour, the head wave due to that speed would be about 11 feet; but as the bow and stern had an angle of 15° and 20° respectively with the keel, there was no such wave visible due to the resistance of the vessel. With this form of vessel, it would be found that the resistance fell off to about one-sixth that due to the theoretic head of 11 feet.

Putting R=Resistance.
v=Velocity in feet per second.
W=Weight of a cubic foot of water.
a=Midship section in square feet,

then $R=a\,.\,w\frac{v^2}{2g}$, and putting $W=2g$, or $64\frac{1}{3}$ $R=a\,.\,v^2$. Now if the bow was beveled off to an angle θ with the keel, and the stern to an angle $0'$, then R became

$$R=a\,v^2\ (\sin.^2\theta+\sin.^2 0'),$$

and as power was the product of resistance and speed the H.P. would be

$$\text{H.P.}=\frac{a\,v^3\ (\sin.^2\theta+\sin.^2 0')\ 60}{33{,}000}=$$
$$=\frac{a\,v^3\ (\sin.^2\theta+\sin.^2 0')}{550}$$

For illustration take a run of the R.M.S. "Germanic;" speed 18 miles per hour $=26.4$ feet per second; midship section $=944$ square feet; angle of bow with keel$=15°$; angle of stern$=20°$; then

$$\text{H.P.}=\frac{944\times26.4^3\,(0.0670+0.1169)}{550}$$

$$=5{,}808.$$

The actual indicated H.P. was 5,434.

With reference to the cross-section to adopt for a canal, the semi-elliptical section was no doubt a plausible one in theory; but where the strata were of a yielding character, it was with its curved profile, both with regard to cost and facility of execution, practically an impossible one. The author stated that the cost of the Suez Canal, as constructed, was £143,585 per mile, whereas the cost of the walled semi-elliptical section of the same area would be only £80,682 per mile. The Witham Outfall cut, 3 miles long, and drawing to completion under his supervision, was about the same depth as the Suez Canal, but of much greater sectional area, and with side slopes of 4 to 1, and would cost under £50,000 per mile. He should like the author to state the price at which he calculated the cost of the excavations; and also the amount and price of the walling in the semi-elliptical section, as he could not see how the difference in the total cost could be so great as mentioned.

Mr. Robert Gordon observed that he had never had any opportunity of experimenting directly on matters connected with this branch of hydraulics; but his work for many years had given numerous occasions for observations on several points of interest relating to it. He had repeatedly journeyed for weeks at a time in river steamers of from 200 to 600 tons, and in steam launches, at all stages of water, both on a large river and on small natural channels, as well as on artificial canals; and the phenomena accompanying the retardation of vessels in shoal water and restricted channels had been impresively forced upon him. On one canal in particular, about 7 miles long and about 40 feet broad, near Rangoon, he had passed with barely sufficient water to float the launch, with the tide rising in the same direction from behind the launch till the water became 6 to 8 feet deep. At first not more than 1 mile to 2 miles per hour could be got with full speed, and with the greater depth of water only 5 miles were attained. He had also gone through when the water was 10 feet deep, steering himself, and observing closely the retardation and wave formation as the vessel passed at extreme full speed. The launch had some 50 square feet of immersed cross-section, and could make over 10 miles an hour in deep water; but in the canal, which then had about 500 square feet of cross-section, she could not make quite 7 miles per hour. On applying the author's formula to these data he found the results given somewhat at variance with the facts. The actual retardation of the boat in the canal thus varied from 9 to 5 and 3 miles per hour, while the back current, which the author assumed to be the sole cause of the retardation, was, by his formula, only 0.77 mile per hour in the last case. In actual practice no such general back current was observable in the channel, but a strong commotion was set up in shallow water immediately near the hull, which could be best studied in somewhat analogous conditions when a vessel was anchored in a strong current with less than 3 feet depth of water under her. It would then be seen that strong eddies rose up near the stern, and if the bed of the stream was easily acted on by the water, a considerable excavation of the material just below the vessel occurred. Doubtless the same kind of eddying took place under a vessel in rapid motion in shallow water, and it was disturbance of this kind that interfered with the steering. But in any case it was found that the retardation and deficient steerage power were much more strikingly developed in shoal water with only a few feet between the bottom of the vessel and the bed of the canal, whatever the breadth of the stream might be, than in a restricted narrower section of greater depth. This he had frequently had occasion to observe in river-traveling, the shoaling of the water being indicated by a loss of speed, a commotion in the water, with large waves forming on its surface, and defective steering power. He would ascribe the numerous accidents and stoppages on the Suez Canal entirely to this loss of steering power in the large ocean steamships, with their small rudders, and only a few feet of water intervening between their keels and the bed of the canal;

and he believed that more would be done to facilitate the traffic across the Isthmus of Suez by deepening the canal 6 feet than by widening it 60 feet, as vessels could pass with a higher speed and more security in steering with the increased depth than with greater width.

The form of the canal was of much less importance than the depth. The author referred to Mr. Bazin as saying that the form of the channel must be regarded; but what Mr. Bazin did say was, that the form of a channel did not appear to exercise any importance on the flow, although a completely curvilinear canal might discharge nearly one-tenth more than a polygonal one; yet, as such regular curves were rarely found in practice, the form of section was not taken into account. The author's elliptical section had the disadvantage of offering rather steep sides, which would certainly fall in in such soft material as the Suez Canal traversed, and the nature of the ground must generally determine the shape.

If it was desired to study the theory of the resistances to vessels passing through restricted and shallow channels, the labors of Mr. Scott Russell, as well as those of Bazin and of Froude, should be taken into account, as no one has done more than he had to place the matter on a scientific basis. Mr. Froude had contributed most valuable results to the knowledge of the resistances to vessels in the open sea, in memoirs, where he showed that this resistance depended on two forces, the one which might be called skin-friction forming the whole resistance at low speed; the other, which only came into existence when the speed was increased, might be called the "wave-making resistance." This latter arose from the continuous formation of two kinds of waves as the vessel passed through the water; the one kind consisting of the diverging waves formed at the bow and running off obliquely, but retaining their original size for a great distance, while they dissociated themselves from the vessel and opposed no further resistance than the loss of power due to their original formation. The other kind of waves occurred in series and ran transversely to the course of the ship; the first wave being the greatest had its origin at the bow, and was followed by other waves parallel to it, and which might strike the rear of the ship, while a terminal wave appeared near the stern of the ship restoring the equilibrium disturbed on the formation of the bow waves. Mr. Froude found by experiments on a series of models of ships, all built on the same lines and same cross-section, but with different lengths of parallel-sided middle body, that the resistance at high speed was very much increased in dragging the models through the water, if the hollow, or trough of one of these transverse waves came a short distance in front of the stern-post, and it was much lessened if the crest of the wave struck the counter in the same place. These conclusions of Mr. Froude would help much in understanding the results of some of Mr. Scott Russell's experiments on canals. He was the first to discover the existence of what he termed the primary or carrier wave, a wave of translation, as distinguished from the ordinary wave of undulation or oscillation. He found that in a channel 20 feet long, 1 foot broad, and 1 inch to 7 inches deep, he could by the sudden protrusion of a solid mass, or addition of water, cause a wave to travel along and above the surface of the water at a velocity which equaled the square root of the total height of the crest of the wave above the bottom of the channel. This wave caused an actual transference forward in its own direction of every particle of water in the trough or channel. Mr. Bazin repeated these experiments in a channel more than 20 feet wide and about 8 feet deep, and verified Mr. Scott Russell's results, which he declared to be exact for still water, but only approximate where a current existed. Mr. Scott Russell some years afterwards made further experiments on a canal near Preston, about 30 miles long, in order to study the effects of wave-formation on the resistance to the passage of vessels on canals. He found that by keeping the whole of the boat traffic on the canal in one direction for a day he caused an actual transference of the water in the canal from one end to the other to such an extent, that it was 18 inches deeper than the normal at the one end, and 18 inches shallower at the other. This was entirely due to the formation of the primary or carrier waves, which took their rise at the bow of each vessel, and, after

accumulating to a certain size, shot off at a velocity solely due to the depth of water in the canal, and had no relation to the vessel's speed. In 1 foot depth the carrier waves ran at 4 miles an hour; in 5 feet, 8 miles; in 15 feet, 15 miles; and in 26 feet, 20 miles an hour. A considerable amount of energy was expended in this wave-formation, which corresponded to the diverging waves of Mr. Froude in the open sea.

But another class of waves still more important in their relation to the resistance to vessels in canals, was the secondary class of waves, which appeared to be undulatory or oscillating, and like Mr. Froude's transverse waves in open water, constantly accompanied the vessel in her movements. Mr. Russell had described some of the phenomena of these waves, and had given an account of the form and mode of formation of those he observed. The bow-wave was heaped up in front of the vessel; a negative wave or trough accompanied the body of the vessel, and was followed by another large crest of a wave behind the stern, which showed a tendency to break, and did all or most of the damage recorded of destroyed banks of canals. He had constantly watched these waves form in a marked manner in shallow water, irrespective of the fact whether it was in a narrow canal or in the broad river; and it was to the attendant conditions and results that he would ascribe the great retardation observable in passing from deep to shoal water. Possibly the fact that the negative wave, or trough, acquired such prominence in shallow water, near the after part of the body, might account for a great portion of the retardation. It was well known tbat sternwheelers could not work in very shoal water, as their after part often sank so low as to cause them to touch the bottom. Probably the eddying water under the keel and around the stern explained some of the increased resistance; but it was clear to those who studied the subject in the writings of the eminent men who had already worked at it, that great prominence must be given to the whole of the allied phenomena of wave and eddy formation, particularly in shallow waters; and that both the whole depth of the water and the distance between the keel of the vessel and the bottom of the channel below it must enter as factors in any formula professing to express these phenomena mathematically.

Mr. J. Herbert Latham wished to add one remark to what had been said about the depth of the canal. The bows of the boat when they struck the water drove it in different directions, and it had to escape. This was seen most clearly where a flat board struck the water perpendicularly; the water was driven round the sides and under the bottom. The same thing happened when the striking surface was much inclined, as in the bows of a sharp canal boat. The force and the velocities generated were very much reduced by the water striking at a great inclination. The effect in either case was that the water that escaped upwards created a wave at the top of the surface, which increased to such a height that its weight operated to check the rise of water, and to promote the escape of water underneath the bottom. The progress of the boat in the canal was much facilitated if it was easy for the water to escape underneath the bottom instead of having to pass the whole length of the bows, and so necessitating more head in the wave in order to drive the water at greater velocity. In order to facilitate the escape of the water underneath the bow, and so to allow the wave to be as small as possible, a broad and shallow boat was necessary having underneath a clear waterway in most cases, if it was a sharp bow, larger than would suffice for the sides. The water should be moved as little as possible. As the water was moved along the sides or underneath, it should go on lines parallel to the sides of the canal until it closed again at the rear; and if there was a bulging bottom it was possible that water which escaped underneath the forward part might have to be driven aside amidships, and to be sucked in again at the rear, and so a motion created in the water which not only absorbed power, but interfered with the flow of the water, In some American canal-boats the horizontal section through the broadest part of the boat showed the inclination of the bows 4 to 1. His notion was that such sharp bows were best; and underneath those long sloping bows the water could escape under the bottom. At the rear where the boat began to contract, and before the velocity

of the water was checked, the depth of water would be reduced; in fact a depression would occur at the stern, which saved so much elevation at the bows, and he did not consider that was objectionable; the difference of level between the wave at the head and the depression at the stern representing the head which drove the water past the boat. The object in drawing in the stern should be to allow the water flowing rearward along the sides and under the bottom of boat to take up a position of rest in the canal in rear of the boat with the least commotion possible. In one of the cases quoted by the author, the area of cross-section of canal water was four times that of the boat; that would involve an average backward velocity in the water, as it passed the boat, of about one-third the velocity of the boat; and this water should flow past the boat in behind the stern equably. He believed the best sort of boat was one that had a considerable breadth of stern, and a paddle-wheel in the rear. In short, what he suggested was that the sides of the bow should be fine, that the bilge and lower angle of the bow should be rounded for the escape of the water, and that the stern should be such as to be something like a section of an overflow near the *vena contracta* (an adaptation of which was seen in the interior of a nozzle of a fire-engine), but very much elongated to suit the circumstances. It should be such that the water flowing in from the sides, and the water rising from below, to form the wave that followed the ship would have its velocity checked equally. With regard to leaving waterway under the bottom of a vessel, he would instance the flat boats used on the Godavery. They were square bowed, the whole of the water was driven downwards, the bottom was flat, and they would go 8 miles an hour. If they were crossing a sand-bank the water would suddenly pile up at the bow, being unable to get underneath. There still was water to float in, but it had not been able to escape underneath. With those boats the whole of the water that was displaced had to be sent underneath them. In the Suez Canal the ships must be taken as they were found. They were not built for canal service. but still there was no question that they should have such a depth underneath them, that any water would not have to be driven aside at a high velocity when the bulging part of the vessel passed. If that was done they would derive the full benefit from any widening of the canal, and in his opinion deepening the canal was of much more importance to speed.

Mr. Alfred Giles, M. P., said, when he heard of a bow wave of 11 feet on a canal, and of a wave of 8 or 9 feet on the Clyde being forced up by the speed of a vessel, he was disposed to say that that was quite contrary to his experience. He had had considerable experience in observing the waves thrown up by vessels at high speeds, alluding more particularly to the trial trips of steam vessels, and from his observation he doubted whether a wave as described could be produced by any vessel of ordinary and proper form. He had crossed the North Sea in very rough weather, and he had had the opportunity of measuring the highest wave he could find, and that, he thought, was 22 or 23 feet. He crossed from Calais only the other day, after the late storm, and he did his best to measure the height of the waves from the trough to the crest, and the extreme wave was not more than 9 feet. Very few people knew unless they had tried to measure it what a wave of 9 feet meant, and he doubted whether it was possible to raise a wave of 11 feet by the ordinary passage of a boat in a canal Unless canals were made wider than would be necessary for the ordinary transit of boats, the speed at which boats of an improper form should be allowed to traverse them ought to be regulated. He believed that a boat could be designed to go with the greatest speed through a canal scarcely causing any wave; but if a tub like an old wooden collier were put in the canal, and driven 6 miles an hour, a wave would rise that would do very much damage to the banks. It was not a question of tubs or theoretical vessels; but of vessels of practical and useful shape, such as would be adapted to go through the Suez, Panama, or any other canal, and he thought that a vessel of any ordinary form going through an ordinary canal at 6 or 7 miles an hour need not create any wave which would be damaging to the banks. Of course the greater the speed the greater the wave that would be formed; but taking the ordi-

nary speed at which a vessel would go through a narrow channel, it depended very much upon the form of the ship whether the banks of a canal would suffer damage.

Mr. J. D'A. Samuda considered this one of the most important and interesting subjects that could possibly be brought before any Institution. He thought if the discussion were transferred to the great legislative chamber, the Suez Canal question would be dealt with in a very different manner from that which probably it might be there treated from an imperfect knowledge of the issues involved. In the main, the points which had been urged in the paper appeared to him to be correct. He had no experience of canal work, but he had an experience quite equal to the ordinary amount with reference to the passage of vessels through water, and he must say that there were circumstances in this inland sea which, having once been formed, had become such an important factor not only in the commercial but in the political necessities of the country, as to render it of the utmost importance for the Institution to afford such assistance as might enable that canal to be made useful for the purposes for which it was originally designed. It was perfectly clear that however good the design might have been originally, and however well intentioned the construction of the canal for the use which it was expected would be made of it, it was utterly unsuitable to the traffic which was now passing through it, and would become considerably more so with the increased quantity that every year brought upon it. The owners of the canal had dealt with the matter of its improvement in conjunction with the shipowners of this country from a point of view with which no one could find fault—that was to say from the shipowners' point of view. The shipowners no doubt had a great desire to get the freights reduced, and they had succeeded in showing Sir Ferdinand de Lesseps the advantage that would result by reducing the freights. He, on the other hand, had been prepared to accept their view, probably with the idea of condoning that which he had done, and getting further license to do that which he might want to do in the future. What the public in this country were most interested with was that the canal should be able to accommodate what they required, and considering that India and Australia were becoming our main reliance for the supply of food, especially corn and meat, and with the knowledge that in England there never was more than four months' food at their disposal, and that without every inland sea and ocean route being open to them to bring food they would in many cases be absolutely starved, it was of the utmost importance that the canal should become a real, efficient, and useful thing. It never could be made useful and efficient if, having admitted the total inability of the canal to deal with the traffic passing on it, another canal should be cut and the traffic divided, one canal serving the traffic passing one way, and the other canal the traffic passing in the opposite direction. Such a course seemed to him to be quite out of the question; the author put the matter very clearly in that respect, for he showed distinctly the amount of retardation that vessels were obliged to submit to in passing through the canal from the resistance which they met with, from the shallowness and narrowness. However magnificent the second canal might be in one direction, there would be a limit in the opposite direction owing to the existing canal, even though it was made still deeper than at present. The Institution might therefore most advantageously direct attention to the inconvenience which the public were at present obliged to submit to. What was a speed of 5 miles an hour between two hemispheres? Here there were vessels going regularly at 12 to 14 knots speed reducing their speed to 5 knots, constantly coming to grief, constantly being blocked. Some of his own family had passed through the canal more than once, and on every occasion they had been stopped two or three days by other vessels having got aground, or in some way or other becoming entangled. He did not wish to make this a discussion with reference to the course that the canal should take. There were others present who had already pointed out the enormous advantage which might result from doing away with this little ditch, and substituting for it a sufficiently wide, large, and deep channel for dealing with the traffic that had to pass through it,

and it would be clearly shown that trebling the width would in all ways greatly increase the advantage of passing through, because the resistance would be decreased enormously as the width and depth increased. He constantly had vessels on the Thames, and whenever they had passed over any shoal, something like that which was found in proceeding over Barking Shelf, it seemed as though the vessel had been going at an increased speed, whereas the reverse had taken place. The vessel had been held back seriously by its nearness to the bed of the river, whilst the paddle-wheels had revolved very much more freely by reason of the extra amount of slip which had taken place. He might observe that the whole of Mr. Latham's remarks seemed to be based upon the supposition, that water might be driven in one of two directions; either round the side of the ship, or under the bottom of the ship. It appeared to him utterly impossible that any water could be driven underneath the bottom of the ship. Unless a way could be found by which to compress water, he should like to know how a single particle of water could be driven except round the side of the ship? Therefore the width of the canal was of material importance, so as to afford space to get rid of that water which must be pushed out of the way to enable the ship to go through, and must fall in again to enable the space to be filled up. To pass water underneath the bottom of the vessel, either the water must be compressed, or the ship lifted out of the water, and thus caused to travel supported on a less quantity of water than was due to her displacement, either of which processes were perfectly impossible.

Sir Robert Rawlinson, C. B., as an old canal engineer, might have something to say upon this subject, and as a preliminary he would wish to remark that formulas were very useful, but only as the alphabet, to lead to something beyond. He ventured to say that no single formula could be devised, which should be applicable in all cases to canal and river navigation. There were certain rules, however, which were unerring, as, for instance, the squares of the diameters of pipes, and their capacities; but there was not a single formula which was applicable to the flow of water through pipes of small and large diameters. Many had been promulgated which might be true of the pipes upon which the experiments were made, but they were not true when those diameters were increased. He had heard it suggested that the Suez Canal was to be deepened and widened. Several accounts had been given of the enormous cost of making it what it was, and some intimation was conveyed almost every week of the difficulty in getting vessels through that canal. The public in general might not be capable of understanding how these difficulties arose, and that enormous cost had arisen. The canal, where the difficulties now arose, had been dredged through sand; on the section before the meeting there was shown a level bottom and sloping sides, with shallow top margins for extra width of the water. That section had been objected to; but he had no doubt that the French engineers could give some very good reasons why they had adopted it. But what were the difficulties naturally? Being sand, there was a constant tendency for the excavated channel to close by the rising of the bottom. That bottom being sand took the nature of quicksand, and there was a tendency also of those sides, however flat they were laid, to slip, and so shoal the deeper water. A railway cutting might be taken as a dry canal, and the slopes were laid back according to the nature and character of the material through which the cutting was made. There were slopes at $1\frac{1}{2}$ or at 2 to 1, but through the lias shales and some other formations, the sides slipped in. After intervals of years, those slopes had been a perpetual torment to the engineer. That state of things being so in dry cutting, what must be the state in wet sand cuttings similar to the Suez Canal? He imagined that Sir Ferdinand de Lesseps knew by experience pretty well that if he had to make a canal to give capacity for traffic, he should not attempt to widen that canal to twice its present width; for if he did, he would increase the difficulty of keeping it open fourfold. Sir R. Rawlinson would much rather know that Sir F. de Lesseps was going to content himself with a single line of canal to pass the traffic in one direction, and that he would cut a second canal, if one must be made, for a line of traf-

fic in the other direction, making the cross-sections small in order to reduce the enormous annual costs of dredging. With regard to the speed of boats on canals, canals as constructed in this country were made for slow speeds. When they were constructed the only form of traction was manual or animal power, and the speed was necessarily confined to some 1½ mile or 2 miles an hour. He did not think any one in the room had had more propositions put to him for bringing in new methods of traction upon canals than he had. He used to have a project about every three months to consider and report upon. The projectors, as projectors, did not appear to know that twice two only made four, and not by any possibility make any more. Many of them brought forward their schemes never having studied a canal, never having looked at the traffic going along it, but imagining, in their study or home, that they had hit upon some beautiful theory by which to get impossible speeds upon canals. Mr. Samuda had said that water did not pass under the bottom of a boat in motion. Sir. R. Rawlinson knew to the contrary, as that under some conditions was exactly what it did do, and he would mention why. Upon the Bridgewater Canal the highest speed perhaps ever got upon an inland navigation had to be 9 miles per hour, but this was got by horse-power. The boats were termed "swift boats." They were about 80 feet in length, 6 feet beam, and drew laden about 2 feet 6 inches of water. It was necessary to run those boats at 9 miles an hour, as they could not be run at any other speed, that was to say, they could not be run practically at any other speed than 9 miles an hour; because the wave raised at this speed must be kept about one-third under the bow. There would be a trough ahead and a wave behind; and scores and scores of miles had he steered a boat going at that rate. If the horses failed, the boat slackened, and the wave got away from her bow; the boat must then be stopped, and another wave generated. To show that some different action took place with the swift motion of that boat as compared with the slow traffic upon the canal, large quantities of silt or mud used to require removal every summer under the ordinary slow traffic, but when the swift boat had been established, the bottom was clean. Now, if there had not been a backward motion of the water underneath the boat when moving with that velocity he did not know how there came to be so clean a bottom as he found in the canal. With reference to modes of propulsion, one man, imitating atmospheric railway modes, projected a scheme of laying an iron pipe along the towing-path, filling it with water, having a piston, and towing the boats by running the water in front out down an embankment. That was taken up by the late Lord Ellesmere, and he remembered making a very elaborate report. He thought he did it mischievously, because he went into all the details, ending with the remark that one night's frost would seal it up. When Lord Ellesmere came to that paragraph he said: "Why did not you put that at the beginning?" Another proposition was to pile the canal on both sides and make an elevated railway, to put engine power on the top of this elevated railway, and to draw the ordinary boats, which were 15 feet wide, along at 12, 15, or 20 miles an hour. He went down to see the inventor in Liverpool, and found he was a pattern maker in one of the large foundries. He began to talk to him about the speed that he wished to attain upon the canal, and then said, "Do you know what would come of putting one of our 15-feet boats in motion at anything above 3 miles per hour—do you know what would take place?" The man said "No." "Well," I said, "I will tell you: it would sweep all the water before it out of the canal and ground itself on the bottom." I then said, "Are you aware of the nature of our traffic?" "No." "Do you know in what form the traffic passes along?" "No." "Have you ever seen the canal?" "No, I never have?" So the project was drawn without the man ever having seen the canal. Then methods were projected of towing by steam, but as paddles or screws created a counter-current at all bridge and stop-places, there was a suck and great scour on the banks, so that steam was not in his time adopted. He believed there were on some of the Yorkshire canals trains of boats taken along some 4 or 5 miles per hour by steam; but there was nothing of the kind introduced upon the Bridgewater

canal while he was there. There was small steamer on the old river called the "Jack Sharp," and as it went down in the narrow parts its speed might be from 2 to 3 miles, but when it got into the wider canal at Runcorn, it immediately attained with the same power something like 2 miles more, where it had wider and deeper water. He found that the swift boats put the whole body of water in the canal into motion, and the towing was harder upon the horses with a full canal than when the canal was low, showing that the suction that the boat had upon it in being hauled through the water was greater with the greater volume of water in the canal than when there was a lesser volume. Then, as regarded the wear and tear, it was simply enormous, both in horse-flesh, in towing-lines and in destruction of the canal. The swift-boat waves ran up against the sides, undercut the side walling, and ran over the towing path, so that it had to be protected some 2 feet high with sod-made banks to prevent the wash, and as the swift-boat traction was only put on to stave off railway competition, as soon as it was found that there was no use in attempting to do that, they were abandoned. One interesting experiment was made on a length of canal betwixt Preston and Lancaster (about 30 miles, without a lock), where the authorities thought they would have swift boats. They tried one of the swift boats from morning to night, but could not get the speed. At last they came to the conclusion that there was something in their canal that could not be overcome. Some one, however, suggested that they had better try the horses without the boat, and see if they could go 9 miles an hour when they had nothing to draw. They could not, so that it appeared that they had been working all day endeavoring to get a result which was utterly impossible. Then there had been suggestions for laying chains down on the bottom of the canal, and working from a surging-barrel, and he had been told that day of a plan for having a very heavy chain passing from front to stern, and driving from a wheel in the middle, letting that chain run down, forming a grip upon the bottom of the canal, but he did not know whether that was likely to be a working experiment or not. With regard to the Suez Canal, whatever might be done with duplicating it, it did not need any spirit of prophecy to say that, in its present form and at its existing section, it never would be worked at much less cost than it had been up to the present time, because so long as it was kept open, as a canal, so long must the managers pay the penalty by the same unceasing and costly dredging.

Mr. H. J. Marten said he was obliged to confess to a feeling of disappointment at the meagre amount of practical information contained in the paper. The author had furnished a list of no less than one hundred and seventy-nine English canals and navigations, in connection with which there were of course a great variety of sections, great variations in rises and falls to be encountered, various methods of haulage or propulsion, with various draughts and dead loads in vessels of various descriptions, hauled or propelled either singly or in train, all of which circumstances were more or less incidental to a complete consideration of the question of speed.

Out of this long list of canals and navigations, however, with all this rich variety of incident attaching to them, the author had only given the speed attained with horse-towage upon one English canal (the name of which was not mentioned), and the speed attained with steam-towage upon one other English canal, the Grand Junction; and upon two English river navigations, the Thames and Lee; and this information was unaccompanied by any particulars respecting the sections of the channel, draughts, loads, or descriptions of vessels employed. Instead of practical information upon these points, the author had submitted three or four sections to the Institution, which, except in circumstances of rare occurrence, were, for practical considerations, out of the question. These sections also were founded on a theory unsupported by any experimental test, and which, so far as theory was concerned, left everything an open question, totally unsolved by anything advanced in the paper. He could have wished that the author had spent some little time in conducting a few practical experiments, as they would have added value to the paper, and he was sure the records of them would have

been received by the members of the Institution with the respect and interest always accorded by them to original research. It would also have been of advantage, if in the table headed "Dimensions of Canal Locks" the author had given the draught or depth of each lock in addition to its length and breadth. This information was to a large extent available from public records, and should certainly form part of a table of dimensions.

With regard to the question of speed, Mr. Marten regretted to observe that in the table headed "Speed attained on Canals," the speed attained on English canals by horse-towage was set down as 27 miles a day only, which distance in the body of the paper was expanded to 30 miles a day, including stoppages. He thought that with a little more of that research, with a capacity for which the author was very properly credited, he would have found that the speed he had mentioned was very much less than that actually attained. Taking the case named by Mr. Lloyd—the journey between Birmingham and London—148 miles was performed in sixty hours, or at the rate of nearly sixty miles a day, although within the termini named there was no less than one hundred and fifty-five locks to be passed. This speed was double that stated by the author, assuming the day named by him to represent a day of twenty-four hours, and there was nothing in the paper to indicate any other period. As a further instance, the distance between Autherly Junction, on the Staffordshire and Worcestershire canal, and Ellesmere Port, 68 miles, was covered, including the passing of thirty-three locks, in twenty-eight hours, or at the rate, from terminus to terminus, also of about 60 miles a day. Again, the time occupied in traversing the 25 miles along the Staffordshire and Worcestershire canal between Stourport and Autherly Junction, with an ordinary canal boat loaded with 30 tons and towed by one horse, was twelve and a-half hours. In this length the rise was 293 feet, and thirty-one locks had to be passed; and yet the speed between point and point was at the rate of nearly 50 miles a day. Under these circumstances he felt justified in saying that the statement made in the paper as to the rate of speed attained upon English canals was much below the actual fact. Referring next to the tabulated statement as to speed attained on navigations by steam-towage, he found the speed attained upon the Thames was stated to be at the rate of 5 miles an hour. No particulars were, however, given as to where or under what circumstances this speed was obtained, that was, whether with or against the stream, and whether with or without a cargo or hauling-tug. Hence the value of the information for future reference was limited. With a view to supply somewhat fuller information than that contained in the paper with reference to the speed attained on English inland navigations, he would instance the work done on the Severn. That river had been canalized between Stourport and Gloucester, a distance of 42 miles; upon this portion of the river the traffic was principally worked by steam-tugs, which hauled trains of from twelve to fourteen boats at a time. These boats consisted o "Trows," holding from 100 to 125 tons each, of "barges" holding from 80 to 90 tons each, and of ordinary canal-boats holding from 30 to 40 tons each. The ordinary cargo load in the train of vessels attached to the steam-tugs was between 700 and 800 tons. With a load such as this the distance down the river from Stourport to Gloucester was performed in seven hours. This, including the time occupied in passing the five locks intervening between these places, and all stoppages for taking on and throwing off boats, was at the rate of six miles an hour, or a traveling speed of between 140 and 150 miles a day. Coming up the river or against the stream with a similar load, the first stage of 30 miles' length between Gloucester and Worcester was accomplished in eight hours. This gave a traveling speed, including all stoppages, of 3¾ miles an hour. In this stage the lift in the locks, at ordinary summer level, amounted to 12 feet, or about 5 inches per mile. From Worcester to Stourport the distance was 12 miles, the time occupied four hours, and the traveling speed, including all stoppages, 3 miles an hour. The lock lift on this stage, at ordinary summer level, was 16 feet 9 inches, or about 17 inches per mile. The increased lock lift in this latter stage accounted to a cer-

tain extent for the smaller rate of speed attained upon it, as compared with that attained in the other stage.

Referring to the question of "Lockage retardation," Mr. Marten considered that this was under-estimated by the author. The lockage retardation upon the Wilts and Berks canal, which was the example given in the paper, was stated to be 0.434 minute per foot rise or fall. This, however, was only the retardation due to the mere act of raising or lowering the boat when lying in the lock-pit, and omitted from the calculation all those other factors of retardation which had to be practically taken into account, and which came under the head of "Lockage retardation," such as slowing in, getting under way again, waiting for turns, &c. The estimate in the paper was practically as misleading as an estimate of station retardation on a railway would be which included only the period during which a train was standing at a station between stopping and starting, and which excluded any allowance for slackening and getting up speed, and the other incidental retardations of a stoppage. In following this point a little closer he called attention to the case mentioned by Mr. Lloyd, in which, as compared with the speed attained in the pounds there was a retardation of nineteen hours in the journey between Birmingham and London. Of this period the time actually spent in the locks was estimated at five hours and six minutes; and that occupied in slowing, getting under way, waiting for turns, &c., was estimated at thirteen hours and fifty-four minutes. Taking the total rise and fall upon that canal between the points named at 1,108 feet, and the retardation as amounting to one thousand and forty minutes, the retardation was equal to nearly seven minutes per lock, or to 0.94 minute per foot rise and fall, or more than double the time represented in the paper. Again referring to the journey between Stourport and Autherly Junction on the Staffordshire and Worcestershire canal, the speed of an ordinary canal-boat loaded with 30 tons and towed by one horse, was about 3 miles per hour along the pounds. At this rate the distance, 25 miles, should be done in eight hours and twenty minutes. Owing, however, to the lockage retardation, the time actually occupied was twelve hours and thirty minutes, thus showing the lockage retardation to be four hours and ten minutes, equal to eight minutes per lock, or 0.85 minute per foot rise. A better example of the effect of lockage retardation was, however, to be found in that produced by the flight of twenty-one locks between Autherly Junction and the Birmingham canal. These locks lay within a distance from the top to the bottom lock of 1½ mile. The rise was 15.9 feet, or about 7 feet 7 inches per lock. The average time occupied in passing an ordinary canal-boat through these locks was two and three-quarter hours; and allowing half an hour for the horizontal distance gained, there remained two and a-quarter hours, or one hundred and thirty-five minutes as representing the "lockage retardation." This gave about six and a-half minutes per lock, and as before 0.85 minute retardation per foot rise. From the above examples it would be seen that the lockage retardation upon canals having a rise and fall of from 8 to 12 feet per mile amounted to about one-third of the traveling time. It would also be observed that "lockage retardation" upon any particular canal was due not only to the time actually spent in the lock pits, which would be equal to the total rise and fall multiplied by the time occupied in filling or emptying 1 foot of rise or fall in the locks of that canal, but also to the time occupied at each lock in slowing, getting under way again, waiting for turns, &c., multiplied by the number of locks to be passed; and the sum of these two factors would represent the total "lockage retardation." Mr. Marten stated that, for practical reasons, he did not anticipate much greater speeds than those already attained could be reasonably looked for in the future of inland navigation in this country. In his opinion improvements would have to be made in the direction of lessened "lockage retardation," and in carrying larger loads per boat where the prospects of future traffic were of such a character as to justify the outlay necessary for effecting such a "change of gauge."

Although he was unable to concur with several of the statements contained in the paper, he considered that the author deserved well of the institution in bringing the subject before it, and he trusted

that the ventilation which it had received during the discussion would be of permanent value.

Mr. J. B. Redman said the subject matter of the paper and the discussion thereon were of considerable interest, from two causes: first, the remarkable success of the Suez Canal, notwithstanding all its physical drawbacks; and secondly, the renewed attention which was being directed to the development and improvement of the inland navigations of Great Britain. With reference to the Suez Canal, he referred to a paper, published a few years back, by Captain Steele, the Secretary of the Mercantile Marine Board, who had described the canal as a practical navigator, and who would be able to tell what were the difficulties from a seaman's point of view of navigating the canal in its present condition. A mere inspection of the Admiralty chart would show how very confined the channel was for the vessels now navigating it. The entire length of the canal from sea to sea was 98 statute miles. It had twelve "gares," or passing places, about 6 miles apart on the nothern section, and from 3 to 4 miles apart on the southern. Ten of the "gares" were on the western side, and two on the eastern, which appeared to show that the western side was that which admitted most easily of, and afforded the greatest facility for, widening. But an inspection of the chart would also show that the widening could be carried out on some portions of the canal, but in the narrow parts of the canal it was clear that, to get double the width for vessels to pass each other, it would be necessary to have an entirely new bank on the outer side of one of the existing banks, and constructed before the intervening bank was dredged away, and for the navigation at the same time to be kept open. That, of course, would be a work of some considerable difficulty. Mr. Samuda had stated the disadvantages of duplicating the canal with a canal of similar dimensions, as compared with the existing canal widened, or a canal wide enough to admit of passing vessels on an entirely new route. It was clear that the great drawback to the existing line of canal arose from the frequent stoppages occurring from vessels grounding; and in a canal double the width, or the existing canal widened, it was evident that if a port regulation were adopted, and the vessels outward bound or homeward bound hugged the port side, in the event of an accident happening to a vessel, it would be confined to that vessel, and the navigation could be continued. Again, a canal double the width would admit of the water displaced by the vessel navigating its channel passing freely on either side; the wave raised in front of the vessel would be of less height, and the retardation would be consequently less. There was another point connected with the present route, with regard to the question of widening or an entirely new canal. The soundings in portions of the canal were 27 and 28 feet, but in a large number of places they were only 26 feet and 25 feet; 25 feet was the ruling depth. That applied to the two lakes, the Great Bitter Lake and the Little Bitter Lake. The Great Lake was fully 25 feet deep; but the smaller lake was only half the depth, with a submarine 10-feet channel dredged for the passage of vessels. As to the author's statement that the subject of retardation to the passage of vessels through narrow channels had not been considered until of recent years, Mr. Taunton had referred to a large number of papers published in the transactions of the Institution and elsewhere, and his list commenced with a paper read before the Royal Society as far back as the year 1827, by Mr. James Walker, Past-President Inst. C. E. A deceased member, Mr. George Parker Bidder, assisted in making the experiments. That, however, was not really a case in point. The experiments were made in the East India Dock Import Basin, Blackwall, which was 1,410 feet long, 560 feet wide, and 24 feet deep. The paper published in the Philosophical Transactions of the Royal Society contained drawings of the two boats experimented upon and the apparatus. These experiments showed that the resistance increased as the square of the velocity, and that at higher velocities it was an increasing resistance. But Mr. Walker had distinctly stated that the resistance would be very much greater in a narrow canal. As to the internal navigations of the country, undoubtedly a great deal might be done in developing some of the early works by increased works, doing away with some of the locks, increasing the cuttings, and mak-

ing them more interchangeable than at present; but the subject was so great that it would certainly form a good theme for an interesting paper. Whatever might be thought in reference to some of the positions of the author, the profession generally, and especially those members of it who were interested in canals, were certainly indebted to the author for having brought the subject forward in the manner he had done.

Mr. J. I. Thornycroft thought that in the theory of resistance as expressed by the author's formula, certain incorrect assumptions were made. One was that the velocity in open water was equal to the velocity of a boat passing through the water of a canal; or, its velocity past the land was equal to the velocity through still water, less the speed of current caused by the vessel past the greatest immersed section. The author had also assumed that an ellipse of equal area afforded the same facility of flow as a circle, and stated that the hydraulic mean depth was the same in a semi-ellipse and a semi-circle of the same area. That he believed to be the case when the depths were equal, but not otherwise. The author had also made the incorrect assumption that the friction at the bottom of the canal was greater with increased depth. That might be so in a very infinitesimal way as far as the water was compressible, but water was usually taken, and might be so taken in that case, to be incompressible. The author's reasoning led to this fallacy, namely, that if an indefinitely wide ellipse were used of a given area, the flow would be the same as in a semi-circular section. It appeared to him doubtful whether a semi-circular section was the best for a canal, but of course it was necessary to consider if it was practicable to make a semi-circular section. He was afraid that engineers would complain of the difficulty of keeping up the banks. In order to approach the subject, he thought it would be well to consider the stream-lines of water and the waves as they occurred in deep, wide water. The subject of the motion of a vessel through water, where the section was not limited, had been studied by many authorities. Professor Rankine had considered the motion of water about a vessel, and Mr. Froude had drawn the lines which the water would take, or rather the form which uniform straight bands of water would take, if the water were limited to motion in a horizontal plane, while a particular solid was moving through the mass, or might be considered as obstructing the flow. Of course that was an assumption to make the matter more simple. The real stream-line motion about a solid was so complicated that he could not define it exactly —only for some limited forms.

When a vessel floated in water with a free surface, waves formed at the bow and stern, which very much complicated the matter; but it might be taken that when a vessel passed through water, the water took part of the motion of the vessel at the bow and stern, and acquired a greater head or higher level, and increased the pressure on the bow and stern; this increase of head was transformed into velocity about the midships portion of the boat, where in deep water it had a distinct motion towards the stern of the vessel as described by the author. That motion in deep water was rapid somewhat near the vessel, gradually tapering off to no motion at a great distance. If a vessel surrounded with water having the motion described, were put into a shallow channel, it was evident that the motion underneath the vessel must be restricted, for part of the water which took motion from the vessel, would be replaced by the solid bottom of the canal, so that the vertical motion at the bow and stern must be obstructed and a greater motion take place on either side. Mr. Samuda had made a statement (from which he differed) to the effect that water must be compressed in order to pass under a vessel. If a vessel fitted the canal at its sides, only leaving a passage for water underneath, of course the vessel would go along the canal and the water would have to pass under. When a part of the hull of a vessel moved rapidly near the bottom of the canal, there was a rapid motion of water and scouring (perhaps where it was not injurious), and increased motion along the sides of the canal, when compared with the corresponding motion in deep water. In Mr. Samuda's description of a steamer passing over a bank down the river, he had spoken of the vessel being retarded and the paddles going more rapidly. That he took to be an indication that the stream-lines on the

sides of the vessel were more rapid, as previously described, in shallow water than in deep water, and instead of the vessel going faster it was moving more slowly by the land, although the relative motion of the vessel and the water alongside was greater. Mr. Scott Russell had made some valuable experiments on the passage of waves along canals, but he thought the author was right in saying that the waves of translation moved very little water; most of the water had to pass the vessel, and therefore his formula for the motion of the water past the vessel perhaps approximately represented the facts. With regard to the second form of section, it had been argued that a wide section was necessary. He was not sure whether that was so or not, but it was certain that a deep section was necessary, and it seemed to him that if there was about an even depth of water in all directions from the surface of the hull, that was perhaps the most advantageous section, speed alone being considered in a limited section of water. He had observed this in working a steamer in the upper Thames, where there was a canal, a wide stream, and various forms of section. About the locks there was sometimes a deep, narrow section where the boat attained a moderate speed with apparent ease, and the waves ran smoothly along the bank without any breaking or any destructive effect. If the banks were smooth there would be little or no damage done, either to the banks, or the energy in the water which accompanied the vessel. In the Clyde, where rapid steamers were used of great power and the banks were paved, the effect of a vessel going at a high speed in a comparatively small channel might be seen. There was one effect which caused very much retardation where there was shallow water at the side. There were two principal kinds of waves which followed a steamer—those which diverged from the bow, and the transverse waves at the bow and stern, particularly at the stern. The transverse waves, in going along a narrow channel, with somewhat shallow sides, were bent into a curve and retarded, as could be seen on the sea-coast, the waves almost always coming in towards the shore, never making an angle of anything like 90° with the shore. So, in a canal with shallow sides, the waves turned to the shore, ran aground and broke; that used up the force in the waves. The stem and stern waves were necessary; they were a part of the motion of the vessel; they had a *vis viva*, which belonged to the vessel as much as her own motion. Not so with the waves which ran off from the bow to the shore; they must go to the shore. But when the water was deep near the banks the waves would follow the vessel with more ease, and would, he thought, be less destructive. He did not know the object of the wide, shallow water at the sides of the Suez Canal; it would, no doubt, lessen the energy of the diverging waves from the bow, and might be very useful in protecting the soft banks; but it seemed to him that if a greater depth could be given it would be advantageous. With reference to wave motion he would refer to Mr. R. Edmund Froude's paper in 1881, before the Institution of Naval Architects, which gave a clear notion of the waves accompanying a boat. One thing in the author's paper which showed its inefficiency was that as the length of a vessel was varied the attendant waves fell on the vessel differently, and there was a great change in the resistance at any particular speed; now, when a vessel was moved along a canal, there would be a change in the form of the system of waves accompanying her, and the waves would only be repeated in a greater length for a particular speed, so their incidence on the vessel would be changed. This one cause of change alone would make the author's formula complicated, so as to be inapplicable if completely included. But perhaps with a number of experiments some useful rule might be advantageously established.

The use of an aluminium process for the decoration of iron and steel, as well as for their protection against rust, is spoken of in the German technical press. This process is intended to take the place of nickeling, tinning and coppering. The coating of aluminium is said to leave the sharpness of outline unimpaired, and to adhere very closely, being applicable to both cast and wrought work. Decoration with gold, silver, or vitrifiable pigments is said to be facilitated by this method. It is considered that the high price of aluminium—caused by the expensive processes by which it is made—will not seriously affect the success of this process.

HADFIELD'S PATENT MANGANESE STEEL.

By JOSEPH D. WEEKS, Pittsburgh, Pa.

Transactions of the American Institute of Mining Engineers.

MANGANESE has, until recently, been most highly esteemed as a good thing to keep out of steel. Its value in the process of manufacture has been fully recognized, but after it has played its part in the crucible or the converter, then the less of it the better. It is true that the mission of this metal and its influence upon the character of steel have been a source of much controversy. Our own Holley, in one of his special reports on ferro-manganese, states that "it has been suspected by some, and believed by a few, while it is still denied by many, that manganese as an ingredient in steel has not only a body-giving and toughening influence, but a positive neutralizing influence upon any excess of hardening or cold-shortening substances as phosphorus." In another paper on the same subject he states that "it should appear from such facts as we have that manganese toughens" the structural steels, "increases their soundness and prevents red-shortness." Notwithstanding these rather guarded assertions as to its value, the general belief, it will be found, is with Dr. Siemens, that manganese is "merely a cloak to hide impurities," and its presence in steel has been endured, not welcomed.

But whatever may have been the difference in opinion as to the effect upon steel of a small percentage of manganese, not to exceed, say, 1½ per cent., there has been a general agreement among metallurgists that any amount in excess of this would produce metal rotten and utterly worthless. In the Terre Noire experiments, referred to in Mr. Holley's report before quoted, 1 per cent. is the highest given as found in the steels reported upon. In a paper read by M. Gautier, of Terre Noire, before the British Iron and Steel Institute, on the "Uses of Ferro-manganese," this same percentage is given as the proper amount to be used in the manufacture of what this distinguished metallurgist terms "manganese steels," while in all three of the papers the analyses of the steel show the usual percentage of manganese to be much below this. Indeed, from 1 per cent. to 1½ per cent. has been regarded universally as "high manganese," and the published testimony is that more than this renders steel worthless.

In opposition to these views, Mr. Robert Hadfield, of the Hadfield Steel Foundry Company, Sheffield, England, has demonstrated that a steel containing from 7 to 30 per cent. of manganese is not only not a rotten and worthless product, but that in the ingot, as cast, it is harder, stronger, denser and tougher than most steel now manufactured, even when forged and rolled, and in addition it possesses curious and remarkable properties, which, it is believed, will make this steel exceedingly valuable for many purposes for which the ordinary steels are not now used.

In the samples of steel which, through the kindness of Mr. Hadfield, I am permitted to exhibit to the Institute, the manganese is from 9 per cent. in ingot No. 10 to 19 per cent. in the ax. No samples of the higher percentage have reached me. The bent flat piece contains 9½ per cent.; ingot 180 and the pit car-wheel which has been so badly hammered with so little effect, 11½ per cent.; the adze, 13¾ per cent.; and ingots Nos. 20 and 21, 14½ per cent. The ax and adze are castings just as they came from the sand, neither forged nor hardened, and have been ground since I received them. These are rough specimens, the Hadfield Foundry not being adapted to this class of work, but with proper care in moulding and manufacture, such articles can be made as smooth and clean as cast-iron. Indeed, some of the most valuable characteristics of this steel are shown in casting. It possesses great thinness and fluidity, casts without misrunning, does not settle as much as ordinary castings, and does not draw, particularly at the junction of the thick and thin parts. It is also free from honeycomb and other similar defects.

It is evident that a metal that casts in

this manner, and that needs no hardening nor tempering, must be especially adapted not only to the manufacture of most articles that are now cast, but for a wide range of articles that are now forged, rolled or hammered, such as the larger edged tools, hammers, picks, etc., guns, armor-plate, shell and other projectiles, car-wheels in place of chilled wheels, implements and parts of machinery, especially bearing parts, safes, steel tyres, plow-steel, etc. A razor has been cast from this steel and used without hardening. It was not equal to the best steel razor, but it was a fair implement.

But perhaps the most remarkable and valuable of the properties of Hadfield's steel is its great toughness, combined with its extreme hardness—two properties that are generally regarded as incompatible. The toughness will be evident upon an inspection of the fracture of the ingots. The little steel needles scattered all over the face of the fracture, forming an acute angle with the face, show the character of the rupture to be entirely different from that of ordinary steel. These needles are very tough, and, small as they are, do not break off when struck, but bend almost like native copper. It also requires a blow of considerable force to bend them. It was exceedingly difficult to break these ingots, a number of blows of a steam hammer being required, sledges having no effect. Ingot No. 10, with 9 per cent. of manganese, was broken from a piece 2 feet 6 inches long, supported at both ends. It bent 1½ inches before breaking, though it had not been forged. Hammered samples from this ingot gave 42 tons (94,080 pounds) tensile strength, and 20.85 per cent. elongation in 8 inches. The flat piece—No. 180 (9½ per cent.)—which has been hammered, was bent cold, and does not show the least crack. This piece has been drilled. The bulging of the steel under the drill-point is quite noticeable. This piece of wire was also bent cold after drawing. The small colliery-wheel (11½ per cent.) was struck 50 blows with a heavy sledge, and bent as will be seen.

Notwithstanding this toughness, the steel is extremely hard. The lower percentages—say 9 per cent. to 10 per cent.—which are the toughest, can be drilled and machined, but not as readily as the ordinary steels; those somewhat higher with difficulty, while it is practically impossible to drill, turn or otherwise machine the higher percentages. The colliery-wheel, which bent so under the sledge blows, shows on the head and hub the results of attempts made in this country, at my request, to drill and turn them. The edges were taken off the tools instantly, hardly scratching the wheel. The ax (19 per cent.) and the adze (13¾ per cent.), as has been already stated, were sent me rough as they came from the sand, and were ground by Messrs. Hubbard, Bakewell & Co., Pittsburgh. Regarding the steel, Mr. Charles W. Hubbard writes me:

"The steel ax and adze we ground for you were extremely hard. There seems to be a peculiar close, hard, greasy nature about the materials that resists the action of the grindstone and emery-wheel, as they have less effect on them than anything we have ever seen in the line of steel or iron. I would say the material has the very essence of anti-friction. A journal made of such material would run to an extreme number of revolution in a sand-box without friction or heat."

I have not tested this ax, but one made in a similar way cut through ⅜-inch iron. I have already intimated that this steel can be rolled and forged. The lower percentages are more easily worked, but steel with as much as 18 per cent. has been hammered. The higher percentages require great care, however. One of the most remarkable properties of this steel exhibits itself in connection with hammering or drawing it. When thus manipulated, it becomes exceedingly hard and loses some of its toughness. If now the steel is heated to a hot heat, yellow or nearly welding, and allowed to cool in the air, or is cooled in water or oil, it becomes exceedingly tough. The flat piece, No. 140, was so heated and cooled before being bent. The wire was similarly treated after drawing, which made it extremely hard. This is virtually annealing, but it will be noticed that it has an effect upon Hadfield's opposite to that upon carbon steel. It should be noted that this steel is non-magnetic in bulk and a poor conductor, though fine drillings and scrapings are attracted by the magnet.

The process of manufacturing this steel is exceedingly simple. Melted ferro-manganese high in manganese (Mr. Hadfield suggests 80 per cent.) and as low as possible in carbon, silicon and other foreign bodies, is added to iron that has been nearly or quite decarburized, or to molten steel. The manganese is thoroughly incorporated by stirring, and the steel poured into ingots or other suitable moulds. The percentage of ferro to be used, and consequently the amount of manganese in the steel, must be raised according to the use to which it is to be put. No absolutely exact proportions can be given. To produce a steel suitable for armor-plates, sufficient ferro to give, say, 10 per cent. manganese in the steel should be added; for car-wheels, axles or railway plant, say 11 per cent.; edge tools and steel tyres, 12 per cent.

THE PROPORTION BETWEEN STRESSES AND SECTIONS IN GIRDER WORK.

From "The Engineer."

Is the existing method of constructing girders by piling and riveting large and comparatively thick plates together followed because experience has proved it to be the best possible, or has mere habit and tradition an influence in its retention? From time to time the question has been discussed whether any better mode of securing ironwork other than by rivets is practicable, and up to the present the resulting verdict is in the negative. We do not purpose to criticize this opinion, but the fastening of the parts of a girder together is only one item in the designing and building of ironwork. The adjustment of good proportions between stresses and the sections resisting them is another and extremely important point, and the question we have put above relates to it. In some treatises on ironwork students are warned against entertaining the idea that it is possible to so design a girder or other similar structure as that every part will be alike in its strength and stress proportion. No doubt, in a certain sense, the warning is deserving of attention, but there is this fault about it that it tends to discourage original thought, and deters students and the younger members of the engineering profession from attempting a closer approximation between stresses and sections than obtains at present. No girder or truss is stronger than its weakest point, and the greater the number of weakest points in a girder the nearer, paradoxical as it may appear, is the approach to proper and economical proportion of material to load. A girder, every foot of which would be its weakest, or strongest, point, woud be in this respect perfect; and, reciprocally, any girder having any given point much in excess of all the rest in strength, is, by that excess, showing both a waste of material and an undue loading of the weakest point as well, which, as a consequence must be strong enough to sustain, not alone the legitimate stress due to correct design, but also to carry the weight of the excess material at the point unnecessarily strong, and which may be designated redundant iron. Thus it will be seen that bad proportioning of a girder causes greater waste of material than is superficially apparent; and were any one to set out even a moderately-sized girder, making some one point stronger than all the rest, and were then carefully to investigate the effects of that excess weight of material on other parts, necessitating increased section and consequent weight for them, and the effect of this again elsewhere, results might—nay, would—be found showing that it is very expedient to study proportion in girders with tolerable exactness.

An excellent method of ascertaining at a glance how near to exactness a girder has been proportioned to its total work, is to set out respectively the curves of stresses and the straight lines of resisting sections. We say straight lines of resistance advisedly, for such they are, and this fact seems more or less

ignored by many girder designers. The stresses of a girder, when graphically set out, are almost invariably curves of one or another type. Now, if even a simple girder, loaded uniformly all over, have respectively its stress lines and its resisting lines set out the one on the other, and to a true scale, that is, so that both vertical and horizontal scales are the same, an inspection of them will show that something remains for us to learn in the rolling of plates and angle-irons for such work, and their subsequent distribution in a girder. In a perfect girder to sustain a statical load, the graphic delineation of stresses will show a line more or less curved, the character of the curve depending upon the distribution of the load. The graphic setting out of the sections of resistance will show an exactly similar curve at such a distance from the stress line as would, on the scale adopted, represent the margin of safety adopted. Owing, however, to the present system of constructing girders by fastening a number of plates together, the section or resistance line can never in practice be a curve. It will be a series of straight lines, with abrupt terminations, and under the plate system of construction a designer must first set out his stress line curve, and then so set out his section of resistance lines as that its lowest point will not fall within the margin of safety line he will of course have set out beside the stress line. When this has been done, he will find a rather startling excess of strength in various parts of the lines, all dead or redundant material—"an old man of the sea"—lost money. Mathematicians delight to set out stress curves, and to give their readers and pupils the formulæ from which these have been arrived at. Such work is useful—very large iron structures, in fact, could scarcely be designed without their aid. Fortunately it is easier, so far as statical stress is concerned, to get tolerable uniformity between stress curves and resisting sections in large than can be attained in small work; this, of course, is due to the proportionally greater number of separate parts in the larger structure; or, to put the thing in another way, as each plate is represented by an ordinate from the horizontal datum, the greater the number of plates the greater also the number of ordinates, and one great change of section is subdivided into a greater or less number of smaller changes. It is well known that sudden changes of diameter are carefully to be avoided in shafting, and we now desire to direct the attention of those of our readers who may not thoroughly realize the fact, that just as it is certain that abrupt or even moderately abrupt change in the diameter of a shaft seriously weakens it, so also is it true that the greater are the abrupt changes of resisting section in girder or truss ironwork, so also is the waste of material. The corollary or deduction from this being, that regarded from this point alone, the smaller the plates, and consequently the greater the number used in a girder, the less will be the idle and wasted material present in the work. Other reasons also suggest the advantage of using small and thin in preference to large and thick plates. The former, to begin with, can be had at a lower price; they are more easily made of uniform quality, require less labor to shift, less loss is incurred in a waster—test samples spoiling plates, it is better to spoil a small than a large one; better and sounder riveting can be attained with thin plates, and every plate is more likely to do its fair share of work. It perhaps will be objected that however sound our arguments may be for statical loads, the stresses due to dynamical work are so variable that such reasoning cannot apply. Such an objection has some force, but how much? Rolling, or other loads inducing vibration have their limits just as well defined as have statical stresses, and this being so, the value of the objections possible to raise against our reasoning is capable of determination, and can be so determined by any one expert in calculating dynamical strains. All that is necessary is to set out the dynamical stress curve for a rolling load, and then plotting the resistance section line. The only real force of any objection to the foregoing reasoning is that the calculation of dynamical stress needs a higher order of mathematical proficiency, and that a structure intended for dynamical work must of necessity have much more idle material, or rather much more material idle, when no train or other load is moving over it. In other respects, a neglect of the principles involved in the above reasoning is just

as bad in the one case as in the other. If some of our mathematical readers, or those who make the designing of girders a specialty, will set out the curves of stress and the lines of resistance for a girder, say, of 120 ft. span for such comparatively statical work as a road bridge, and also for a railway, we shall be happy to publish them and open our columns to a discussion on the merits of thick *versus* thin, and small *versus* large plates in girder work.

Another point deserving of attention at the hands of bridge and girder designers is the consideration of how far equality of resisting section to stress may be more nearly attained by using plates of different sizes and thicknesses. Conditions of working might, and no doubt do, arise in which variation of sizes and thicknesses would economize material if adopted. The distribution of iron to the greatest advantage, and how best to effect it, is a field of investigation in which both the theorist and the practical man can work together, and which indeed must be worked by both jointly if the best results are to be attained. It is a branch of study in which one cannot do without the other. A good deal may be, and doubtless is, already known on the subject, but it is certain something remains to be learned.

A NEW FORMULA FOR THE CALCULATION OF DRAW-BRIDGE STRAINS.

BY F. BERESFORD, C. E., Cincinnati, Ohio.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

IN 1857 Clapeyron published his celebrated "Theorem of Three Moments." Since then it has been greatly improved, but mostly by German and French workers in this department of Applied Mathematics.

In 1873 Weyrauch published a very complete work on the subject in which he considered the Moment of Inertia as variable instead of constant, as it had been taken previous to this time. Prof. Eddy has also made some additions to the formula, so that in its present form it is almost perfect in its applications to continuous girders. However, when it is used in the calculation of Draw-Bridge Reactions and Moments, it is found to be faulty for the following reasons:—

1st. The area of cross-section of the chords is assumed to be constant throughout the entire bridge; this in itself is, in our opinion, sufficient to condemn its use. Practically it is found that the area of cross-section of chord of the last panel of the first span (counting both panels and spans from left to right) may be at least ten times that of the first panel, that of the intermediate panels being between the first and last and increasing as we approach the center of the bridge.

2d. It considers the bridge to be of uniform height and makes no provision for a slanting chord member. As it is often found convenient, as a matter of economy, to have one chord slanting, the importance of this question is at once apparent.

3d. It neglects the deflections due to the web members, a matter of considerable importance, when the chords are not parallel.

4th. In the equation for the deflection $\frac{1}{r}=\frac{d^2y}{dx^2}=\frac{M}{EI}$, the one upon which the "Theorem of Three Moments" depends, the Moment of Inertia (I) appears either as a constant or variable factor. The Moment of Inertia of a beam or girder is, we think, well understood, but never, so far as we know, has any person defined the Moment of Inertia of a *truss*. (Area of cross-section of chord) $\times\frac{(\text{height})^2}{2}$ is generally given as the value of the Moment of Inertia, but it has not been proved to be correct. Indeed, there is great doubt as to its correctness. Until we have a value which is known to be correct, it is not wise to use it.

It is the object of this paper to obtain a formula that will as far as possible, be free from these sources of error. We know that, at best, it will be but an approximation, but, nevertheless, we hope that it will give results nearer the truth than any heretofore obtained.

Mr. Chas. Bender, C. E., has, in his discussion of the "Properties of Continuous Bridges," incidentally derived the "Theorem of Three Moments" in such a manner as to be well suited for the present purpose, and we shall in general pursue the same method as that adopted by him.

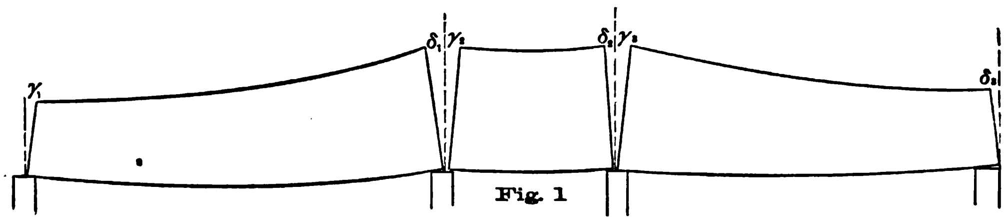


Fig. 1

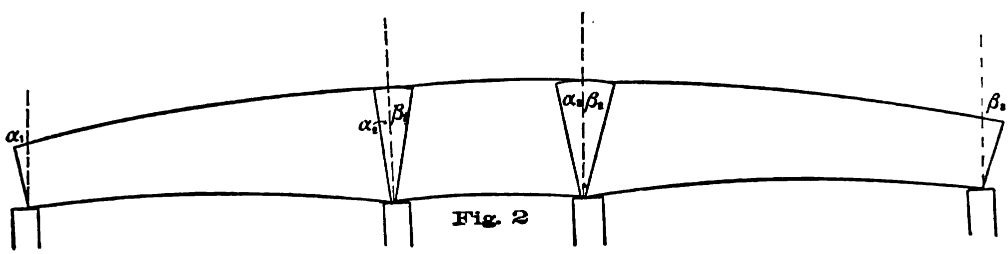


Fig. 2

Fig. 3

Suppose the bridge to rest upon four points of support, thus making it consist of three spans. Now conceive for an instant these spans to be simple trusses, not fastened together at the piers. If these trusses are not loaded, the posts over the piers will remain vertical; but by loading them in any possible manner, the posts will, by reason of deflection of the trusses, make certain angles with their originally vertical positions. The value of these angles depends on—the length of truss, the height at the different panel points, the character of the loading and the quality of the material.

Let these angles be represented by γ_1, γ_2, γ_3, δ_1, δ_2, δ_3, as in Fig. 1.

Now by exerting on the top of the end post at the pier B or C, A, B, C, and D, being the piers upon which the bridge is supported, a certain tensile strain and at the bottom of the same post a compressional strain equal in amount, we can cause the sum of the angles $(\delta_1+\gamma_2)$ or $(\delta_2+\gamma_3)$ to become zero. This is in reality what occurs when a bridge is made continuous. Then we have the ends so fastened together that there can be no angle between the end posts; this causes, when the bridge is loaded, certain unknown moments at the piers depending upon the forces above mentioned.

These forces would, were the dead and live loads removed, cause certain upward flexures due to the unknown moments, and therefore, cause the end to make angles α_1, α_2, α_3, and β_1, β_2, β_3, of Fig. 2 with their vertical positions.

Over one pier we have: The sum of the angles due to flexure is equal to that due to deflection, that is

$$\alpha_2+\beta_1=\delta_1+\gamma_2 \text{ and } \alpha_3+\beta_2=\delta_2+\gamma_3. \quad (1)$$

By means of these fundamental relations we propose to obtain our formula. The second members are in terms of known loads at known distances from the piers, and the first are in terms of the unknown moments, the values of which give a solution to the problem, since the

reactions at the piers can at once be found from them.

Suppose h_b to be the height of the bridge at pier B and f the force referred to above, which acts along the horizontal chord, then,

$f\,h_b = M_b$, M_b representing the moment producing upward flexure and acting at the pier B. By the principles of Statics, we have

$M_b = p_1\,c_1\,n_1$, p_1, c_1, n_1, representing, a force acting vertically at the pier A, the panel length, and the number of panels of the first truss, respectively.

In the continuous truss, Fig. 3, we must have at the pier B, in order to preserve equilibrium, such a moment, that being added to M_b will give zero, and since $c_1\,n_1$ is the same in both cases, the force must be $-p$, that is, it acts in a direction opposite to the one at A. By similar reasoning it can be shown that $-p_2, +p_2, -p_2, +p_2$, act at B, C, and D, as shown in the figure.

We have the equations

$$\left.\begin{aligned} M_b &= p_1\,c_1\,n_1 \\ M &= M_b - p_2\,c_2\,n_2 \end{aligned}\right\} \quad . \quad . \quad . \quad (2)$$

M_c being the moment at the pier C and subscript $_2$ being used for middle span.

Since at the end piers the arm of the force is zero, the moments at these points are each zero and no bending moment occurs there.

Before obtaining the values of α, β, γ, δ, it will be necessary to make several auxiliary proofs, with which we will now proceed.

1st. Let A B C be any triangle, a, b, c, being the sides as shown in Fig. 4, then,

$$a^2 = b^2 + c^2 - 2\,bc \cos. A.$$

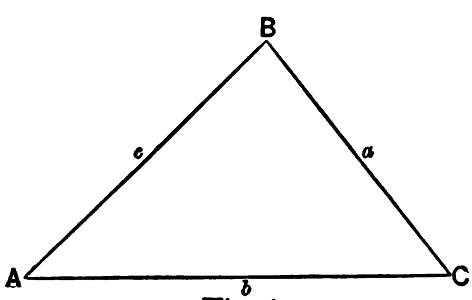


Fig. 4

If under the action of a certain force the length of the side a is changed by Δa, that of b by Δb and that of c by Δc making the total lengths $a+\Delta a$, $b+\Delta b$, $c+\Delta c$, then

$$(a+\Delta a)^2 = (b+\Delta b) + (c+\Delta c)^2 - 2(b+\Delta b)(c+\Delta c) \cos. (A+\Delta A)$$

A being at the same time changed by ΔA

$$a^2 + 2a\,\Delta a + (\Delta a)^2 = b^2 + 2b\,\Delta b + (\Delta b)^2 + c^2 + 2c\Delta c + (\Delta c)^2 - 2(bc + b\Delta c + c\Delta b + \Delta b\Delta c) \cos. (A+\Delta A)$$

Since Δa, Δb, Δc, are, at most, exceedingly small quantities, the squares and products of them can, by the principles of Differential Calculus, be neglected.

As ΔA is also very small $\cos. \Delta A = 1$ and $\sin. \Delta A = \Delta A$.

$$\therefore\ a\Delta a = b\Delta b + c\Delta c - (b\Delta c + c\Delta b) \cos. (A+\Delta A),\ bc \sin. A,$$

or $\Delta A =$

$$\frac{a\Delta a - b\Delta b - c\Delta c + (b\Delta c + c\Delta b)\cos. A}{bc \sin. A.}$$

If $A = 90^c$ then

$$\angle A = \frac{a\,\Delta a}{bc} - \frac{\Delta b}{c} - \frac{\Delta c}{b}$$

$$\Delta B = \frac{\Delta b}{c} - \frac{a\,\Delta a}{bc} + \frac{c\,\Delta a}{ab} = \frac{\Delta b}{c} - \frac{(a^2-c^2)}{abc}\Delta a$$

$$= \frac{\Delta b}{c} - \frac{b\,\Delta a}{ac} \text{ since } a^2 - c^2 = b^2$$

$$\Delta C = \frac{\Delta c}{b} - \frac{c\,\Delta a}{ab}$$

2d. Let A, B, C, be the three angles about the x^{th} panel point of an unloaded truss, and θ_x the angle which the upper chord at this point makes with the lower.

As the bridge is unloaded the lower chord members lie in a straight line, or

$$A + B + C = 180^\circ \text{ and } A = 90^\circ.$$

but when a load is applied each angle is changed by a very small amount, so that,

$$A + \Delta\,A + B + \Delta\,B + C + \Delta\,C = 180^\circ$$
$$\text{Let } \Delta\,A + \Delta\,B + \Delta\,C = \delta.$$

From Fig. 5 in which d_x, d_{x+1}, are the diagonals, h_x, h_{x+1}, the posts, and c_x, c_{x+1} the lower chords, we see from the previous problem that

$$\Delta A = -\frac{\Delta\,h_x}{c} - \frac{\Delta\,c_x}{h_x} + \frac{d_x\,\Delta\,d_x}{h_x\,c_x}$$

$$\Delta B = \frac{\varepsilon_x\,\Delta\,\varepsilon_x - d_{x+1}\Delta\,d_{x+1} - h_x\,\Delta\,h_x}{c\,h_x} + \frac{(d_{x+1}\,\Delta\,h_x + h_x\,\Delta\,d_{x+1})\,h_{x+1}}{c\,h_x\,d_{x+1}}$$

$$\Delta C = \frac{\Delta h_{x+1}}{c} \quad \frac{-\Delta d_{x+1} h_{x+1}}{c\, d_{x+1}}, \text{ so that}$$

$$\delta_x = \frac{\text{Sec. } \theta_x \ \Delta \varepsilon_x - \Delta c_x}{h_x}$$

$$= \frac{d_x \Delta d_x - d_{x+1} \Delta d_{x+1} + h_x \ \Delta h_{x+1} + h_{x+1} \Delta h_x - 2\, h_x \ \Delta \ h_x}{c\, h_x}$$

In the above value of δ_x the first term of the second member is the amount due to the chord members. This while seemingly a difference is really a sum, for, the first term of the numerator of this fraction expresses the change of length in the upper chord, due to compression.

The second term of the second member of this equation, the part due to the web strains, has the + sign when the diagonals are tension members and the posts compression members, and the − sign when the reverse is true.

Now $h_{x+1} - h_x = c \tan. \theta_x$ and

$h_{x+1} + h_x (1 + 2\lambda) = 2 (1 + \lambda) h_x$ nearly, hence this term becomes

$$\pm \frac{R'}{E} . 2 (1 + \lambda) \tan. \theta_x \text{ nearly, and}$$

$$\mu_x = \frac{R'}{E}\left(c \frac{(\lambda \sec.^2 \theta_x + 1)}{h_x} \pm 2(\lambda + 1) \tan. \theta_x\right) \text{ nearly} \quad . \ . \ . \quad (3)$$

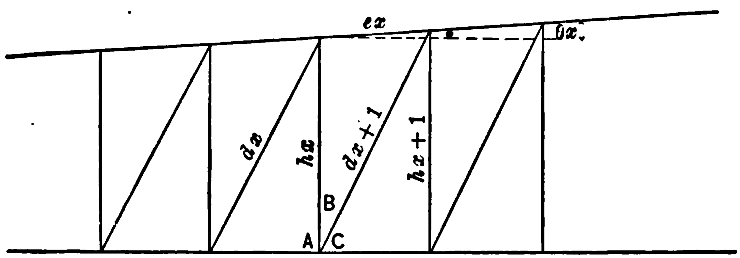


Fig. 5

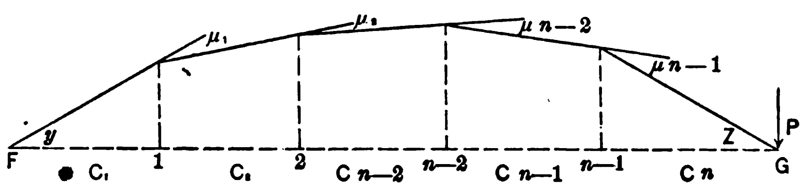


Fig. 6

The second term expresses that in the lower chord due to tension. If we consider compression as a positive strain and tension as a negative, the first term becomes the sum of two quantities.

In any piece of material the change of length due to a compressional strain is equal to $\frac{l \times \text{Stress}}{E \times \text{Area}} = \frac{R\, l}{E}$, that due to a tensile strain is equal to $\frac{R' l}{E}$, in which R is the proof strength for compression, of the material, R′ the same for tension, l the length of the piece, and E its Modulus of Elasticity. Let $\frac{R}{R'} = \lambda$ then

$$\mu_x = \frac{c\, R'}{E\, h_x} (\lambda \sec.^2 \theta_x + 1) \pm R' (h_{x+} \quad {}_x) \quad [h_{x+1} + (1 + 2\lambda) h_x].$$

3d. If $\mu_1, \mu_2, \mu_3, ---- \mu_{n-1}$ represent the angles which the lower chord members make with each other after the truss has been bent, as in Fig. 6, and y and z the angles which the end members make with the straight lines joining the two end panel points, then

$$y + z = \mu_1 + \mu_2 + \mu_3 + + + ----- \mu_{n-1},$$

and

$$y + (y - \mu_1) + (y - \mu_1 - \mu_2) + + ---- (y - \mu_1 - \mu_2 - \mu_3 --- {}^{n-1}) = 0$$

For the lower chord members

$$c_1, c_2, c_3, --- c^{n-1}$$

are of the same length, thus making the sum of the sines of all these angles equal to zero, and as the angles are very small, we may use them instead of their sines.

$\therefore ny=(n-1)\mu_1+(n-2)\mu_2+(n-3)$
$\mu_3+----2\mu_{n-2}+\mu_{n-1}$
and $nz=\mu_1+2\mu_2+3\mu_3+---$
$+(n-1)\mu_{n-1}$

$$y=\frac{1}{n}\sum_1^{n-1}(n-x)\mu_x \text{ and } z=\frac{1}{n}\sum_1^{n-1}x\mu_x. \quad (4)$$

If at the end of the n^{th} panel, or at the point G, a force P acts as in Fig. 6, then the stress on any horizontal member due to this force is P $\times$ distance from point of application of P to member in question $\times\frac{1}{\text{height at this pt.}}$, or, if the panel pts. be numbered 1, 2, 3, 4, etc., from left to right, as in the figure, then stress on the x member $=\frac{P\,(n-x)c}{h_x}$. Substituting the value of R′ in equation (3) and value of μ_x in equation (4) we have

$$y=\frac{-Pc}{En}\sum_1^{n-1}\frac{(n-x)^2}{h_x^2\,A_x}$$
$$[c(\lambda\sec.^2\theta_x+1)\mp2(\lambda+1)h_x\tan.\theta_x]$$
$$z=\frac{-Pc}{En}\sum_1^{n-1}\frac{x(n-x)}{h_x^2\,A_x}$$
$$[c(\lambda\sec.^2\theta_x+1)\mp2(\lambda+1)h_x\tan.\theta_x]$$

A_x being the area of the x^{th} section of chord. Since the moment at F is Pcn we have

$$y=\frac{-M}{En^2}\sum_1^{n-1}\frac{(n-x)^2}{h_x^2\,A_x}$$
$$[c(\lambda\sec.^2\theta_x+1)\mp2(1+\lambda)h_x\tan.\theta_x]. \quad (5)$$
$$\text{and } z=\frac{-M}{En^2}\sum_1^{n-1}\frac{x(n-x)}{h_x^2\,A_x}$$
$$[c(\lambda\sec.^2\theta_x+1)\mp2(1+\lambda)h_x\tan.\theta_x]. \quad (6)$$

We can now find the values of α, β, γ, δ.

γ and δ, it will be remembered, are angles caused by the deflection of the truss. To find their values: Conceive the truss to be loaded in any possible manner. First, consider the effect due to a load W concentrated at one of the panel points, and from this find the effect of the loads concentrated at the other panel points, and thus of the whole load causing the deflection.

Suppose the curved line FG, in Fig. 7, to represent a curve passing through the lower panel points after the load W has been applied, and H.I. the tangent to the curve at the point of application of the load, distant mc from the pier G.

The problem really becomes the following:

1st. To find the angles y and z of the two segments of the truss, mc, and $(n-m)c$.

2d. To find the values of the angles Ψ and φ, as shown in Fig. 7, in terms of y and z. Ψ and φ, being the angles which lines drawn to join the two end panel points, and the panel point at H

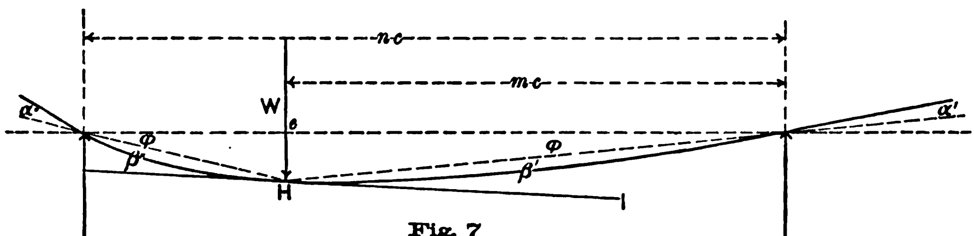


Fig. 7

make with the line joining the two end panel points.

In the above figure *β',β'' are y angles while α',α'' are z angles.

$\therefore$ From equation (5)

$$\beta'=\frac{M}{Em^2}\sum_1^{m-1}\frac{(m-x)^2}{h_x^2\,A_x}[c(\lambda\sec.^2\theta_x+1)$$
$$+2(\lambda+1)h_x\tan.\theta_x]$$
$$\beta''=\frac{M}{E(n-m)^2}\dagger\sum_1^{n-m-1}\frac{x(x+1)}{h_x^2\,A_{x+1}}$$
$$[c(\lambda\sec.^2\theta_x+1)-2(\lambda+1)h_x\tan.\theta_x] \text{ and}$$
$$\alpha'=\frac{M}{Em^2}\sum_1^{m-1}x(m-x)$$
$$[c(\lambda\sec.^2\theta_x+1)-2(\lambda+1)h_x\tan.\theta_x]$$
$$\alpha''=\frac{M}{E(n-m)^2}\sum_1^{n-m-1}(1+x)\,(n-m-x)$$
$$[c(\lambda\sec.^2\theta_x+1)+2(\lambda+1)h_x\tan.\theta_x]$$

* The angles are called β', β'', α', α'' merely for convenience, and should not be confused with α and β mentioned above.

† x is hereby replaced by $x+1$ because the arm of the force is $c(x+1)$ instead of cx.

In obtaining α' and β' the first panel point to the right of the point of application of W is marked 1 and so on.

But $M=\dfrac{Wm(n-m)c}{n}$

$$\beta'=\frac{W(n-m)c}{Emn}\sum_{1}^{m-1}\frac{(m-x)^2}{h_x^2\,A_x}$$
$$[c(\lambda\sec.^2\theta_x+1)+2(\lambda+1)h_x\tan.\theta_x]$$

$$\beta''=\frac{Wmc}{E\,(n-m)\,n}\sum_{1}^{n-m-1}\frac{x\,(x+1)}{h_x^2\,A_{x+1}}$$
$$[c(\lambda\sec.^2\theta_x+1)-2(\lambda+1)h_x\tan.\theta_x]$$

$$\alpha'=\frac{W\,(n-m)\,c}{Emn}\sum_{1}^{m-1}\frac{x\,(m-x)}{h_x^2\,A_x}$$
$$[c(\lambda\sec.^2\theta_x+1)+2(\lambda+1)h_x\tan.\theta_x]$$

$$\alpha''=\frac{Wmc}{E\,(n-m)\,n}\sum_{1}^{n-m-1}(1+x)\,(n-m-x)$$
$$[c(\lambda\sec.^2\theta_x+1)+2(\lambda+1)h_x\tan.\theta_x]$$

To find Ψ and Φ, let w be the angle which the two lines, drawn from the end panel points to the point of application of the load, make with each other, and as β' and β'' are angles which they make with the tangent HI, therefore,

$$w+\Phi+\Psi=180^\circ, \qquad \text{and}$$
$$w+\beta'+\beta''=180^\circ.$$
$$\therefore\ \beta'+\beta''=\Phi+\Psi$$

Since Φ and Ψ are very small angles they can be taken as proportional to their tangents without appreciable error

$$\therefore\quad \Psi:\Phi::\frac{\varepsilon H}{mc}:\frac{\varepsilon H}{(n-m)c}\ \text{or}$$

$$(n-m)\,\Phi=m\Psi\quad \Psi=\frac{n-m}{m}\Phi$$

$$\therefore\ \Phi=\frac{m}{n}\Big(\beta'+\beta''\Big)\text{and }\Psi=\frac{n-m}{n}\Big(\beta'+\beta''\Big)$$

so that

$$\Phi=\frac{W(n-m)c}{En^2}\sum_{1}^{m-1}\frac{(m-x)^2}{h_x^2\,A_x}$$
$$[c(\lambda\sec.^2\theta_x+1)+2(\lambda+1)\,h_x\tan.\theta_x]$$
$$+\frac{Wm^2c}{E\,(n-m)n^2}\sum_{1}^{n-m-1}\frac{x(x+1)}{h_x^2\,A^{x+1}}$$
$$[c(\lambda\sec.^2\theta_x+1)-2(\lambda+1)h_x\tan.\theta_x]$$

$$\Psi=\frac{W(n-m)^2c}{Emn^2}\sum_{1}^{m-1}\frac{(m-x)^2}{h_x^2\,A_x}$$
$$[c(\lambda\sec.^2\theta_x+1)+2(\lambda+1)h_x\tan.\theta_x]$$
$$+\frac{Wmc}{En^2}\sum_{1}^{n-m-1}\frac{x(x+1)}{h_x^2\,A^{x+1}}$$
$$[c(\lambda\sec.^2\theta_x+1)-2(\lambda+1)h_x\tan.\theta_x]$$

If the posts remained perpendicular to the chord during deflection or upward flexure then δ would be equal to $(\Psi+\alpha')$, and γ would be equal to $(\Phi+\alpha'')$, for $(\Psi+\alpha')$ and $(\Phi+\alpha'')$ give the proper values of the deflection from the horizontal line of the two end chord members, and if the angle between the end posts and chord members remained constant throughout, it would also express the deflection of the end posts from their originally vertical positions. We have seen, however, that in any right triangle of which the angle $A=90^\circ$ that

$$\Delta A=\frac{-\Delta h_x}{c_x}-\frac{\angle c_x}{h_x}+\frac{d_x\angle d_x}{c_x\,h_x}$$

(see Fig. 5).

$$\Delta A=-\frac{h_x^2 R+c_x^2 R'-d_x^2 R'}{Ec_x\,h_x},$$ if c_x is a tension member, which is the case when the truss is deflected by a load,

$$\Delta A=-\frac{h_x}{c_x}R'(\lambda+1)$$

When the truss has an upward flexure, the member c_x is a compression piece, and we have

$$\Delta A=\frac{-h_x^2 R-c_x^2 R-d_x^2 R'}{Ec_x\,h_x}=-\frac{(h_x^2+c_x^2)}{c_x\,h_x}$$
$$R'(\lambda+1)=-\left(\frac{h_x}{c_x}+\frac{c_x}{h_x}\right)R'(\lambda+1)$$

From this it is seen that this change of angle tends to increase the value of δ and γ while it increases that of α and β also.

Applying the above principles to finding the values of δ_1 and γ_2 of Fig. 1 due to a load W_1 on the first truss distant m_1c_1 from the pier B, and to a load W_2 on the second truss distant m_2c_2 from the pier c.

$$\delta_1=\frac{W_1}{E}\Bigg\{\frac{m(\lambda+1)}{An}+\frac{mc}{n_1^2}\sum_{1}^{n-m-1}\frac{x'(x+1)}{h_x^2\,A_{x+1}}$$
$$[c(\lambda\sec.^2\theta_x+1)-2(\lambda+1)h_x\tan.\theta_x]$$
$$+\frac{n_1-m}{n_1^2}c\sum^{m-1}\frac{(m-x)\ (n-m+x)}{h_x^2\,A_x}$$
$$[c(\lambda\sec.^2\theta_x+1)+2(\lambda+1)h_x\tan.\theta_x]\Bigg\}$$

In the value of δ_1 of the letters n_1m, etc., refer to the first truss, while in the value of γ_2 they refer to the second.

Now, due to the whole loads ΣW_1 and ΣW_2, we will have

$$\delta_1 = \Sigma \frac{W_1}{E}\Big\{\frac{m(\lambda+1)}{An} + \frac{mc}{n_1^2} \sum_1^{n_1-m-1} \frac{x(x+1)}{h_x A_{x+1}}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]$$
$$+ \frac{(n_1-m)}{n_1^2} c \sum_1^{m-1} \frac{(m-x)\ (n-m+x)}{h_x^2 A_x}$$
$$[c(\lambda \sec.^2\theta_x + 1) + 2(\lambda+1)h_x \tan.\theta_x]\Big\}$$

$$\text{and } \gamma_2 = \Sigma \frac{W_2}{E}\Big\{\frac{(n_2-m)\ (\lambda+1)}{A_1} +$$
$$+ \frac{(n_2-m)}{n_2^2} c \sum_1^{m-1} \frac{(m-x)^2}{h_x^2 A_x}$$
$$[c(\lambda \sec.^2\theta_x + 1) + 2(\lambda+1)h_x \tan.\theta_x]$$
$$+ \frac{mc}{n_2^2} \sum_1^{n-m-1} \frac{(x+1)\ (n_2-x)}{h_x^2 A_{x+1}}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]\Big\}$$

We have now to find the values of β_1 and α_2 of Fig. 2. β_1 is caused by the unknown moment M_b acting at the pier β while α_2 is caused by M_b acting at the same pier together with M_c acting at the pier C.

$$\beta_1 = -\frac{M_b}{E}\Big\{\frac{(h_n^2+c^2)(\lambda+1}{c_1 h_{n_1}^2 A_{n1}} +$$
$$+ \frac{1}{n_1^2} \sum_1^{n_1-1} \frac{x(x+1)}{h_x^2 A_{x+1}}$$
$$[c(\lambda \sec.^2\theta_x - 1) - 2(\lambda+1)h_x \tan.\theta_x]$$

$$\alpha_2 = -\frac{M_b}{E}\Big(\frac{(h_0^2+c^2)(\lambda+1)}{c h_0^2 A_1}$$
$$+ \frac{1}{n_2^2} \sum_1^{n_2-1} \frac{(n_2-x)^2}{h_x^2 A_x}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]\Big)$$
$$-\frac{M_c}{E}\Big(\frac{(h_n^2+c)(\lambda+1)}{c h_{n_2}^2 A_{n2}} + \frac{1}{n^2} \sum_1^{n_2-1} \frac{x(n_2-x)}{h_x^2 A_x}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]\Big)$$

In the value of β_1 the letters all refer to the first truss, while in the value of α_2 they refer to the second. Substituting these values of α_2 β_1, γ_2 and δ_1 in eq. (1), we get by assuming the area of cross-section of chord (A) to be constant throughout the lower chord of the middle truss and equal to the area cross-section of the last chord of the first truss while the areas of the others decrease in regular order as we approach the pier A, each one being $\frac{2}{3}$ of the preceding:

$$M_b \Big\{\frac{(h_x^2+c^2)(\lambda+1)}{c h_n^2} + \frac{1}{n_1^2} \sum_1^{n_1-1} \frac{x(x+1)}{h_x^2 (\frac{2}{3})^{n_1-x+1}}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]\Big\}$$
$$+ M_b \Big\{\frac{(h_0^2+c^2)(\lambda+1)}{c h_0^2} + \frac{1}{n_2} \sum_1^{n_2-1} \frac{(n_2-x)^2}{h_x^2}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]\Big\}$$
$$+ M_c \Big\{\frac{(h_n^2+c^2)(\lambda+1)}{c h_n^2} + \frac{1}{n_2} \sum_1^{n_2-1} \frac{x(n_2-x)}{h_x^2}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]\Big\}$$
$$+ \Sigma W_1 \Big\{m(\lambda+1) +$$
$$\frac{mc}{n_1^2} \sum_1^{n_1-m-1} \frac{x(x+1)}{h_x^2 (\frac{2}{3})^{(n-m-x+1}}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]$$
$$+ \frac{(n_1-m)}{n_1^2} \sum_1^{m-1} \frac{(m-x)(n-m+x)}{h_x^2 (\frac{2}{3})^{m-x}}$$
$$[c(\lambda \sec.^2\theta_x + 1) + 2(\lambda+1)\ h_x \tan.\theta_x]\Big\}$$
$$+ \Sigma W_2 \Big\{(n_2-m)\ (\lambda+1) +$$
$$\frac{(n_2-m)}{n_2} c \sum_1^{m-1} \frac{(m-x)^2}{h_x^2}$$
$$[c(\lambda \sec.^2\theta_x + 1) + 2(\lambda+1)h_x \tan.\theta_x]$$
$$+ \frac{mc}{n_2^2} \sum_1^{n_2-m-1} \frac{(x+1)\ (n_2-x)}{h_x^2}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda-1)h_x \tan.\theta_x]\Big\} = 0.$$

It is often the case that the second span consists of but one panel; then the loading rests directly on the piers and $\Sigma W_2 = 0$. The y and z angles of this span also become zero.

$$M_b \Big\{\frac{(h_n^2+c_1^2)\ (\lambda+1)}{c h_{n_1}^2} +$$
$$\frac{1}{n_1^2} \sum_1^{n_1-1} \frac{x(x+1)}{h_x^2 (\frac{2}{3})^{n_1-x+1}}$$
$$[c\ (\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]\Big\}$$
$$+ M_b^* \frac{(h_0^2+c^2)\ (\lambda+1)}{c h_0^2} + M_c^* \frac{(h_1^2+c^2)\ (\lambda+1)}{c h_1^2}$$
$$+ \Sigma W_1 \Big\{M(\lambda+1) + \frac{mc}{n_1^2} \sum_1^{n_1-m-1} \frac{x(x+1)}{h_x^2 (\frac{2}{3})^{n-x-1}}$$
$$[c(\lambda \sec.^2\theta_x + 1) - 2(\lambda+1)h_x \tan.\theta_x]$$

* h and c of these two terms belong to the second span.

$$+\frac{(n_1-m)}{n_1^2}c\sum_1^{m-1}\frac{(m-x)(n-m+x)}{h_x^2\left(\frac{2}{3}\right)^{m-x-1}}$$

$$\left.[c(\lambda \sec.^2\theta_x+1)+2(\lambda+1)h_x \tan.\theta_x]\right\}=0$$

In practice it is usual to make the third span of the same length as the first, therefore, if we now begin at the pier furthermost to the right and work towards the left, we shall get an equation like the above, except M_c will exchange places with M_b. We now have two equations from which can be obtained by elimination the value of either moment. In case of a pivot bridge, which consists of but two equal spans, the moment at $C=o$ if A, B, C are the points of support, and supposing the areas of the cross-sections of the chords to decrease to the right in the second span as they did in the first span of the three-span bridge, we have:

$$2M_b\left\{\frac{(h_n^2+c^2)(\lambda+1)}{ch_n^2}+\right.$$

$$\frac{1}{n_1^2}\sum_1^{n_1-1}\frac{x(x+1)}{h_x^2\left(\frac{2}{3}\right)^{n-x-1}}$$

$$\left.[c(\lambda \sec.^2\theta_x+1)-2(\lambda+1)h_x \tan.\theta_x]\right\}$$

$$+\Sigma(W_1+W_2)\left\{m(\lambda+1)+\right.$$

$$+\frac{mc}{n_1^2}\sum_1^{n-m-1}\frac{x(x+1)}{h_x^2\left(\frac{2}{3}\right)^{n-x-1}}$$

$$[c(\lambda \sec.^2\theta_x+1)-2(\lambda+1)h_x \tan.\theta_x]$$

$$+\frac{(n-m)}{n_1^2}c\sum_1^{m-1}\frac{(m-x)(n-m+x)}{h_x^2\left(\frac{2}{3}\right)^{m-x}}$$

$$\left.[c(\lambda \sec.^2\theta_x+1)+2(\lambda+1)h_x \tan.\theta_x]\right\}=0$$

In the above investigation it has been supposed that the tops of the piers were at the same height above any horizontal datum plane.

If this is not the case we shall have some additional moments at the piers, for the end posts would no longer be parallel to each other, were the trusses simple and unloaded, but certain angles $(\delta_{a_1}+\gamma_{a_2})$ and $(\delta_{a_2}+\gamma_{a_1})$ would exist which must be made zero, in order to make the bridge continuous. To do this we must add some additional moments M_{aB} and M_{ac} at the piers B and C. We have in this case δ and γ for any one span equal, that is $\delta_{1a}=\gamma_{1a}$, &c.

If we suppose the datum plane to beat the top of the pier A or first pier, it either cuts or passes over the other piers according as they are higher or lower than the first. If k_x represents of x^{th} pier above the datum plane, then,

$$\gamma_{ax}=\delta_{ax}=\pm\frac{(k_x-k_{x-1})}{c_{x-1}-n_{x-1}}$$

Since the angles are very small and we make no appreciable error in taking the tangent of an angle instead of the angle. The + sign being used when the pier in question is higher than the datum plane and the − sign when it is lower.

Substituting in eq. (1) these values of δ and γ, and also of a and β in terms of the unknown moments we are enabled to obtain two equations in terms of the additional unknown moments, from which we can find their values.

As a practical example let us take the bridge computed by Burr* and find the values of the moments.

The bridge consists of three spans. The first and third, each consisting of 6 panels of 24 ft. each, the second, of 1 panel of 8 feet. In the first span $h_1=21$ ft., $h_2=23$ ft., $h_3=25$ ft., $h_4=27$ ft., $h_5=29$ ft., while the height of the second span is constant 30 ft. The load is 18 tons per panel pt. throughout the entire bridge. In this case we have $M_b=M_c$. Substituting in the formula

we have M_b (.200+.428+.143)=

$$-18[\tfrac{8}{6}\dagger(5+4+3+2+1)+$$
$$\tfrac{24}{36}(10\times.143+6\times1.121+3\times1.237+$$
$$1.119+2.128+3\times1.369+$$
$$6\times.704+10\times.256)]$$

$\therefore$ $M_b=-965.20$ foot tons. The value given by Burr is −1668.73 foot tons.

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* Stresses in Bridge and Roof Trusses, pages 118, 119, and 120.

† In this problem the proof strength for compression is taken as 6000 per sq. in., that for tension 10000, thus making $\lambda=\frac{3}{5}$.

At a recent meeting of the Paris Academy of Sciences, an account was read of a deposit of saltpetre in the neighborhood of Cochabamba, Bolivia, by M. Sacc. An analysis of this vast deposit, which is large enough to supply the whole of the world with nitrate of potash, yields the following results:—Nitrate of potash, 60.70; borax, with traces of salt and water, 30.70; organic substances, 8.60; total, 100.00. The author concludes that the saltpetre is the result of the decomposition of an enormous deposit of fossil animal remains.

THE BASIC OPEN-HEARTH STEEL PROCESS.

By THOMAS GILLOTT, M. I. C. E.

From the Selected Papers of the Proceedings of the Institution of Civil Engineers.

In the following account of the application of the basic, or Thomas-Gilchrist, dephosphorization process to the open-hearth furnace, the author has confined himself to his own personal experience, dating from May, 1882, at the works of the Farnley Iron Company, near Leeds. The basic Bessemer process is now well known and established, and will probably be the most extensively adopted for the production of steel in large quantities for general use; but, as in the contemporary acid processes the Siemens furnace retains advantages for the production of exceptional qualities, and has the further capability of dealing with raw materials which are altogether unsuited for the Bessemer (basic) process. Pig-iron most suitable for the basic Bessemer process should approximately contain, according to Messrs. Thomas and Gilchrist, silicon, 0.5 to 1.8; phosphorus, 0.8 to 3; sulphur, under 0.3; manganese, not over 2.5 per cent.; and, in order to secure the necessary heat, the carbon would probably have to be 3.5 per cent.; but in the basic open-hearth process the proportion of heat-producing elements need not be high, and large quantities of wrought iron scraps, which often contain 0.2 per cent. of phosphorus, and are altogether unsuited for the acid process, may be converted into steel of high quality and great purity. After unsuccessful attempts to produce a high-class steel in the acid process by the use of hematite pig for the bath, and wrought-iron scraps, or specially puddled iron made from pigs in which phosphorus was comparatively high, the author was instructed to apply to the open-hearth furnace the process which Messrs. Thomas and Gilchrist had at that time just succeeded with in the Bessemer converter. As no basic bricks excepting such as were too costly for practical use, were suitable for constructing the furnace throughout, the author consulted chemists as to materials which would be sufficiently refractory at a steel-melting heat, and at the same time securely isolate the dolomitic lime basin of the furnace from the silica walls above; so as to prevent that fluxing of lime and silica, and consequent destruction of the furnace, which would result from these materials being in immediate contact. He was advised that magnesia on the lime, alumina above the magnesia, and silica brickwork carried up on the alumina, would be secure; so, after some trials, bricks were made from magnesite, containing 98 per cent. of carbonate of magnesia, and others from bauxite of the approximate composition, silica, 15; alumina, 82; lime, 1; peroxide of iron, 2; and with these the furnace was built. This furnace had been previously used in the acid process for 50 to 55 cwt. charges, and was altered to suit the basic process. At the commencement the author recognized the necessity of providing for the removal of the slags in the early stages of the process, so that the banks of the furnace might be preserved; and any phosphorus transferred from the melted portion of the charge to the slags could be withdrawn, so as to reduce the amount of purification at the later stages, and allow the heat to be better employed by reducing the covering of slag on the metal; and this provision has proved very useful. The building of the furnace was conducted as follows: The iron bottom plates and the bridge plates are similar to those used for the acid furnace; brackets bolted to the side plates of the furnace support the brickwork by a plate at the level of the top of the lime basin, and prevent settlement of the upper brickwork in case of any shrinking of the lime basin or fluxing in work. The metal tapping hole and spout are at one level, and two slag tap holes at higher levels on the opposite or charging side of the furnace. In other respects the furnace does not differ from an ordinary Siemens furnace. Bricks made from magnesian lime, and of the following approximate composition—lime, 58; magnesia, 24; silica, 8; alumina and oxide of iron, 10 parts—are burnt at a full white heat, crushed, mixed with hot tar,

in a mortar mill, and are taken immediately to the furnace and rammed with red hot rammers over the furnace bottom plates, wooden plugs being inserted to form the tap holes, parts of the bottom being walled in the same bricks, and grouted with material similar to the ramming. Isolating courses of magnesia and bauxite bricks are laid on the top of the iron brackets on the sides, and across the ends of the basin over the bridge plates; and the shape of the hearth is completed by lime ramming. The use of hot rammers is necessary to coke the tar; otherwise the lime swells and falls to pieces in about twenty-four hours after mixing. The upper part of the furnace is then completed in silica brick in the usual manner, the gas and air passages, regenerators, reversing valves, &c., being similar to those used in making steel in the (acid) Siemens-Martin process.

The building of the furnace having been completed, and the furnace lighted, it will be sufficiently hot in about seventy-two hours for forming the bottom, and for this a full melting heat is necessary. Well burnt and freshly ground magnesian limestone is laid over the bottom and up the sides, with not more than 10 per cent. of ground firebrick added, so as to just flux the lining sufficient for binding the layer forming the furnace bottom. The wooden plugs for forming the tap-holes are burnt out, and the holes are stopped by dry lime only, or dry lime mixed with a small quantity of powdered coke. The gas and air ports should be so arranged as to throw the flame well down on the bottom, so that the heat may be sufficient to allow of the smallest possible admixture of silicious flux. The furnace is then ready, and the operation is thus conducted: Fresh and well-burnt lime (that used was from stone containing 98 per cent. of carbonate of lime), about one-twentieth to one-tenth of the weight of the pig-iron, is laid upon the bottom of the furnace; the pig is then charged, and the wrought-iron or steel scraps are put on the top of the pig, all being cold. The whole of the charge, if possible, is placed in the furnace at the commencement; feeding afterwards is always avoided when the state of the furnace will admit. In three or four hours, the pig being melted, the charge will have sunk so as to enable some slag to be tapped off. The first slag is generally very fluid, and more or less so as the silicon in the pig is higher or lower; consequently the sides of the hearth are far better preserved by getting rid of the more silicious slag at an early stage, and before the full heat is reached. In three and a-half to six hours, the time being dependent on the proportion of pig to scrap, the whole charge will be melted, more slag can be taken off if required, some lime added if the slag is very fluid, and ore with the lime if the proportion of pig to scrap is high. When boiling has ceased, and after the bath of fluid metal has been well stirred, the metal may be sampled. The samples are taken from the furnace in a ladle, cast into an ingot about 3 inches in diameter by 2 or 2½ inches thick, and flattened, when sufficiently cool, under a steam hammer into a plate ⅝ inch thick. This is quenched in cold water and then bent double, broken in two pieces, and the larger portion broken again. When the metal is sufficiently pure, these samples are so tough as to be scarcely, but nearly, broken through by being flattened close; the fracture is generally free from crystals, of good color, and uniform. Crystals in specks are sometimes found, but when regular as regards size, and equiangular to the naked eye as regards shape, the presence of crystals does not always indicate less purity than does their absence. Long streaks in the fracture, dark color, with little toughness, are more certain indications of the presence of phosphorus. The sample being satisfactory, the slag may be removed; but at this period it is generally too pasty to run, therefore it is withdrawn by raking through the center door, which is placed 4½ inches lower than the two end doors. About 5 per cent. of hematite pig is then charged through the end doors on the bridges, which pig when melted causes the metal in the bath to boil more or less violently for about fifteen minutes. After this, fifteen to thirty minutes are allowed to elapse, and any excess of slag beyond what is required to cover the metal in the casting ladle is removed, when the metal in the bath is ready for adding spiegel or ferro-manganese, and tapping. The addition of hematite pig causes a thorough agitation of the bath far beyond what can be done

by stirring with an iron bar; and every analysis the author has had made proves a further removal of phosphorus by the addition. There is a drawback, however, to this on account of the occasional failure of parts of the basin during the after-boil, which causes a further boiling out of the bottom or sides of the basin in the neighborhood of the failing place. Sometimes the tap-hole stopping commences to work out during the after-boil; and these accidents are generally due to some of the lime used for repairs, or stoppings, being imperfectly burnt; or it has been partially slaked during, or after grinding. The author has tried several times to secure the quality of steel aimed at without adding pig at the end of the purification, but the results have been uncertain and irregular. In only one instance has the author obtained evidence of re-phosphorization on adding the ferro-manganese after adding hematite pig. In this case the sample that had been flattened, quenched, and broken, tested 0.104 per cent., and the sample from the finished steel showed 0.122 per cent. of phosphorus. In other cases where the two respective samples were tested for phosphorus, the reduction per cent. averaged 0.016, and varied from 0.008 to 0.025 less phosphorus than the trial samples contained. When quite satisfactory the trial samples show 0.06 to 0.07 per cent. of phosphorus.

The author has always added the ferro-manganese in the furnace, as the reaction is somewhat too violent to permit of addition being made in the casting-ladle after the metal has been tapped; and a more uniform mixture is secured by adding in the furnace. A small quantity of slag is usually left on the metal so as to cover the steel in the ladle and avoid chilling while pouring the ingots. Some of this slag is nearly always left on the furnace bottom, and is raked out at the door, and some also remains in the tapping hole. If the slag be not removed immediately, the clearing of the tap hole is a troublesome operation, and for this purpose a door opposite to the tap hole is absolutely necessary. After casting the metal and raking out the slag, the bottom and sides of the furnace basin are repaired by magnesian lime, well burnt and finely ground, and any holes that may have been formed during working are carefully filled with the same kind of material. The gas is turned on to fix the lime by semi-fusion with the surface of the basin, the tap hole is stopped, and the furnace is ready for another charge in from one hour to three hours after the cast is over. The slag adhering to the bottom of the furnace requires occasional removal by melting off with fluxes, or breaking away; otherwise the capacity of the furnace is reduced and the level of the metal unduly raised. The quality of metal which the Farnley Iron Company desired to produce was that which possessed the maximum ductility when cold, together with the capability of sustaining, without failure or injury, the severe tests required by the most difficult flanging and welding when hot. In this respect the results of the mechanical tests which will be submitted are a sufficient proof of success; the chemical features will be dealt with separately. But it may here be remarked that Farnley Best Yorkshire cold-blast pig-iron has been the only phosphoric pig-iron used; and, being so low in silicon and sulphur, regular in quality, and more free from sand adhering to the pig than the cheaper hot-blast irons, a large portion of the successful results obtained must be credited to the principal raw material used. Two typical casts will now be given (229), one in which pig with a small admixture of wrought-iron scraps was worked with ore; and another (375) with less pig, more scrap, and no ore:

	Tons.	cwt.	qr.	lb.
No. 229. Charge 6.30 A.M. Farney No. 3 pig..	1	8	0	0
June 29, 1882. Charge 6.30 A.M. Steel Scrap.	0	8	2	0
Charge 2.45 P.M. Hematite pig No. 3.	0	2	0	0
Charge 3.10 P.M. Spiegel, 18 per cent. Mn.	0	1	1	0
Charge 3.25 P.M. Ferro-Mn., 53 per cent.	0	0	1	5
	2	0	0	5

Commenced adding ore and lime at 10.30 A. M., 4½ cwt. Sommorostro ore, and 1¾ cwt. burnt lime; 2 cwt. of slag run off 2.40 P. M. Sample flattened, quenched, broke tough, without any crystal in fracture, 3½ cwt. of slag removed. Cast 3.28 P. M. One 15 inches square, and one 12 inches square, full ingots.

12-inch ingot hammered into a slab and rolled into a plate 8.10×5.4½×½ inch bare. Strips from plate heated red, quenched, bent cold, and closed to ⅜ inch between folds without crack. The plate, after annealing, was bent to 2 feet 9 inches diameter, welded longitudinally, and flanged at both ends without crack or failure.

[Tests and analysis are given afterwards.]

	Tons.	cwt.	qr.	lb.
No. 375. Charge 6.30 A.M. Farnley No. 3 pig.	0	17	0	0
Charge 6.30 A.M. Common wrought-iron scrap.	0	6	0	0
Charge 6.30 A.M. Wrought turnings.	1	2	0	0
Charge 1.20 A.M. Hematite No. 3.	0	2	0	0
Charge 1.20 A.M. Ferro-manganese, 71 per cent.	0	0	2	19
	2	7	2	19

Four cwt. of slag taken off, and about 1½ cwt. left on metal, 1.10 P. M. Sample flattened, quenched, broke very tough, no crystals in fracture.

Cast 2.25 P.M.,	Ton.	cwt.	qr.	lb.
1 ingot, 19 inches square..	1	17	0	26
1 piece, 12 inches square..	0	5	2	14

Hot and cold tests satisfactory. Phosphorus in finished steel=0.056 per cent.

The design of the furnace in several respects unsatisfactory for basic work. When lime is added during the melting, small quantities carried by the draught on the faces of the silica bricks forming the ports, rapidly flux the gas and air passages, which causes a silicious liquid to trickle down across the furnace, and a further fluxing of the lime basin on the top of the bridge-plates B is occasioned by the droppings from the faces of the ports. The author has not yet succeeded in making a lime brick that will withstand the current of gas and air, and the cooling down of the furnace after tapping. Under the combined effects, disintegration of the lime brick occurs, and any failure of the ports must be repaired at once—for which the furnace has to be cooled down. Usually the ports need repair after eighteen to twenty-one days' work; the side wall on the tapping side lasts about twice as long as the ports, and the crown of the furnace in the center requires minor repairs at the same time as the sidewalls. Showers of minute sparks, with brown smoke at the chimney top, are given off in the later stages of the process, and the regenerators require cleaning out after sixty or seventy casts. The slag tap holes in the furnace used are too near the jambs between the charging doors, but from the arrangement of the furnace no other position could be found for them. To remedy these inconveniences, the author has designed another furnace, in which the gas and air passages are carried up outside the hearth and in separate casings, so that they may be changed and the inlets repaired without cooling down the furnace. This arrangement of gas and air ports has been patented by Messrs. Hackney & Wailes. Three doors for charging and repairing, and two slag-spouts are provided on the opposite side to the tap hole for the metal, and two sight holes for inspecting the furnace while working. The other reference letters apply to the same parts as the furnace before described. Owing to the very soft quality of steel produced the heat required is nearly as great as Sheffield silica bricks are able to withstand; so long as the slag can be kept clear of the magnesia and bauxite bricks they have been satisfactory, and, but for their great cost, magnesia bricks would doubtless be much better than silica bricks for faces of the ports. The waste is not excessive; taking the more recent work, without charging or crediting skulls and pit-scrap, the results are as under:

CASTS 302 TO 408 (inclusive).

Materials Charged.

	Tons.	cwt.	qr.	lb.
Farnley pig-iron............	90	8	0	0
Hematite pig-iron..........	9	18	2	0
Iron and steel scrap........	130	5	0	16
Ferro-manganese...........	4	14	2	14
	235	1	1	2

The total weight, including spare metal, of ingots produced from the above is 219 tons 6 cwt. 3 qrs. 26 lbs., or about 93 per cent. of the raw materials. About 9 cwt. of raw dolomite per ton of ingots has been used for the repairs of the furnace, as deduced from the work done during one year. The following mechanical tests are from casts of the numbers annexed; nearly all the plates have been for boiler-works requiring difficult flanging; excluding some three or four accidental casts, they are a fair average of the quality of steel:

Breaki'g Weight per Square Inch.	Elongation in Inches.			Reduction of Area.	Description of Piece Tested.
	10	8	2		
Tons.	Per cent.	Per cent.	Per cent.	Per cent.	
25.89	25.00	27.50	53.12	55.75	No. 229 plate from ingot 12 inches square; size, 2.015 inches by 0.46 inch.
22.29	31.25	34.06	53.12	62.90	No. 242. Axle from 15 inches square ingot; billet turned to 1 inch diameter.
24 96	35.00	37.50	62.50	62.78	No. 245. Plate from 12 inches square ingot; size, 2.08 inches by 0.37 inch.
23.00	30.00	31.25	62.50	63.48	No. 246. Plate from 15 inches square ingot; 1.99 inch by 0.755 inch.
24.16	27.50	30.47	—	56.53	No. 289. Plate from 15 inches square ingot; size, 2.035 inches by 0.59 inch.
23.92	28.12	34.18	59.37	58.25	No. 304. Plate from 19 inches square ingot; size, 1.52 inch by 0.68 inch.
25.98	24.37	26.56	40.62	50.00	No. 305. Billet from axle from 19 inches square ingot, turned to 1 inch diameter.
26.68	23.75	25.00	43.75	49.64	No. 313. Plate from 19 inches square ingot; size tested, 1.765 inch by 0.69 inch.
26.68	26.25	28.47	53.12	49.77	No. 315. Plate from 2 feet 3 inches by 1 foot ingot; size tested, 1.52 inch by 0.705 inch.
24.07	28.75	31.25	50.00	48.93	No. 345. Plate from 19 inches square ingot; size tested, 2 inches by 0.565 inch.
24.76	27.99	30.62	53.12	55.49	Average.

All the test pieces were parallel for a length of 11 inches, plates being machined on the edges only; a space of 10 inches long was in every case divided into ten equal parts. The cases in which a length of two inches adjacent to the fracture showed a greater elongation per cent. than the contraction of area per cent., are explained by the concave shape of the ruptured edges, so that when the pieces were joined for measuring the elongation, the fractured edges were in contact at the sides only; and the true elongation is consequently less than the distance between the points from which the percentages in the table are calculated; this only occurs with rectangular sections.

The pig-irons used were of the following composition:

Analyses by J. O. Arnold, F.C.S., Ironmaker.

	Farnley.	Hematite.
Iron	93.111 (by difference)	93.194
Graphite	3.250	3.798
Combi'd carbon	0.392	0.410
Silicon	1.245	2.285
Sulphur	0.013	0.004
Phosphorus	0.601	0.058
Manganese	1.188	0.199
Titanium	—	0.152
	100.000	100.000

The wrought-iron scraps used were not all of high quality, and would average not less than 0.2 per cent. of phosphorus; consequently, charges 229 and 375, before given, would respectively contain 0.46 and 0.35 per cent. of phosphorus at the commencement, the samples taken from the casting ladle in both cases showing that the phosphorus had been reduced to 0.067 and 0.056 per cent. respectively. Other phosphorus tests from casts for which the charges were similar to 375, the selection being made from a number, are as follow:

Casts, No.	242	271	273		274	275	276	283
P. per cent.	0.065	0.078	0.090		0.097	0.092	0.086	0.097

Casts, No.	287	300	301	305	335	336	340	348	351
P. per cent.	0.076	0.083	0.082	0.050	0.083	0.076	0.064	0.079	0.053

Casts, No.	352	360	375	382	383	402
P. per cent.	0.060	0.071	0.056	0.055	0.056	0.062

All the foregoing phosphorus tests were from ladle samples taken after tapping, except No. 305, which was drilled from a finished forging; some of those given are of casts where the phosphorus was expected to be somewhat high. The author is indebted to Mr. J. O. Arnold, F. C. S., for all the analyses of metal and slags, excepting that of the hematite pig. As in three samples of Farnley pig-iron sent to other chemists the phosphorus was respectively returned as 0.38, 0.317, and 0.29 per cent., and also as the percentage of phosphorus in the finished steel appears high compared with the published results of bassic Bessemer work, the author requested Mr. Arnold to furnish him with an account of the method by which the phosphorus was estimated, and his note is given in an appendix to this communication. The order of removal of phosphorus is evidently different from that which it has been shown to be in the Bessemer converter. In the latter process it has been proved that the phosphorus is not eliminated to any great extent until the carbon and silicon have almost disappeared; consequently the author was scarcely prepared to find so small an amount as 1.974 per cent. of phosphoric acid in the final slags of charge No. 229. He therefore took a sample of slag from No. 301, which had been run off after the pig was fluid, but before the scraps were melted, and this sample, when tested, afforded 5.087 per cent. of phosphoric acid. Athough samples of metal have been taken when the whole charge was melted, and before boiling commenced, the phosphorus in the melted metal was always found to be much lower than the charge was known to contain originally, proving that the elimination of phosphorus in the basic open-hearth process commences in the early stages. With respect to the removal of sulphur, the results were not so encouraging. The preceding table gives three tests: (*a*) when the charge was all melted; (*b*) from the flattened test sample; and (*c*) from the finished steel. They were taken from casts in which scraps known to be high in sulphur were purposely tried.

No. of Cast.	Sulphur.			Phosphorus.		
	Per cent. *a*	Per cent. *b*	Per cent. *c*	Per cent. *a*	Per cent. *b*	Per cent. *c*
213	0.226	0.192	0.218	0.114	0.066	0.070
214	0.230	0.182	0.172	—	0.039	0.041
225	0.200	—	0.189	—	—	—
227	—	—	0.107	—	—	—

None of these charges were satisfactory under the hot tests. The evolution of sparks and brown smoke has been before referred to, and as the author in several instances was unable to balance the phosphorus in the charge with that contained in the slags added to the phosphorus left in the finished steel, he took samples of the deposits in the regenerators. One sample from the gas regenerator contained 4.741 per cent., and another sample from the air regenerator gave 2.995 per cent. of phosphoric acid (P_2O_5), the other constituents being S_1O_2, Fe_2O_3, CaO, and MnO. These deposits appear to indicate that some of the phosphorus leaves the furnace otherwise than with the slags. Three analyses of finished steel are furnished from charges that have been already given in detail; one sample (229) was taken by a sample spoon from the casting ladle, but the other two, Nos. 245 and 345, both similar charges were drilled from the specimens tested in the machine, the tensile strengths, &c., of which are included in the table of tests:

	No. 229.	No. 245.	No. 345.
	Per cent.	Per cent.	Per cent.
Carbon.....	0.240	0 140	0.140
Silicon......	trace	trace	0.004
Sulphur.....	0.060	0.037	0.074
Phosphorus..	0.067	0.056	0.050
Manganese..	0.526	0.191	0.598

A quantity of slags from pig and scrap charges similar to Nos. 245 and 345, all being taken from those run off before the final additions and ground up, gave an analysis S_1O_2 17.830, and P_2O_5 2.726 per cent.; and a complete analysis of slags tapped out of the furnace with the metal, and consequently taken after the additions of hematite pig and ferro-manganese, is given on following page.

	Per cent.		Per cent.
P_2O_5	0.806	CaO	39.700
S_iO_2	13.640	MgO	11.750
Fe_2O_3	1.870	MnO	8.762
FeO	18.571	S.	0 119
Al_2O_3	2.235	Alkalies and loss	3.047
			100 000

The author has purposely omitted the question of cost, but it may be stated that the process is more expensive as regards furnace repairs, and greater attention is required in working, although the number of men employed at the furnace is not greater than required by the acid process. The preparation of the material for repairs, namely calcining and grinding dolomite, and burning limestone for adding, is not a part of the duties of the melters, and involves greater expenses than those for corresponding work in the Siemens or Siemens-Martin processes. For the production of exceptionally soft steel of great purity, and the utilization of much wrought-iron scrap and certain kinds of phosphoric pig that in the present acid processes are useless for good steel, the basic open-hearth process offers peculiar advantages, and will doubtless be further developed in the future.

ON PERSONAL SAFETY WITH ELECTRIC CURRENTS.

By Prof. A. E. DOLBEAR.

From "The Engineer."

The serious accidents which have occurred within the past four or five years through accidental contacts with wires carrying strong electric currents have seemed to call attention to the necessity of providing safeguards against such mishaps. It is probable that carelessness has been the cause of most of the accidents reported, and it is true that if weak currents only were used nobody could be hurt. It is, however, worth while to consider the electrical relations of the human body in order to learn where danger lies. I have noted in various places the opinions of different persons as to what constituted a safe current. In one of the old books it is stated "a spark 18in. long begins to be dangerous." In another place one says that a difference of potential of 800 volts is too high an electro-motive force for individual safety.

Now a difference of potential of 1,000 volts will not give a jumping spark the hundredth of an inch long. The electromotive force developed by the Holtz electro machine may be as high as 50,000 volts, and the spark from it will at most give a spasmodic jerk to the elbow and no manner of hurt come of it or of repeated shocks from it, so that more than difference of potential must be considered in determining what is dangerous about electricity. The ability of electricity to do work of any kind, destructive physiological work as well as any other, depends upon both difference of potential and current strength, and the amount of work is proportional to the time also. Now the discharge from a Holtz machine through a short wire may give a very strong current; for example, let $E = 50,000$ volts, $R = .001$ ohm, then $\frac{50,000}{.001} = 50,000,000$—fifty millions of amperes; but the wire may show no sign of being injured, for the time of the current's passage was too short, probably less than the millionth of a second. If the discharge had continued for an appreciable part of a second the wire would have been vaporized. The discharge is probably equally quick when taken on the knuckles, but there is not enough energy to hurt anything. Again, the amount that will traverse any conductor, with a given difference of potential between its ends, will vary inversely as its resistance, and for the human body it may only roughly be calculated. The resistance of the body is very great. Many measurements, made with different individuals taking wires in their fingers and hands, gives a resistance varying between 6,000 and 15,000 ohms; but this depends in a large degree upon the moisture of the skin when contact is made. Hands which are ordinarily dry have a high resistance;

the same hands moist with perspiration or purposely wetted may lose half their resistance. Dr. Stone, of London, has made many experiments, and finds that the resistance of the body may be reduced to 500 ohms or less, by having the skin soaked.

On an arc light circuit with forty lamps the difference of potentials will be about 2,000 volts. If a man with a dry, thick skin were to grasp the terminals at the dynamo, the current that would go through him would be $\frac{2000}{10000} = .2$ of an ampere, and in one second two-tenths of a coulomb would have traversed his arms. If the same hands were soaked, the current might be $\frac{2000}{500} = 4$ amperes, or 4 coulombs per second. In the first case there would have been spent in him an amount of energy equal to $2000 \times .2 =$ 400 watts, or more than half a horse-power, and in the second case $2000 \times 4 =$ 8000 watts, or $\frac{8000}{746} = 10$ horse-power in the interval of one second. Now, any electromotive force above about 1.5 volts is sufficient to decompose water, and it has been shown that the fluids of the body are better conductors of electricity than any of the tissue, even the nerves; it may fairly be inferred that such a current with such an electro-motive force might decompose a notable quantity of the fluids of the body into their constituent gases.

To determine the highest limit of safety would require experiments which no human beings would be willing to submit to. Animals might be employed perhaps; but if 800 volts have at any time proved to be dangerous, one may set a lower limit to the resistance of the body and determine the current strength $\frac{800}{1000} = .8$ amperes. That result looks threateningly large, but it is because the resistance is made so low. I have never found one less than 5,000 ohms, then $\frac{800}{5000} = .16$ amperes. But that represents 128 watts—a quantity of energy no one would care to have spent in him. After all, what would be safe for one would be entirely unsafe for another; so that each individual would have his own factor of safety, or the difference of potential which he could work with, with impunity. From what we now know it would seem as if one-tenth of an ampere current body was as much as any one could safely have traverse his body for one second. If, then, he fixes his minimum resistance, the product of the two will give him the electro-motive force which he may feel timidly safe with. Thus, if one's resistance with wet hands is found to be 8,000 ohms, then $8{,}000 \times .1 = 800$ volts; while if his resistance was only 1,000 ohms, he could only venture to touch wires having a difference of potential of $1{,}000 \times .1 = 100$ volts. The ordinary incandescent light circuit would be on his limit.

THE EMPLOYMENT OF DOUBLE FLOATS FOR MEASURING VELOCITIES IN LARGE STREAMS.

By H. BAZIN.

Translated from "Annales des Ponts et Chaussées," for Abstracts of the Institution of Civil Engineers.

A comparison is made in this article of recent important and careful observations of the velocities of flow in large streams, with the object of showing that the discrepancies between the results are merely apparent and due to the nature of the instruments, and with the view of indicating the corrections necessary in the results of observations with double floats.

The current-meter, with an electric recorder, is the most reliable instrument; but if the experiments are conducted in a deep rapid current, the apparatus must be costly. The double float provides a simple and ready method, but it is liable to errors, which increase so much with the depth and velocity of the current as to render the results useless in some cases. The conditions necessary to ensure perfect accuracy are, that the two floats should travel with the same speed as the layer of water surrounding the bottom float, and that this float should retain its original position in the stream. Unfortunately, the surface float and cord modify the motion of the lower float, and

the lower float is raised by the eddies of a rapid current; and when the velocity exceeds 5 feet a second no reliance can be placed on the position of the bottom float.

The experiments compared are those of Mr. Ellis on the Connecticut, 1874, with a current-meter and floats; those of Major Allan Cunningham on the Ganges Canal, in 1874-79, with floats; those of Mr. Harlacher on the Elbe and the Danube, in 1876-79, with a current-meter; those of Messrs. Nazzani and Zucchelli on the Tiber, in 1880-81, with a current-meter and floats; and those of Mr. Gordon on the Irrawaddi, in 1882, with a current-meter and floats. Numerous Tables are given of the results of each series of experiments.

In Mr. Ellis's experiments the velocity of the stream was small, the greatest velocity hardly exceeding 3¼ feet per second; whilst the area of surface of the top float and connecting cord was only one-sixth of that of the bottom float, so that the bottom float was not liable to be affected by eddies, and could be little influenced by the motion of the cord and top float. Accordingly, as might be anticipated, the results of the two methods of observation fairly coincide. The surface-velocities obtained by the current-meter are evidently too low, owing doubtless, either to the ripples over the surface of the water, or the eddies produced by the boat from which the current-meter experiments are taken. On the other hand, the velocities given by the double float are manifestly too great for the lower depths. As in these experiments the velocities are very low, the slightest errors modify the results; so that the parabola, indicating the decrease in velocity in a vertical plane in a line with the current, cannot be traced with precision. The proportion of the surfaces of the upper float and connecting cord to the surface of the lower float was a little over one-half in Major Cunningham's experiments; and the velocity of the current was greater than in Mr. Ellis's observations. The increased relative proportion of the surface of the upper float and cord, and the increase in velocity, together with the fact that Mr. Ellis's observations were conducted on a river and the others on a uniform canal, materially reduce the proportion between the maximum and mean velocities, and the parameter of the parabola marking the decrease in velocity. In Mr. Harlacher's experiments the value of the relation between the maximum and mean velocities is larger than that obtained by Major Cunningham, the maximum velocity of the streams being also larger. The maximum velocity was almost always at the surface in the Elbe observations, taken at Tetschen; and the parameter reached, on the average, the value of 0.61, which is higher than in the previous observations. The upper float and cord in Mr. Zurchelli's observations on the Tiber exposed a surface of nearly one-fifth of that of the lower float, in depths ranging between 20 and 23 feet, and about two-fifths when the river had risen so as to attain depths of 33 to 50 feet. In every case, in these experiments, the relation of the velocities observed at the various points to the mean velocity approximated to unity, the values diminishing with the height of the river for points near the surface, but increasing for points near the bottom. Comparing the above results with those on the Mississippi and the Irrawaddi, it appears that in these rivers the relation of velocity to mean velocity, and the parameter also, decrease in proportion as the depth increases, but are smaller. especially on the Mississippi. This progressive decrease is due to the influence of the surface of the connecting cord, which increases with the depth, and ends by completely vitiating the results in the great depths of 70 feet and 110 feet reached by the Irrawaddi and Mississippi respectively; for the relation of the surfaces of the upper float and cord to the surfaces of the bottom float, whose maximum on the Tiber is two-fifths, attains four-fifths on the Irrawaddi, and unity on the Mississippi. The results obtained by Mr. Nazzani with current-meters on the Tiber do not coincide with those from the double floats, as the relation of the velocities and the parameter increase in the experiments with the current-meter and decrease with the floats. In Mr. Gordon's recent comparative experiments on the Irrawaddi with current-meters and floats, the results obtained are very different. The proportion between the velocities observed at the various points and the maximum velocity decreases with the depth in both cases, but the decrease is much more marked in the results obtained by the current-meter; and in

some instances the results of the two methods at the greatest depths differ by one-third of the maximum velocity. Mr. Gordon's observations prove beyond a doubt that the employment of double floats for measuring velocities, at considerable depths in rapid currents, leads to serious errors. On the other hand, these experiments also indicate, as in previous results, that the current-meter furnishes too low velocities at the surface.

Comparing the results of the preceding experiments, it appears that the relation between the velocities varies but little in the observations with current-meters, being always between 1.14 and 1.20; whilst this relation in the case of floats is lower and more variable, ranging between 1.02 and 1.14. The influence of the cord connecting the two floats is more marked as the depth and velocity increase. It is evident that gaugings conducted with double floats furnish too large values for the discharge, and should be revised; whilst the unexpected rise of the proportion between the maximum and mean velocity on several large rivers necessarily implies a greater impediment to the flow, so that the formula ordinarily employed would also give too large discharges. The old formulas of Prony, Eytelwein, and others, were based upon a limited number of observations, representing simply the steady flow of a moderate sized river for which the formula $v=50\sqrt{RI}$ was generally adequate. The observations on the Seine and Saône show that for these gently flowing rivers the value of the coefficient c may be made 50, as in the above formula. Mr. Graëff has found $c=36$ to be suitable for the tributaries of the upper Seine. It appears that for the Rhine at Basel, in spite of the size of the river, the value of c is as low as 38, owing to roughness of the bed, which is covered with large shingle. Even very large rivers are affected by the irregularities of their bed, so that for the Danube at Vienna $c=42$. In the Irrawaddi also, the value of b is less than 50. The Mississippi experiments give anomalous results, and very high values for c where the inclination is very small; but taking only the most reliable results, the mean value of c would be 70. These observations, however, like those on the Irrawaddi require correction, which would reduce the value of c below 60. Observations with current-meters are needed on the very large rivers, from which a suitable coefficient might be deduced, so that serious errors in the calculation of their discharges may be avoided, for which at present there are not sufficiently accurate data.

THE TRANSMISSION OF POWER BY ELECTRICITY.

By ROBERT LUCE, A. M.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

The advantages to be gained from an economical method of transmitting power are self-evident. Many systems have been tried and found wanting. Endless ropes and chains are too cumbrous and expensive. Waterpipes are liable to all sorts of catastrophes, and water-power from artificial reservoirs is both costly and inefficient. Gas motors are of limited power, and compressed air is hard to manage. But in electricity, thanks to living inventors, we have a force into which power can be transferred as by the wave of the magician's wand, a force which flows to any distance with the rapidity of lightning and the stillness of a ray of sunlight, and then at bidding resumes its original shape quicker than thought can follow it. This force has been known to the world for ages, but it is only within the past decade that its vast possibilities have taken definite shape in the mind of science. The electric transmission of power is yet in its childhood, but its growth has been so wonderful that the world has not been able to keep up with it. Words written about it a year ago are now behind the times. Young as it is, the science of dynamical electricity has already become of such worldwide importance that its history, present condition, and possible future are of general interest.

The keystone of modern science is the

truth that matter and energy, the components of the universe, are never destroyed. To put the converse of half of this truth into scientific parlance, energy is always conserved. It may change its form or its place, but it never increases or diminishes, never grows, never shrinks. Energy may be active, or it may be dormant,—in scientific phrase, potential. The great problem which confronts mankind to-day—which always has confronted, always will confront the human race—the problem of problems in the physical world is, how to utilize to the best advantage the energy dormant in matter? How shall we get the most work out of coal, wood, water, air, and every other form in which matter presents itself to us? Energy is seldom in the place we want it, almost never in the form we want it. We must carry it to the desired place in its original bonds, as, for example, in coal, or we must transform it and carry it in some new prison, as, for example, in coal gas. The question is, how can we transport energy so as to lose the least of it while performing the least labor? Students of dynamical electricity maintain to-day that they have solved the problem. They maintain that energy can be transmitted by electricity more cheaply, with less net loss than in any other way known to modern science. The claim is surely worth investigating.

To discuss intelligently the methods by which energy, alias power, is usually transformed into electricity, it will be necessary to go back to the beginnings of electrical science. And yet it is not so very far back, for it was but a little over a century ago that the science was in so rude a state that the Electoral Academy of Bavaria actually proposed the following subject for a prize dissertation: "Is there a real and physical analogy between electric and magnetic forces, and if such analogy exists, in what manner do these forces act upon the human body?" At that time physicists were divided as to the correct answer to the question at issue, and for 45 years they quarreled over it, until at last Oersted settled it forever by demonstrating the magnetic properties of electric currents. In 1829 this Danish physicist noticed that a magnetized needle was deflected from its direction when it was placed near a closed electric circuit. The same phenomenon occurring when the current was replaced by a magnet, it became evident to him as to all contemporary physicists that a complete analogy existed between electricity and magnetism. From this first observation really dates one of the most beautiful achievements of the human mind in the domain of natural philosophy. Before 1820 the intimate relation between the electric current and a magnet had often been spoken of, and had even served as a basis for several electrical theories; but no one had rendered it palpably evident until Oersted's experiment opened to science the luminous path which scientific men have since trod with so much success. It was in 1820 that Ampere made known to the world the mutual action of two currents, and of magnets on currents; and in the same year Arago discovered that an electric current imparts magnetic properties to iron and steel. Ten years later (1830) Faraday supplemented the labors of Oersted, Ampere and Arago, by demonstrating that a magnet can create an electric current.

From the discoveries of these four men has been developed the science of dynamical electricity. Just as geometry was constructed from axioms, self-evident truths, so this science has been built up from certain truths which may be termed electrical axioms, and it cannot be too forcibly impressed upon the reader's mind that these electrical axioms must be thoroughly understood, with all their significance and in all their bearings, before proceeding further. They are known from the name of their formulator as Ampere's laws, and are:

I. Two currents which are parallel and in the same direction attract one another. (A).

II. Two currents parallel, but in contrary directions, repel one another. (B).

From these we deduce two more laws, which can easily be verified by experiment:

III. Two rectilinear currents, the directions of which form an angle with each other, attract one another when both approach or both recede from the apex of the angle. (C and D).

IV. They repel one another if one approaches and the other recedes from the apex of the angle. (E).

To continue our mathematical simile: In geometry, every proposition has its converse. Likewise the laws of parallel and angular currents have their converse in what is known as Lenz's law, which is:

"If the relative position of two conductors A and B be changed, of which A is traversed by a current, a current is induced in B, in such a direction that by its electro-dynamic action on the current in A it would have imparted to the conductors a motion of the contrary kind to that by which the inducing action was produced."

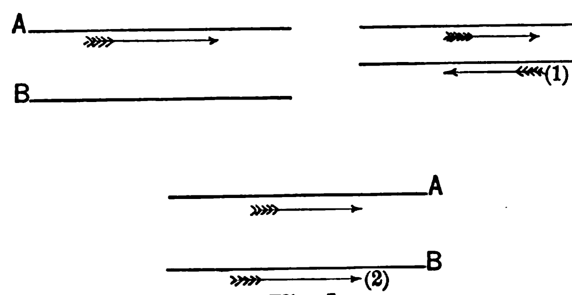

Fig. 1

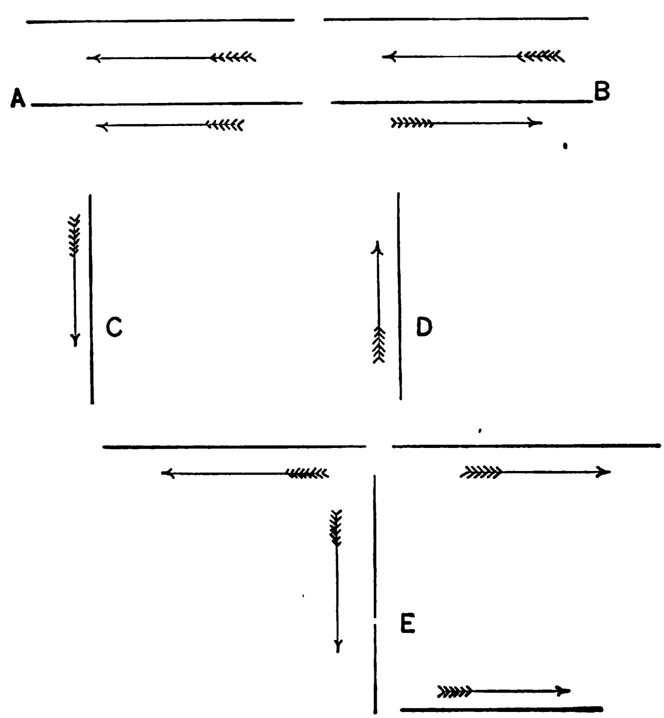

Fig. 2

In Fig. 2 suppose the conductor A be traversed by a current, but the conductor B not so traversed. If we cause B to approach A, a current will be induced in B in the direction of the arrow (I), for a current flowing in that direction in B would, according to Ampere's second law, tend to repel itself from the current flowing in A, and there would be a motion of the contrary kind to that by which the inducing action was produced. From this it is easy to see how motion will produce currents of electricity, and, *vice versa*, how currents of electricity will produce motion. The elaborate dy-

namo-electric machines of to-day are worked on these very principles according as they are used respectively as generators or motors. Strange to say, nearly half a century passed before any one thought of what they had in common; so we must consider them separately, and the two sets of machinery which slowly developed from their application.

MAGNETO-ELECTRIC AND DYNAMO-ELECTRIC MACHINES.

In 1820 the great Faraday succeeded in rotating a system of wires through which ran currents, by means of a permanent magnet placed near it, but he did not grasp the practical importance of his discovery, and it was reserved for Pixii, a French manufacturer of physical instruments, to make, in the year 1832, the first magneto-electric machine, *i. e.*, a machine for inducing currents of electricity in a wire or coil of wire by means of a magnet. Pixii revolved the poles of a horse-shoe magnet before the poles of a double electro-magnet. Thus two conductors changed their relative positions, and since one was traversed by a current, in accordance with Lenz's law a current was induced in the other. As this machine had the mechanical disadvantage of having the heavier part (the permanent magnet) put in motion, a change was soon made, so that instead, the magnets were fixed and the coils rotated. For many years after this step no material advance was made in their construction. They were unsatisfactory in use, because their effect did not increase proportionally to their dimensions, and therefore machines for the production of powerful currents were cumbersome and costly.

Werner Siemens and Mr. Wheatstone almost simultaneously, and quite independently, discovered the principle which puts our electrical generators so far ahead of those of half a century ago, viz., the principle of the accumulation of currents by their mutual action on one another. In a machine constructed on this principle the permanent magnet is replaced by an electro-magnet, which is put in the same circuit with the coils of the other electro-magnet revolving before it. There is always enough magnetism in the coils of the other when the machine is started. This induced current in the second induces a still stronger current in the coils of number one, and this new one added to the little one already there induces yet a stronger one in number two, and so it goes, back and forth, the strength of the current produced being limited only by the capacity for saturation which the coils possess. In this way a trace of magnetism suffices to originate torrents of electricity. Such a generator is commonly called a dynamo-electric machine.

Just here a few words about the terminology of the subject in hand. A machine using the principle of Pixii's, described above, *i. e.*, having permanent magnets and revolving coils, is called a magneto-electric machine, because "by a magnet electricity" is produced. On the other hand, where the principle of mutual accumulation by electro-magnets is employed, the term dynamo-electric—"by power electricity"—is applied. In point of fact, dynamo-electric and dynamo-magneto-electric would respectively be more accurate terms, but the others are in more common use. Later we shall come to a class of machines in which an electrical current generated by chemical means is transformed into power, and these are known as "electro-magnetic," without any real reason for the name, except that electricity and magnets are essential to their working. No distinctive name has been applied where a current generated by power is employed, but since we call the generator a "dynamo-electric" machine, we may for the same reason call the motor an "electro-dynamic" machine, for here we have "by electricity, power." For the sake of brevity, "dynamo-electric" is often shortened into "dynamo." I shall take the same liberty with "electro-dynamic" in calling the two sets of machines respectively "dynamos" and "electros." The armature is the soft iron core with coils of insulated copper wire about it which revolves between or before the poles of the magnets or magnet.

A commutator is a device for taking the currents off the armature of a dynamo and uniting them into one which shall flow continuously in one direction. There are many different arrangements in use for this purpose, but the explana-

tion of one will be sufficient to show the principle of all.

Suppose D to be the shaft of an armature made of two pieces of copper, A and C, with a strip, B, of ivory or some other non-conducting substance between. A is connected with one end of the coils in the armature and C with the other.

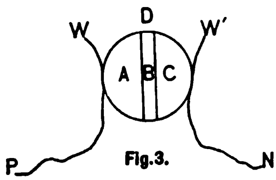

Fig.3.

As the armature revolves between the poles of an electro-magnet, half its coils are always leaving the south pole of the magnet and going toward the north pole, and as the north pole induces the same sort of a current in the wire coming toward it as the south pole does in the wire going from it, the currents in that half of the armature will have the same direction, and *vice versa* the currents in the other half will have the opposite direction. When these halves pass the pole, the direction of the currents in them is respectively reversed. But at the same time the connection between the strips A and C and the wires W and W′ is reversed, so that a positive current, P, will always flow from one wire, and a negative, N, from the other. The wires W and W′ are kept pressed against the revolving shaft by springs. The sparks seen when the dynamo is in motion are caused by the imperfect connections made by these wires, or commutator brushes, as they are called. They are the only parts of a dynamo liable to wear and tear, and as they can be easily replaced, the item of repairs is, or ought to be, very small in the cost of running the machine. The only difference between various machines lies in the arrangement of coils, cores, armatures and electro-magnets. Effects of tension (intensity) or quantity are produced by using fine or coarse wire in the coils.

Under the head of the transmission of electricity it will be enough for the present to state what may seem a truism, viz., that a considerable current of electricity can be transmitted over a wire to a long distance. Let us now consider the history and principles of those machines that convert the electricity into power.

Among the many experiments which Faraday made in electrical science, after the discoveries of Ampere and his contemporaries, were some to ascertain the effects of currents upon one another and upon passive conductors. As we have seen, Ampere discovered the attractive and repulsive powers of currents, and no doubt was the first actually to produce motion by means of electricity, but Faraday was the first to make any extended experiments in this direction, and he should have the credit of first bringing the motory powers of electricity into prominence. It has been claimed that the first machine actually moved by "electro-magnetism," as it used to be called, was invented by Professor Henry, of Albany, New York, consisting of an oscillating beam surrounded by a conductor of insulated wire and two stationary magnets. The proofs advanced in support of this claim are not positive. However that may be, it seems to be well established that the first rotary motion by means of an electric current was the production of an American, one Thomas Davenport, of Brandon, Vermont. His claim to this honor has not of late years been pushed or even published, and the man and his invention have been forgotten. Still there is positive evidence that in 1834 he invented and exhibited an electro-magnetic engine. If any European produced a rotary motion in this way before this date, I have been unable to ascertain it. I beg leave here to put in a plea for Mr. Davenport, and to claim for America the honor of first utilizing the force which is destined to revolutionize the mechanical systems of the world. The originality of this plea may be judged when we read among the writings of the celebrated physicist, Ganot, that "M. Jacobi, of St. Petersburg, was the first to construct an electro-magnetic machine, with which, in 1838, he moved on the Neva a small boat containing twelve persons." By reference to the Boston papers of the year 1835, it will be seen that in that year Davenport exhibited in Boston an electro-magnetic engine running on a small circular railway. Three years before Jacobi's boat swam the waters of the Neva, Daven-

port's engine, the result of American application, of American perseverance, of American intelligence, ran along the rails, giving America the honor of being the first to harness the giant force and compel it to do her bidding. To the combined minds of England, Italy and Denmark, we owe the discovery of "electricity by power," but to America, to Professor Henry and Thomas Davenport, we owe the discovery of "power by electricity."

After Davenport's discoveries, Prof. Page, of Washington, took up the subject and made many practical inventions. Among them was a large and efficient electro-magnetic engine of sufficient power to propel a railway car with considerable velocity. It would be interesting to touch upon some of the others, but they are hardly relevant. I would only mention a fact little known at the present day, viz., that Prof. Page has shown very strong evidence to prove that the honor of inventing the Ruhmkorff coil should be given to him. Here again America was possibly several years ahead of Europe. My authority for this, and for the statements about Davenport's invention, is Silliman's *Journal of Science and Arts*. A pamphlet on "The History of Davenport's Invention of the Application of Electro-Magnetism to Machinery" gives much information on the subject.

ELECTRO-MOTORS.

The most elementary arrangement for producing motion by electricity would be, of course, a simple mechanism operated in accordance with Ampere's laws, that two parallel currents in the same direction attract, and in opposite directions repel, one another. If we place a magnet so that it may move freely before the poles of an electro-magnet, and if by clockwork, or some other device, we send a current in alternate directions through the coils of the electro-magnet, then, since in accordance with the Amperian theory a current is continually flowing around the permanent magnet, the latter will be alternately attracted and repelled. In other words, the current of the electro-magnet will have produced motion.

The simplest electro-motor is Froment's rotating engine. This consists of an electro-magnet radially outside the periphery of a drum capable of rotation. On this periphery are a series of soft iron coils. As the drum revolves it completes a circuit, by suitable make and break pieces sending a powerful current from a battery through the electro-magnet as each coil approaches the pole within 15° or 20°; the electro-magnet then attracts the armature, and the drum is forced to continue its revolution. The circuit is interrupted as the coil passes the electro-magnet and the magnet therefore unmade. The drum continues its rotation by inertia, or by the action of another electro-magnet, until a second coil approaches the poles of the first electro-magnet, when the circuit is made as before.

Jenkin in his "Electricity and Magnetism," says: "Another form of electromotor is constructed resembling the ordinary beam steam engine; the piston is represented by a magnet which is alternately sucked into a hollow coil, and repelled as the current in the coil is reversed; sometimes a soft iron piston is used, which is alternately attracted and set free.

"Much more attention would be directed to electro-motors than they have hitherto received, were it not for the fact that they are necessarily at least fifty times more expensive to maintain in action than the ordinary steam engine. Zinc is the cheapest metal by the consumption of which electricity is produced. The energy evolved by the consumption of one grain of zinc is only about $\frac{1}{10}$ of that developed by the consumption of a grain of coal. A large fraction of the energy in the case of the zinc can be converted into an electric current, whereas we have not yet discovered any means of obtaining the energy of coal except as heat, and we necessarily waste a great part of this heat in the process of transforming it into mechanical energy. In the transformation of energy into mechanical effect the advantage lies with electricity. The whole of the energy either of heat or of an electric current can never be transmuted into mechanical effect. In the best steam engines not one quarter of the heat is so transformed; more frequently about a tenth is used. It is probable that larger fractions of the total

energy than these could be transformed by an electro-motor into mechanical effect; but this advantage, even if realized, cannot nearly counterbalance the disadvantage entailed by the cost of zinc, which is 20-fold that of coal, weight for weight, 200-fold that of coal for equal quantities of potential energy. In estimating as above, that the zinc motor may be only 50 times as dear as the coal motor, I assume that the electro-motor may be four times as efficient as the heat engine in transforming potential into actual energy."

Electro-motors have assumed a new importance since the invention of dynamo-electric machines has made possible the production of electricity at such a comparatively small expense. The only trouble with Davenport's invention, with Page's engine, and with the innumerable other motors which have promised literally to electrify the world, has been the comparatively high cost of obtaining electricity to run them. Now that the dynamos can be run so economically, the question of electro-motors assumes a more important aspect. It is this that gives to the subject of the transmission of power by electricity interest and importance.

ELECTRICAL TRANSMISSION THEORETICALLY CONSIDERED.

For many years the two sciences of magneto-electricity and electro-magnetism were growing up, developing, side by side. But at last some one saw what they had in common, united them, and, lo! a new science sprang into being. The exact time of the origin of the idea of using the electricity generated by power to produce power in turn, and the identity of the man who conceived it are alike uncertain. J. Chrétien states that the first attempt to transmit motive power by electricity was made in 1873, at the Vienna Exposition, by H. Fontaine. Certainly in 1876 at Philadelphia, and in 1878 at Paris, the experiment was made. It is enough for us that, to-day, the electrical transmission of power is an accomplished fact.

The underlying principle in this transmission is the coupling of two or more dynamos. Let the current generated by one flow over a short circuit, and let another machine be placed in the circuit; then the electro-magnet of the second will become excited and a current will also pass through the coils of the armature. By Ampere's law two currents attract or repel one another according as they run parallel or in opposite directions. Suppose in this case the first motion is repulsion and the armature begins to rotate. As its coils respectively approach the pole opposite the one by which they were repelled, they will be attracted. As soon as they pass the pole, the commutator will have changed the direction of the current and they will be repelled. On approaching the original pole from which they started, they will be attracted, since the current still remains inverse. We have now followed the armature through an entire rotation. This rotation will evidently be kept up, and the stronger the current the faster the armature will turn. By connecting shafting with this revolving armature the power may be utilized.

It has now been shown, first, that power can be made to generate an electric current; second, that an electric current can flow to a considerable distance; third, that an electric current can generate considerable power; in other words, that electric transmission is possible. The question now comes up, is it practicable? To answer this we must first ascertain two things: (1) How much power can be advantageously employed? and (2) How much of it can be reclaimed?

As to (1), we know by repeated experiment that up to a certain point, not far from 50 revolutions of the armature per second, the strength of the current produced is essentially proportional to the speed of rotation. If a strength of current is desired greater than given machines will produce with a rotation of 50 turns to the second, the size or number of machines employed may be increased, so we may conclude for practical purposes that an indefinite amount power may be employed.

As to how much power can be reclaimed, there has also been much experiment, and the scientists do not altogether agree; but as nearly as I can judge from the various results obtained, I am warranted in setting the practical efficiency of a dynamo at about 85 per cent. That is to say, about 15 per cent. of the

power expended on a dynamo will be lost in the process of conversion into electricity.

It might be inferred that if an exactly similar dynamo were used as an electro (a motor), its efficiency would be 85 per cent., and, therefore, leaving out of account the transmission of the electricity, 85 per cent. of 85 per cent. might be reclaimed. But here another factor comes in, viz., the induction of counter-currents in the electro, which is the practical hindrance to reclaiming so much as this. As has been shown, when a current is conducted into the coils of the electro, the armature will begin to revolve. Its rotation is exactly the same as it would be if the motive power came from a pulley belt attached to its wheel. The result is that currents are induced in the coils. But these currents are directly the opposite of those which are produced by the dynamo and which are giving the armature of the electro motion. These counter currents lessen the effect of the direct current from the dynamo proportionally, and the faster the revolution the stronger the counter-currents. Then, since the strength of the current from the dynamo is greatest when the electro is at rest, it will be readily seen that when the most work is obtained from the electro, its armature is revolving at one-half the speed of that of the armature of the dynamo. Hence, if two equal machines are arranged for the transmission of power, the amount of work reclaimable from the second machine will be in general 50 per cent. of that employed on the first. This, of course, leaves out of account friction and the efficiency of the two machines. If we allow 10 per cent. loss for the former, and 25 for the latter, we have left but 65; and as 50 per cent. of this is lost by the production of counter-currents, we finally secure from the electro but between 30 and 35 per cent. of the power originally employed. Even if the loss be as great as this, the transmission would be not only possible but very profitable.

The question of the amount of power reclaimable was the subject of much discussion in connection with experiments made at the Electrical Exhibition in Munich, in the fall of 1882, by M. Marcel Deprez. He succeeded in transmitting power from the village of Weisbach, a distance of 57 kilometers, over an ordinary telegraph wire. The insulation was good, but differed in nothing from that usually employed on all telegraph lines. A heavy rain fell during almost the whole duration of the experiments. In the first of these there was immediately obtained at Munich a speed of 1,500 revolutions per second, the generating machine moving at the rate of 2,200. "The two machines being identical," said Deprez, "the proportion of the work recovered at Munich to the work expended at Munich was, setting aside passive resistance of every kind, $= \frac{1{,}500}{2{,}200}$, or more than 60 per cent." These figures were subsequently sharply criticized in the *Electrical Review*, by the electricians Hospitalier and Cabanellas, and Deprez was finally forced to admit that while the electrical result, as he termed it, varied from 50 to beyond 60 per cent., the industrial result varied only from 25 to more than 35, according as the generator turned at 1.600 or 2,000 revolutions. As perfect insulation was supposed, and other inacceptable hypotheses were assumed, the official report of 38.9 per cent. reclaimed can only be accepted under the most express reserve. The real amount of industrial work reclaimed was probably about 20 per cent. Taking into consideration the unfavorable circumstances, the fact that ordinary telegraph wire with ordinary insulation was used, and that the dynamos were not suitable for the experiment, it will be seen that the results do not disprove the conclusion that for practical purposes between 30 and 35 per cent. can be reclaimed, but rather go to substantiate it.

OBJECTIONS ON THE SCORE OF HEAT.

As Mr. Siemens has said, the principal objection that has been raised by electricians to the conveyance of power to the distance of miles is on account of the apparently rapid increase in the size of the conductor required with increase of distance. Many writers have given their views on this point, some maintaining, others opposing, the idea that a serious difficulty on this score is to be anticipated. In an able paper presented to the Franklin Institute some time ago by

Professors Thomson and Houston, it was conclusively shown that the thickness of the conductor required is of no particular moment. Their statements had particular reference to the size of cable necessary to convey the power of Niagara to New York City. One electrician had asserted that such a cable would require more copper than exists in the enormous deposits about Lake Superior. Another estimated the cost of the cable at $60 per lineal foot. But the Professors mentioned clearly proved that without increasing the size of the wire the same current can be sent to any distance, provided the number of dynamos be increased or diminished in proportion. "Stripped of its theoretical considerations," they concluded, "the important fact still remains, that with a cable of limited size an enormous quantity of power may be transmitted to considerable distances."

Sir William Thomson ascribes the credit of originating the idea of utilizing Niagara to Mr. C. W. Siemens, in March, 1877. In May, 1879, Thomson stated before a committee of the House of Commons that, taking Niagara as an example, under practically realizable conditions of intensity, a copper wire of half an inch in diameter would suffice to take 26,250 horse-power from water wheels driven by the falls, and (losing only 20 per cent. on the way) to yield 21,000 horse-power at a distance of 300 British statute miles; the prime cost of the copper amounting to £60,000, or less than £3 per horse-power, actually yielded at the distant station. In his inaugural address to the British Association in 1881, he gave a solution to the problem of what was to be done with the enormous electro-motive force at the New York end of the wires, which consisted in the use of large numbers of accumulators. All that is necessary to do, in order to subdivide this enormous force of 80,000 volts into what may be called small commercial electro-motive forces, is to keep a Faure battery of 40,000 cells always charged direct from the main current, and apply a methodical system of removing sets of 50 and placing them on town supply circuits, while other sets of 50 are being regularly introduced into the main circuit that is being charged. Of course, this removal does not mean bodily removal of the cells, but merely disconnecting the wires.

On looking over the views of different electricians, I can but come to the conclusion that the question of the size of conductors necessary for the transmission of power depends solely on the question of how much heat will be developed, and how it can be got rid of. Our doctrine of the conservation of energy teaches us that the heat developed is but another form of the wasted power. Now it seems to be agreed that when we allow the electricity to work a machine, as well as flow through the circuit, the power wasted is proportional to the square of the current or the velocity of the flow. Then, of course, the bigger the current the more the power wasted, and the more heat developed. But as Professors Thomson and Houston clearly demonstrated, the electromotive force can be indefinitely increased without altering the current, provided the resistance be likewise increased. The latter can be done without increasing the section of the conductor, by increasing its length. Therefore, by allowing only a very small current to pass through the wire, and by maintaining a very great electro-motive force, the amount of power transmitted can be made as large as we like, and the waste from the passage of the current as small as we like.

Then we conclude that however long and thin the wires may be, electricity may be brought from any distance, however great, to give out almost all its original energy to a machine, and that this requires a great difference of potentials; in other words, great electro-motive force, and a small current; and that, finally, the objections on the score of the size of the conductor, the heat generated, and the power wasted, three mutually dependent subjects, are invalid. In other words. it may be considered as indubitably proved that theoretically the transmission of power is feasible. It remains to be shown what has been practically accomplished in this direction.

PRACTICAL APPLICATIONS.

The most important use to which the principle of electric transmission of power is at present being put, is the running

of railway cars. It was only a little over five years ago that the first electric railway was built—an experimental one—in Germany, and to-day there are many in that country where their practical success has been first demonstrated. In France and England they are fast becoming popular, and it seems strange that those in America are yet all of an experimental nature. In this branch of electrical science at least, America is far from foremost.

The advantages of electricity over steam for railway purposes are many and great. In the first place the bulky locomotive is done away with, as the electro, or motor, can be placed either under the car or on trucks by itself, in either case great weight and room being saved. The machinery for converting the coal into the power, or rather extracting the power from the coal, is not portable, but stationary, and can be placed in the most convenient spot, even water-power thus being made available. For transmitting the power in many cases no difficulty has been experienced in using one rail as the positive and the other as the negative conductor. Sometimes it has been found that the dirt sticking to the rails and the felloes of the wheels formed a sort of crust so insulating as to prevent adequate communication. To remedy this, tubes have then been hung on poles beside the track with longitudinal slots on their under sides. In these run wheels connected with the electros, or motors, on the train. The tubes act as conductors, and the movements of the wheels in the tubes correspond to those of the cars, so that a good and constant communication is secured.

Far steeper grades can be surmounted on an electric than on a steam railway. Several carriages, each equipped with a motor, can be joined to one tram, which will give a distribution of motive power sufficient to overcome great inclines, or electro-magnets can be attached to the car directly over the rails, so that when they are thrown into connection with the current the car is pulled, as it were, toward the rails, and the adhesive power of the wheels is increased wonderfully. In this way the adhesive power of a 10-ton electric locomotive becomes greater than that of a 40-ton steam locomotive. Inclines of 2,000 feet to the mile have been surmounted with the electric locomotive.

As to speed, it is impossible to give the limit which can be reached on electric railways, because those so far constructed are on streets or in localities where very rapid transit is not possible or desirable. On the very first one built a rate of seven miles per hour was customary. On the Berlin railway, opened in 1881, the greatest speed reached was at the rate of 18 miles per hour. More was possible, but the police authorities refused to permit more than 9 miles per hour. Up to August, 1882, there had been no breakdown on this road, but Dr. Siemens acknowledged that in the winter the loss of power was more than quarter of the power supplied, a fact which indicates one of the barriers to complete success in electrical railroading. On the Siemens' railway, at the Paris Exhibition of September, 1881, a distance of over 1,600 feet was traversed in a minute, which is at the rate of nearly twenty miles per hour. There is every probability that electric locomotives can be run faster than any steam locomotive now in use.

By a happy coincidence, which belongs to the very nature of the electric motor, the static effect is maximum when the motor is in repose. This renders the starting very easy.

Economy is a most vital question. The electric locomotive can be built a great deal more cheaply than the steam locomotive. No coal has to be carried, and the coal used can be of a cheaper quality. Huge repair shops are wholly unnecessary. One man can run the electric locomotive; electric brakes are by far the strongest and most trustworthy. It is claimed that an electric car can be run on street railroads at one-fourth the cost of a horse car. This problem is wonderfully simplified by the utilization of some kind of secondary batteries, or accumulators, as they are called. By their help the electricity can be manufactured at any time and place convenient, and stored until it is required to be used. This, it is evident, will do away with the intricate and cumbrous paraphernalia of posts, wires, transversers, etc. Prof. W. E. Ayrtoun said, in March, 1882, that using only a single cell of Faure's accumulator, about 300 lbs. dead weight

contains all the energy and all the machinery necessary for over ten miles' run of a tram car with 46 passengers. In spite of the temporary character of the arrangement at the time in use at Leytonstone, the total weight of the Faure cells, dynamos and gearing combined, was only one and a-half ton, or one-third the weight of the detached steam or compressed air engine commonly used for tram cars. In a ton of the cells, as at present constructed, there is about 50 miles' run of a tram car containing 46 passengers. Electric storage of energy, moreover, makes us nearly independent of accidents to the engine or dynamo machines, or irregularities in their working, while the process results in no considerable loss of power.

Since March, 1882, an electrical railway has been in operation at Breuil-en-Auge, France, with great practical success. The Faure accumulators in the tender supply the electricity to the dynamo used as a motor. This railway is the property of a large bleaching establishment, and runs through the bleaching fields to pick up the linen. When the train stops, the dynamo is thrown into gear with a set of windlass rollers employed to wind up the linen. With it a single workman can do in 30 minutes what it used to take 11 hours to accomplish. A steam engine would never do in a bleaching field, and the application of electricity in such a case is not only novel, but suggestive. The public may soon insist that its lungs and ears have as much claim as the linen of the bleaching field, to be protected from the smoke and cinders of a steam engine.

One of the principal advantages of electrical railways, not suggested above, is that by properly arranging the connections, a train running into a section already occupied by another train will be brought to an immediate standstill and will remain at rest until the preceding train has passed out of that section. This will reduce the dangers of railway traveling, as far as collisions are concerned, to a minimum.

Although the application of electricity to the running of railway cars is almost the only direction in which the art of transmitting power by the electric current has made any practical advancement, in many other lines experiments have been made which show the vast possibilities of this art. A few instances may be interesting. Professors Thomson and Houston succeeded in transmitting considerable power through a wire only 0.004 inch in diameter. Important experiments upon ploughing by electricity were made in France early in 1879, which showed that for this purpose electricity can replace steam with advantage and economy. Since that time great progress has been made in pumping, pile-driving, punching, sawing, sewing, embroidering, weaving, printing, etc., by electricity. In short, enough has been done to show that methods, not possibilities, are now the things to be considered.

CONCLUSIONS AND SPECULATIONS.

What I have said will suffice to show that out of the science of dynamical electricity is fast developing an art. For a century scientists have been studying this most subtle force, and have been paving the way for the work—the useful, practical work which is to follow. The world has every reason to believe that artists will now step in and apply the principles that the scientists have discovered and the laws that they have formulated.

The possible applications of the principle of the electrical transmission of power are almost numberless. All the uses to which the energies of steam, water or compressed air are now turned, may be subserved as well by electricity, and this may be generated in places far more convenient than those now usually employed for the conversion into active energy of any of the forces latent in nature. We shall, I believe, at no distant date, have great central stations, possibly situated at the bottom of coal pits, where enormous steam engines will drive many electric machines. We shall have wires laid along every street, the electricity tapped into every house, and the quantity of electricity used in each house registered as gas is at present. The storage battery will fill a place corresponding to the gasometer in the gas system, making the current steady, rendering the consumer independent of the irregular action or stoppages of the dynamos of the central station, and enabling the use of dynamos of the highest tension, *i. e.*, those which produce the

currents of the greatest intensity. The electricity will be passed through little electric machines to drive machinery, to produce ventilation, to replace stoves, and to work all sorts of apparatus, as well as to give everybody an electric light. Solar heat will be used to run the dynamos in the cloudless regions. Everywhere the powers of the tides and such waterfalls as Niagara are to be utilized. Is not a millennium to be anticipated when the water power of a country shall be available at every door?

Electrical transmission has the unparalleled advantage of being superior to the obstacle presented by distance. Then again, it operates its miracles in perfect silence and repose. No force appears in the conductor such as appears in shafting, in pipes with compressed air or water, in endless chains or belts; and, in case of powerful currents, insulation is easy. The conductor is inert, and can be bent or shifted in any way whilst transmitting many horse-power, provided, of course, its continuity be not interrupted. It can be carried round the sharpest corners, through the most private rooms, into places where no other transmitter of power could possibly be taken. There is nothing to burst or to give way. In short such a method of transmission would be the acme of dynamical science.

In other ways electricity has long been serving mankind. The Ruhmkorff coil, the telegraph, the telephone, are but the most important of the mechanisms so far devised for its application. Now, it is proposed to utilize more fully nature's other forces by means of this strange and novel agency—to carry the power of the coal bed, the waterfall, the ocean, to localities where it can do work. The power which is daily wasted in a hundred ways—enormous, immeasurable as it is, will some day do its share toward the support and the advancement of the human race. Steam, which in the last century has conferred so many benefits on the world, will give way before electricity. The dynamo will replace the steam engine. This prediction seems wild and visionary, yet when steam was first thought of as an available force, its advocates were considered, just as the advocates of dynamical electricity to-day are considered—mere enthusiasts. But public opinion never stops the march of intellect. After it had proved the powers of steam to be enormous, genius never halted, but straightway went on anticipating still more wonderful discoveries in the realms of electricity.

The prophetic ken of science was happily exhibited by Dr. Lardner in his treatise on the Steam Engine. "Philosophy," said he, half a century ago, "already directs her fingers at sources of inexhaustible power in the phenomena of electricity and magnetism, and many causes combine to justify the expectation that we are on the eve of mechanical discoveries still greater than any which have yet appeared; and that the steam-engine itself, with the gigantic powers conferred upon it by the immortal Watt, will dwindle into insignificance in comparison with the hidden powers of nature still to be revealed, and that the day will come when that machine which is now extending the blessings of civilization to the most remote skirts of the globe will cease to have existence, except in the page of history."

To-day we are beginning to appreciate the truth of this prophecy. To-day we see dynamical electricity in the forefront of the physical sciences. The principle of the transmission of power by electricity fast approaches its realization. We are, in truth, just entering upon a wonderful age.

Faulty Construction of the Y Level.—When adjusting for parallelism of attached level to line of collimation great annoyance is caused by want of balance of the telescope with its attachments. In most instruments the eye end is very much lighter than the object end, and if the tripod be old and springy, and especially if the bubble be sensitive, it is impossible to get a good adjustment without temporarily weighting the eye end. A properly contracted telescope, when taken out of the Y's, should balance when supported by a cord passed between the bubble tube and the telescope at the point where the vertical axis cuts the barrel. This balance can easily be secured in every instrument by setting the collars at the proper relative distance from the ends of the telescope. It is not necessary that the eye piece and object glass should project equally beyond the Y's, and yet makers seem to think it essential. If they would sacrifice symmetry somewhat they would add greatly to the utility of their instruments.

If engineers will give this point due consideration, makers will be led to correct their present faulty construction.—R. G. Kimball, Polytechnic Institute, Brooklyn.

ACCOUNT OF THE SWISS PRECISION-LEVELING.

By R. GUISAN.

Translated from "Compte-rendu de la Société des Ingénieurs-civils," for Abstracts of Inst. of Civil Engineers.

As the Swiss were obliged to base all their levelings on bench-marks whose values were fixed by their neighbors, very contradictory results were introduced into their work. An international geodetical commission was accordingly held at Berlin in 1864 to frame rules for precision-levelings which should be common to all central Europe. At this it was resolved that side by side with the trigonometrical determination of heights, these levelings should be executed by the method of equal sights, the necessary checks being obtained by the polygonal combination of stations. The Swiss portion was commenced in July, 1865, under the directions of Messrs. Hirsch and Plantamour (the directors respectively of the Neufchatel and Geneva Observatories), and it will be finished this year (1884).

The instruments used were Ertel's levels on a modified form, having telescopes magnifying forty-two times, fitted with three horizontal stadia cross-hairs, the angle subtended by two threads, being about 3′ 30″. Each of the twenty divisions of the bubble scale was tested by the meridian-circle at Neufchatel, and corresponds to about three seconds of arc. The staves, which are three meters long, are made of very dry pine and graduated to centimeters, black and white alternately, the even numbers being written on one side, the odd on the other; these staves are provided with a box-level and plummet, and their feet are tipped with an iron cylindrical spur which fits in a hole pierced in an iron tripod. The staff holder grounds his staff firmly by a blow of its butt, after which it can be rotated without fear of displacement between the back and fore sight. On bad ground or in wind the staff is braced to keep it truly vertical.

Before commencing field work, as also at its close, the three instrumental constants are very carefully determined; they are:

1st. The angular value of the bubble scale-graduations.

2d. The angular reduction of the mean of the three threads.

3d. The angular distance of the two outer threads.

The method of doing this is detailed at length by the author, and he further shows clearly that by the system of leveling adopted curvature and refraction may be safely neglected.

The staff length is a most important factor in leveling, and this was most carefully and frequently tested. To find the equation for the two staves per meter, two benchmarks in bronze were let into the rocks in front of the Neufchatel Observatory at a difference in height of 2.90 meters. The instrument was set up exactly midway between them (at thirty meters), and their difference in level was read on each staff successively at varying heights of the instrument. The absolute staff length was tested by the standard at Berne, and this latter will be compared this winter (1883-1884) with that at Paris, when, on the completion of the leveling, final values will be given to all the readings taken. From the comparisons already made, the staff length is not affected by moisture, and only very slightly by temperature; its changes are not proportional to the interval elapsing between the comparisons, nor are they systematic, as they vary accidentally as much in one direction as in another.

The principle of completely separating the observations from the calculations was adopted. The original field books are copied, the originals being sent to Neufchatel, and the copies to Geneva, where the reductions are made in duplicate. To simplify the work and reduce the expense, the polygon system is adopted, and where this is impossible the line is leveled twice over. The work is repeated, in either case, if it does not close satisfactorily. Amongst the chief rules of procedure are the following:

1. The leveling to be executed by equal sights whenever possible; the difference between length of back and fore sights never to exceed ten meters.

2. The length of sight is as a rule to be limited as under:

(*a.*) Upon railroads with gradients under 1 in 100 to 100 meters.

(*b.*) Upon railroads with steeper gradients from 50 meters to 100 meters.

(*c.*) Upon highroads in the plains from 30 meters to 60 meters.

(*d.*) Upon mountain roads, from 10 meters to 25 meters.

3. The spirit-level to be always shaded from the sun.

4. The three instrumental errors, viz., collimation of optical axis, inequality of pivots, and bubble-error, to be determined at least once a day.

5. The field work to be carried on continuously except on wet or windy days; 3 kilometers at least should be the length of line leveled per day along railways, and 2 along roads in the plains.

6. Bench-marks to be made at every kilometer, and to be clearly described in the field book.

For adjustment of the errors made in the field Messrs. Hirsch and Plantamour have applied the method of least squares on the following principle: Two approximations are obtained, and both are based on the supposition that the closing error of a polygon equals the arithmetical and not the algebraical sum of the errors made in its different sides. This supposition, though not exact, is necessary for the solution of the conditional equation. Two values for the correction to be applied to a side are found from each polygon of which it forms a part; one by taking into account accidental errors only; the second by taking into account errors due to change of length in the staves. The probable mean of the correction for each side is then obtained, and, by comparing each individual value with this mean, allowing for the weights assigned, the mean error of each correction just found. The closing error having been thus reduced, the second approximation is applied, in which the mean error of the correction found by the first approximation is taken as the measure of the accuracy of the different sides. Fresh corrections, as well as the mean errors of such corrections, are again obtained, and with these new values the closing errors of the various polygons are reduced to a minimum. An example is given in illustration, as also a table, by which it appears that in six sides, together measuring 272 kilometers, the mean error per kilometer is below 1 millimeter.

In verifying work either by duplicated leveling or by closed polygons, a discrepancy of 0.0007 meter per kilometer in the two results is allowed on favorable ground, and of 0.0050 meter on unfavorable. In the Swiss work, the mean error per kilometer has consequently been—

1. In duplicated leveling—

(*a.*) On ten favorable lines measuring 416 kilometers 0.0014 meter.

(*b.*) On twenty unfavorable lines, measuring 830 kilometers, 0.0085 meter.

2. In nineteen closed polygons, whose sides measure 6,693 kilometers, 0.0030 meter.

Leveling lines the second time in the opposite direction has sensibly improved the closing of polygons.

During the progress of this work twenty benchmarks have been placed on the boundaries of countries conterminous with Switzerland, and it now only remains for an international congress to agree upon a common datum level, which should *a priori* be a point along the Mediterranean, to allow a comparison between the precision-levelings of all central Europe.

[*Note.*—In a note the author remarks that the settlement of the instrument makes the fore sight less than it should be, and proves from the leveling of thirteen lines with steep gradients that the difference in level is invariably greater in ascents than in descents. Mr. Hirsch, however, replies that in a work he is now engaged on this question is fully discussed, but that the effect of settlement —all other things being equal—depends rather on the length of the line leveled than on its difference in level.—TRANSL'R.]

M. Daubrée presented to the French Academy at its session of December 1st., the treatises of Mr. Cope Whitehouse on the caves of Staffa. The highest French authority on this subject has thus expressed approval of the cogency of the arguments by which the American *savant* has shown that the popular opinion of the formation of Fingal's Cave by the sea is untenable, and that it must be referred to the agency of man, and probably of that race which has left its traces in other great works in stone on the Irish and Scotch coasts.

OBITUARY.

JOHN B. JERVIS.—The venerable John B. Jervis, the Nestor of American engineers, has at last passed away, at the great age of 89, which he reached with fewer of the infirmities than usually attend so great an age. Mr. Jervis was already a prominent engineer when the first American railroad was built, and it was by his recommendation as Chief Engineer of the Delaware & Hudson Canal Company that his young assistant, the now venerable Horatio Allen, was sent to England before the construction and trial of the "Rocket," to order locomotives for the railroad which this company was building through the woods of the North-eastern Pennsylvania, to carry coal from the mines to its canal, the story of which was recently told in these columns by Mr. Allen. As one of the few men of reputation in the engineering profession in this country at the time we began to build railroads, Mr. Jervis had an important part to play, and he remained a leading railroad man until, already many years ago, advancing age led him to retire from such active duties.

Mr. Jervis began at the very bottom of his profession, in 1825, as axeman on the Erie Canal during its construction. Later he became Chief Engineer of the Delaware & Hudson Canal, and its canal and railroad were built under his direction.

In 1831 he was Chief Engineer of the Mohawk & Hudson Railroad, the line from Albany to Schenectady which now forms part of the New York Central & Hudson River Railroad, and the next year he designed for it a locomotive with a swiveling truck, which has since become almost universal on American railroads, and the use of which is now rapidly spreading in Europe. In 1836 he became Chief Engineer of the Croton Aqueduct, which is generally regarded as his great monument, and one of the finest engineering structures in the country. There were no precedents in this country for a structure of this kind, but the results attained were precisely what Mr. Jervis had estimated, and the cost differed from his estimates by only 1 per cent.

In 1849 Mr. Jervis became Chief Engineer of the Hudson River Railroad, and not only took charge of its construction, but argued in favor of its financial success, at a time when few thought it possible for a railroad to secure a paying traffic alongside a stream so favorable to navigation as the Hudson.

The Hudson River Railroad having been completed in 1851, Mr. Jervis became connected with Michigan Southern & Northern Indiana, the Chicago & Rock Island (of which he was President in 1854), and the Pittsburgh, Fort Wayne & Chicago, but it is already many years since he retired from active railroad management. Having built the first railroad in America on which a locomotive ran, he lived to see the country covered with a net-work of 125,000 miles, which in methods of construction and in the character of the rolling stock used bears the impress of his ideas and those of the other pioneers in railroad building.—*Railroad Gazette.*

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY CIVIL ENGINEERS.—At the meeting held Wednesday evening, January 7, Col. Wm. H. Paine presiding, the following persons were declared as elected to membership:

Members—Peter Lather Archibald, Chief Engineer, Maintenance of Way, Intercolonial Ry., Moncton, New Brunswick, Canada; Chas. Sumner Henning, Resident Engineer in charge of Maintenance of Way, Atlantic and Pacific R. R., Peach Springs, Arizona; Chas. Maples Jarvis, Vice-President and Chief Engineer, Berlin Iron Bridge Co., East Berlin, Conn.; Henry Ward Beecher Phinney, Resident Engineer, Harlem River Bridge, Suburban Rapid Transit Co., New York City.

Juniors—William Ferris Booth, C. E., Poughkeepsie, N. Y.; Edward Fladd, Ass't Engineer St. Louis Waterworks, St. Louis, Mo.; Sinclair Joseph Johnson, Ass't Engineer Harlem River Bridge, Suburban Rapid Transit Co., New York City.

The following amendment to Section 19 of the by-laws of the Society was adopted by a general vote:

Section 19.—A nomination or proposal shall be presented at the next regular meeting of the Board of Direction following its receipt; *the Board of Direction shall thereupon send to all members of the Society a notice that such person is a candidate for election. Not less than thirty days thereafter the Board shall consider the application, and, if approved,* and the applicant (if for admission as member, associate or junior) classed with his consent, a day shall be fixed for the ballot to be canvassed, which shall be at a regular meeting of the Society, not less than twenty-five days thereafter.

The amended portion is in italics.

The paper of the evening was by Prof. Robert H. Thurston, on "The Real Value of Lubricants, and the Correct Method of Comparing Prices"; this paper was read by the Secretary, and discussed.

Mr. M. Eisller, C. E., followed with an interesting description of the geology of the Isthmus of Panama, as found along the line of the canal. Mr. Eisller referred especially to the disintegrated character of the trachite rock as found at the depth of 33 meters below the surface at the Culebra, and gave his theory for this formation.

Mr. H. C. Y. Moller, C. E., Ass't City Engineer of Copenhagen, Denmark, exhibited maps of the harbor of Copenhagen, and described the harbor works.

ENGINEERS' CLUB OF PHILADELPHIA—*Record of Regular Meeting, Dec. 20th,* 1884.—Past-President Frederic Graff in the chair.

Mr. Geo. S. Strong read a portion of the first of a series of papers upon the "Future of Locomotive Building. It contained a review of the present and past fast train service in Europe and America, with a discussion as to the possibilities of the future, under the following headings: Fast Express Trains in England; Fast Express Trains on the Continent; Fast Express Trains in America. In all of these the journey speed and running average is given. Are Still

Higher Speeds Wanted? Recent Multiplication of Freight Traffic on American Railways; Train Resistance; Resistance of Grades; Resistance of Curves; Atmospheric Resistance; Have we Reached the Maximum Speed in our Locomotives? The Boiler Power Difficulty; Possible Ways out of the Boiler Difficulty; Economy of Fuel; English *vs.* American Locomotives; Comparative Cost of Slow and Fast Traveling. This and the following papers are intended to be a complete analysis of the problem of rapid train service, both passenger and freight, and of the locomotives now employed in such service; and a consideration of the form and characteristics of the engines which must meet prospective demands. Methods of construction, shop practice, and the necessity for more uniformity in standards, are among the important topics.

Gen. Russell Thayer read a paper upon the "Navigation of the Air, with a Description of the Aerial Ship and the Practical operation of its Motor."

Mr. Lloyd Bankson presented, for Mr. H. W. Spangler, an illustrated paper upon "Measuring Chimney Draft." The ordinary method of measuring is by means of a U tube, partly filled with water. The difference in level in the legs of the U is a measure of the amount of chimney draft. When this is small it is desirable to magnify the reading, and several devices were described for the purpose. One, devised by Mr. Barrus, consisted of two vessels connected by a U tube, the vessels being filled with liquids of about the same specific gravity, but of different colors. The line between the liquids which could be seen in the U tube moved with the amount of draft much more than it would have done in a simple U.

Another device consisted of two vessels of different sizes, each covered with a diaphragm of rubber. These diaphragms are connected together. The inner side of one vessel is connected to the chimney, while the inner part of the other connects with a glass tube, and is filled with water. The water rises and falls in the tube with pressure in the chimney.

The Secretary presented for Mr. P. A. Taylor, an account of the enlargement of the East Mahanoy Tunnel, East Mahanoy Railroad, Schuylkill County, Pa. This tunnel is 3,411 feet long, and was completed during the year 1862.

In the spring of 1876 it was decided by the Philadelphia & Reading Railroad Company to enlarge it, on account of its being too low to permit the passage of engines having the standard height of smoke stack adopted by the company.

To do the work it was first thought that a sufficient amount of the roof could be taken off, but on account of slips and falling rocks, caused by the fact that when the tunnel was driven, several of the coal measures were cut through; it was particularly dangerous, and necessitated timbering in some places; the risks also of detaining trains by having the necessary false works in the tunnel to reach the roof were great, and the coal trade being particularly brisk at that time, the idea was abandoned, and it was decided to take out the bottom, a portion of one side, and a part of the bench that had been left in originally by the East Mahanoy Railroad Company.

On May 29th, 1876, the work was started, and finished September 9th of the same year, costing about $41,000,

The length taken out was 3,411 feet, average depth 2 feet, and width 12 feet, all of which was through conglomerate rock, known only in the anthracite region. In addition to taking out the bottom, various parts of the roof had to be taken down, as well as on the sides of the tunnel, as noted above.

A force of about 225 men was employed in the work, one-half working during the day drilling holes for the blasts and getting everything ready for the explosion of the shots, which generally took place about ten o'clock at night, after the fast Centennial Express passed through. The night force was employed in blasting. No. 2 dynamite was used, and 100 shots put off at one time, by electricity. The debris was then cleared away, and the track blocked up to allow trains to pass through. From ten o'clock until two o'clock during the night was the only time that trains entirely ceased to run through the tunnel.

They labored under great disadvantagefor the want of proper ventilation. The shafts which had been sunk during the construction of the tunnel had been filled up for some years, and the smoke caused by the numerous trains passing through, as well as that produced by the blasts, had to seek an outlet at the east and west ends of the tunnel.

During the whole progress of the work but one train was delayed, and that only five minutes, and the only accident that occurred was the killing of one laborer, caused by his falling under the small engine which was used for hauling away the small trucks loaded with debris from the tunnel.

In the grades of the original road-bed of the tunnel there was a slight adverse grade from the west end to a point about midway, causing the water percolating through the seams of the coal measures, to drain partly into the Susquehanna and partly into the Delaware. Now there is a continuous grade with the trade, relieving a heavy pull of the loaded coal trains after entering the tunnel.

Mr. John C. Trautwine, Jr., presented an illustrated description of a design for a 100-foot turntable, by Mr. C. O. H. Fritzsche, of New York, for the N. Y., W. S. & B. R. R. Co., for use in their car shop. It was to carry a six-wheeled shifting engine, 30 feet long, weighing 90,000 lbs., and two cars, each 36 feet long, and weighing 24,000 lbs. each; total extraneous load, 138,000 lbs.

The table is turned by a steam engine, which, with its boiler, is carried upon an iron platform about 7½ × 13 feet. attached to one side of the turntable near the middle of its length, and thus revolving with it. The power of the engine is communicated by means of bevel gearing and a long shaft running along the center-line of the turntable to a wheel in each end carriage.

PROCEEDINGS OF THE ENGINEERS' CLUB OF ST. LOUIS—ST. LOUIS, Jan. 7, 1885.—The

Club was called to order at 8 o'clock by President Moore, thirty-five members and six visitors being present.

The minutes of the last meeting were read and approved. The report of the executive committee was read and approved.

The President appointed the following committee on Smoke Prevention: Prof. William B. Potter, chairman; Messrs. W. H. Allderdice, Theodore Allen, H. Constable, F. H. Pond and C. E. Jones.

Mr. J. B. Johnson stated that the St. Paul Engineers' Club had been admitted to the Association of Engineering Societies, and that the Engineers' Society of Western Pennsylvania, of Pittsburg, were considering the question of joining the Association.

On motion the Secretary was instructed to communicate with the members of the club and ascertain the number desiring membership in the Mercantile Library for the ensuing year.

Mr. John A. Sobolewski read a paper on "Economy in Gas Engines." In the general discussion which followed, he said that the economy consisted in the absence of skilled labor, the cleanliness of the machines, the ease with which they are started, and the absence of risk from fire; also, that the gas-bills were ninety-nine per cent of the cost of operation.

Mr. C. T. Aubin read a paper on "Protection against Fire, and Means of Extinguishing the Same."

On motion, the privilege of the floor was extended to Mr. H. Clay Sexton, Chief of Fire Department. He thought many of our large buildings were built for nothing but to burn down, that the extent of some of our fires was not the fault of the firemen, but the fault of the men who construct the buildings, and the carelessness of those who occupy them.

General discussions followed.

Moved that a committee of five be appointed to investigate the matter of indicating the location of fire-alarm boxes by colored glass in the street lamps, or otherwise, and report at the next meeting.

It was thought that the question was not in the province of the Club, and the motion was lost.

ENGINEERING NOTES.

Our Birmingham correspondent says, a very creditable piece of engineering work has just been accomplished at the Highfield Engineering Works, Bilston, by Messrs. Thomas Perry and Son. It is the casting and polishing to a high degree of perfection a pair of chilled iron rolls, each weighing about 9 tons, for use in a lead works in France. The contract is a repeat one, and was placed in England, since neither in France nor in Belgium could chilled rolls sufficiently hard be obtained, whereas Messrs. Perry claim that the rolls now supplied are harder than steel rolls. They will work one above the other, and are each 24⅝ in. diameter, by 10 ft. long on the working part, but there is something like an additional 2 ft. on the bottom roll for the fixing on of the wheels. The rolls present almost a mirror surface, and the obtaining of such a face on long and narrow rolls of this description has necessitated much care in the lathe.

IRON AND STEEL NOTES.

Cast Iron Cutlery.—This title may appear anomalous, but cast iron cutlery of certain forms is far more common than its purchasers generally imagine. And it is not necessarily of a poor quality, although made of nothing but cast iron. In the writer's family is a pair of scissors of cast iron that has been used for three years, and has been several times sharpened. The writer has shaved with a cast iron razor, which did excellent work for months. There are in Connecticut two quite extensive establishments which reckon cast iron cutlery as among the important products of their work. This allusion to cast iron shears and scissors does not refer to the combined cast iron and steel articles which are usually considered superior to the forged ones. These have a steel inner plate cemented on each blade by the fused iron when it is poured into the mould; but the cast iron shears and scissors are wholly and entirely of cast iron, and they are finished for the market precisely as they come from the moulds. The quality of the iron used is the same or similar to that used in casting for malleable iron, and for cutlery it is cast in chills. When broken, the crystallization is very similar to that of hardened cast steel, and, except for lack of elasticity, it serves the same general purpose. But although this cast iron is not adapted to tools which work by blows, it is sometimes made into ice picks and axes, hatchets and steak choppers. The manufacturers of cast iron shears and scissors make no secret of the material, and sell their goods for just what they are. Of course they are sold cheaper than forged work of steel can be sold. Retailers, also, know that this cheap cutlery is not steel, and usually—unless dishonest—they will truthfully answer questions on the subject. But, really, a pair of cast iron shears or scissors for ordinary household work is just as good as one of forged cast steel. There is only one difficulty in the way of superseding cast steel forgings by cast iron castings in these implements, which is that the chill that makes the iron hard does not always extend to a depth that will allow of repeated grindings and resharpenings, the material crumbling before it can be brought to an edge. But when first ground and edged, the shears are as keen as those of tempered cast steel, and the blades retain their edges longer.

Remarks on some New Observations on the Working of Steel—By D. Chernoff.—The author, whose name is already well known in connection with the working of steel, read a paper before the Imperial Technical Society of St. Petersburgh on the phenomenon observed by Mr. Beck-Gerhard (*ante*). He stated that seven or eight years ago he had noticed, in cold-sheared samples of steel subjected to a destructive tensile strain, that when the limits of elasticity were reached, the scale on the specimens began to separate in a peculiar manner, and the surfaces of the samples were

marked with curved lines of more or less regularity. He made arrangements to institute an exhaustive inquiry into the meaning of these lines, but unfortunately circumstances obliged him to abandon his intention until the commencement of the present year, when Mr. Beck-Gerhard invited him to examine his specimens, and awakened once more his desire to investigate the subject. The appearance of the rays on the polished surfaces of the steel reminded Mr. Chernoff of the experiments made by Mr. Leger (Sur la constitution des corps trempes. Mémoires de la Société des Ingénieurs Civils, 1877, p. 645), in which the lines of strains produced in glass subjected to various pressures were made visible, through the circumstance of the light passing through the specimens becoming polarized in the regions strained, and therefore capable of being made manifest by being examined through a Nicol prism. Similar observations were recorded in the year 1877 (Société des Sciences Industrielles de Lyon. Séance du 25 Juillet, 1877), and it occurred to Mr. Chernoff that, as in glass bearers the lines of strains assumed various curved forms, so, in metal, similar actions took place, and became, in part, manifested on the surface so soon as the limits of elasticity were passed. In glass the elastic limit coincides with that of ultimate strength, so that it is impossible to fix the waves of strain, but it is worthy of remark that the forms assumed by the fragments of conchoidal fractures resemble closely the lines of strain revealed in glass by the Nicol prism. In metals, on the other hand, the permanent set will begin first where the strains are greatest, and hence the deformation of surface will follow the lines of maximum strain.

Why the regions of maximum strain should arrange themselves in the form of curved rays in metals is as difficult of explanation as the corresponding phenomena in glass; but Mr. Leger suggests that the propagation of strains through elastic substances may be of an undulatory nature comparable to the propagation of sound.

Mr. Rgeshotarsky, one of the officers employed at the Abouchoff Steel Works, undertook to repeat and extend Mr. Beck-Gerhard's experiments, and from his observations it was found that the lines of strain were manifested not only in punching, but in shearing, and in flattening under the steam-hammer. From these experiments the analogy between the visible lines of strain in steel and in glass and sound waves was rendered very striking, because the lines of strain from shearing and punching in the same specimen, and from the two points of compression under the hammer seemed to pass each other without mutual hindrance, except in so far that at the points of intersection the phenomenon of interference was clearly observable. In order to confirm the supposition that shearing produced the same effects as punching, Mr. Rgeshotarsky polished the surface of a $\frac{1}{2}$-inch plate, which had been sheared round its edges, and subjected it to tension beyond its elastic limits. The curved rays emanating from the two sheared edges appeared very distinctly intersecting each other, and were sufficiently prominent to affect the sense of touch. It was noticed by Mr. Beck-Gerhard that the rays are of two kinds. One set, produced by tension, are depressed below the surface, the other due to compression, are raised. What kind of rays should be expected from a specimen which had been sheared, then polished and finally stretched?

Hodgkinson long since noticed that when iron bars, stretched beyond their elastic limits, were again subjected to tension, that the elastic limit was considerably raised; and the same fact may be deduced from the well-known circumstances that drawn iron wire becomes harder and more rigid as it passes through the dies. In the same way, therefore, in the sunk rays produced by the tension due to a shearing force, the metal has been stretched beyond its elastic limit, and consequently that limit has been raised. After obliteration by polishing, when direct tension is applied, the metal in the rays is last to yield, that between them is extended, and forms hollows, leaving the rays prominent. Such is the observed effect, although at first sight it appears paradoxical. Mr. Chernoff acknowledges his inability to discuss the question of the curvature of the lines of maximum strain, but he draws attention to the similarity between the curved lines of fracture in a broken pane of glass and the rays developed by punching and shearing steel. He remarks that the slower the action of the force that breaks the glass the more extensive is the injury, while, under rapid impact, such as from a bullet, hardly any radiating fractures are formed, and he infers that similar results would follow in the case of metal plates, acted on with different degrees of rapidity.

It would be a very useful undertaking, with reference to the strength of guns, and especially of ordnance of large caliber, to investigate the effects produced on the inner tubes by the pressure due to the successive tiers of hoops In spite of very elaborate formulas for calculating the compression produced on the inner tubes by the successive layers of rings, no information exists as to how the strains are distributed in the metal of the tube, because all observation leads to the conclusion that the strains are not transmitted in the regular way assumed by the formulas, but in a wave-like progression forming bands of irregular tension and consequent plains of weakness. Did not the existence of such plains of weakness explain the anomalous behavior of inner tubes, which, though made of excellent metal, carefully tested at every stage of its manufacture, yet crack in an unaccountable manner after more or less prolonged firing?

This paper, as well as the foregoing, is illustrated by numerous plates, for the most part engraved from photographs taken from the specimens.—*Abstracts of the Inst. of Civil Engineers.*

RAILWAY NOTES.

THE SPEED OF ENGLISH EXPRESS TRAINS.—A Paper on English express trains, was lately read by Lieutenant Willock, R. E., before the Statistical Society. A table of the great increase of express services throughout the United Kingdom, forming a portion of the paper, is of more than usual interest at the present time, with the recent accident at Penistone

still fresh in our minds. This may be our excuse for referring more fully to a subject which has a special bearing on the safety of railway traveling. In comparing the express services of 1871 and 1883, we find that the increase of express trains during that period has been 157, or 62.8 throughout the English and Scotch lines, the numbers being 250 per day in 1871, and 407 in 1883. The average journey speed has increased from 37¾ to 41¾ miles per hour, the running average from 40$\frac{4}{10}$ to 44¼ miles, and the total express mileage from 23,672 to 42,693, a daily increase of 19.021 miles, or 80 per cent. The London and North-Western stands at the head of the list as regards express mileage, with 10,405 daily miles, but it is not in the same position as regards running average, all the great companies, indeed, having increased in this respect by more than the average amount, with this one exception. The Great Northern stands first in the running average increase, being 42 miles per hour in 1871 and 46¾ in 1883, being an increase of 4¾ miles per hour. The total express mileage on this system has risen from 3,520 to 6,780, or 92 per cent. The Great Northern, however, shows the greatest number of express journeys on each mile, though in the matter of long runs it of course cannot compete with the London and North-Western, for it is comparatively a short line, and has no long runs extending like those from Chester to Holyhead or Preston to Carlisle. The Manchester, Sheffield and Lincolnshire shows the largest increase of all the lines in the number of district expresses, having risen from 11 in 1871 to 49 in 1883—an increase of 38. As to its average journey speed also, that has mounted from 36 miles to 43—an increase of seven per hour; and in this matter it is surpassed by only one system —viz., the Glasgow and South-Western, which increased by 7½ miles. The running average of the Manchester and Sheffield has of course increased from 38¼ miles to 44¾, or 6 per cent., and its total express mileage from 594 to 2,318, or the enormous number of 1,724, or 290 per cent. The Midland company ranks third in the number of its expresses, of which there are 66, but second as regards express mileage, being 8,175, in 1871 and 8,860 in 1881—an increase of 6,685 miles, or 147 per cent. Its average journey speed is now 41¾ miles—an increase of 4$\frac{1}{10}$ per hour since 1871; and its running average is 45 miles—an increase of 4$\frac{1}{10}$. The Midland system shows a very large augmentation in the number of its daily long runs, these having been 20 in 1871, with a mileage of 1,185, while now there are 84, with a mileage of 4,377. With respect to the total express mileage, the Great Eastern has made more rapid progress than any other line, having jumped from the bottom in 1871, when it was 161 miles, to the fourth place in 1883, with 3,040 miles—an increase per cent. of 1,788. This is owing largely to the extension of the sytem to Doncaster. The number of district expresses has risen from 3 to 34, its average journey speed from 37$\frac{9}{10}$ to 41, and its running average from 38$\frac{8}{10}$ to 43¼. As representing the West of England, the Great Western, though it still stands fifth in the order of total express mileage, has actually reduced its number of district expresses from 28 to 18, and therefore, of course, its total express mileage, which now stands at 2,600 daily miles. Its average journey speed has risen from 38 to 42 miles, and its running average from 41¾ to 46¼. For the southern lines the changes are nothing like so great. The Chatham and Dover has increased its district expresses from 6 to 9, the Brighton from 12 to 18, while the South-Eastern has reduced them from 15 to 12, and the South-Western from 7 to 8. In speed, however, the latter company shows best of all the lines south of London, having risen from 40 miles to 44¼, the Chatham and Dover following suit from 41¾ to 43⅛, the South-Eastern from 40⅜ to 41¾, and the Brighton from 41⅜ to only 41$\frac{3}{32}$. This very small increase is doubtless due to the crowded state of the line between London and Creydon, which would render a very high speed inadmissible. It ought to be added that, notwithstanding the increase of speed, accidents have become less rare, owing to the greater care employed and the more general adoption of efficient brake-power by the more enlightened railway companies.—*Iron.*

ORDNANCE AND NAVAL.

GUNS FOR THE NAVY.—During the discussion on Sir. E. J. Reed's paper at the United Service Institution, the need of guns for our armaments was repeatedly referred to. We learn from Lord Northbrook and Sir Thomas Brassey that a separate vote of money for this purpose for our new ships amounts to £1,600,-000. As we have in our war ships now in commission nothing but old type guns, it is very desirable that the supply of guns of new type should not be limited to new ships only. Most of our readers are aware that new guns differ from our old guns mainly in three respects: (1) They are breech-loaders; (2) they are constructed wholly of steel; (3) they are so proportioned as to discharge a projectile with a deal more energy or stored-up work in it than the old guns, which is effected by their length and dimensions of powder chamber admitting of a much larger charge of slow burning powder being burnt, the pressure being better kept up through the bore. The shot thus discharged has greater power both for the perforation and for the smashing up of armor, but the latter not by any means in the same proportion as the former; for the diameter of the projectile being smaller than formerly, as compared with its weight, it experiences diminished resistance in its passage through soft armor, whereas against really hard armor where a hole is only made by breaking up the plates, the effect is simply proportional to the stored-up work, a fact that must be borne in mind when considering the powers of guns registered, as they generally are, solely with a view to their perforation.

With regard to the supply, we fear that for the heaviest pieces for which it is difficult to get steel we may wait some little time. On the other hand, the chief gun in the constitution of our secondary armaments is the 6in. gun—Mark IV.—firing a charge of 50 lbs. of powder. Guns of this caliber can be rapidly turned out, though perhaps it would be rash to speculate at what rate exactly. The largest guns re-

quired are of 110 tons' weight, for the Benbow. These are ordered from Elswick. In *The Engineer*, June 27th last, page 488, writing on Colonel Maitland's paper, we gave a figure of this gun in section. At present it is the most powerful gun designed. Other vessels of somewhat the Benbow type will carry the 63-ton gun made in the Royal Gun Factories. This has greater calculated muzzle energy than any gun made out of England at present, except Krupp's 119-ton gun, viz., 36,415 foot-tons, with a perforation of 28.6in, of iron; the Krupp gun having 46,061 foot-tons energy and 29.2in. perforation. The Elswick 110-ton gun has 50,924 foot-tons calculated energy and 30.5 in. perforation. The Gun Factory gun and Krupp's piece are also shown in section in *The Engineer* of June 27th last. These guns are, of course, enormously powerful. The French 71-ton gun has 31,272 foot-tons energy and 24.5in. perforation. These guns are likely to appear only as single champions, firing experimentally, for some little time yet. The nation that first has any considerable supply of them will be the nation that has the best steel makers, for on this all depends. Practically, however. the supply of 6in. guns is equally important. Our turret-ships specially come short in secondary armaments of guns, and some foreign designs almost appear to have been framed so as to take advantage of our paucity of pieces by exposing their men *en barbette* and the like, trusting to the fact that no one would point an 80-ton gun at a single man, or two or three together in a gun detachment. It is necessary to arrange in what way these 6in. guns can be introduced into our mastless turret type, and in the meantime the gun factories, and private firms too, ought to be kept pretty busy. To return to the heavy guns. Our readers can best judge of their practical power, perhaps, by the fact that while 19in. is the thickness of plate adopted generally for very heavy armor, the perforations of these guns which we have given above so greatly exceeds it, that there is now no ship afloat that can resist such pieces. L'Amiral Duperré, quoted by Sir E. J. Reed, has less than 22in. of steel maximum armor. A fair blow from any of the above heavy guns—that is, the Krupp, Elswick, Gun Factory, or French gun —would smash up such armor easily. L'Amiral Baudin and Inflexible would come little or nothing better off, and no vessels carry thicker armor. We mention this because it is generally felt that guns have somehow lagged. In numbers there may be some truth in this, but it is due to the rapid development of power which made our designers pause before pushing on large supplies. Now we trust that the matter has taken a sufficiently settled form to enable production to be pushed on.

We may add that our authorities having at length been made acquainted with the secrets of cocoa powder, samples have been delivered made at Waltham Abbey, which have given excellent results. We trust that steps may be taken to enable the work of making guns to be at last pushed forward with due regard to the present position of the Navy.—*Engineer.*

BOOK NOTICES

Publications Received.

Transactions of the American Institute of Mining Engineers, Vol. XII.

Report No. 2 on a Water Supply for New York and other Cities of the Hudson Valley. By J. T. Fanning, C. E.

Sound Signals. By Arnold B. Johnson.

Annual Report of the Engineer Department of the District of Columbia. Maj. Garrett J. Lydecker, U. S. Engineers.

Danger Lines and River Floods of 1882 (Signal Service Notes No. XV.). By H. A. Hazen.

Manuel de Manipulations Chemiques. Par Fr. De Walque. Lauvain: Preters Ruelins.

Hydraulique Fluviale. Par M. C. Lechalas. Paris: Baudry.

Traite Generale des Vins. Par Emile Viard. Paris: F. Lavy.

Theorie de l'Elasticite des Corps Solide. Par M. de Saint Venant. Paris: Dunod.

The Stability of Ships. By Sir Edward J. Reed, K. C. B., F. R. S. London: Chas. Griffin & Co. Price $7.50.

The scope of this book is fully explained in the title and name of the author.

It is hardly necessary to say more than that it is a scientific treatise of 366 pages, illustrated by 225 diagrams and five folding tables.

The whole work is divided into nineteen chapters. These present, in succession, the various methods that have been employed of determining the metacenter, and the conditions of stability.

Cassell's Family Magazine. The Magazine of Art. New York: Cassell & Co.

The Magazine of Art for February is an elegant number. Illustrations and letter-press are both excellent.

Text-Book of Descriptive Mineralogy. By Hilary Bauerman, F.G.S. London: Longmans, Green & Co. 1884.

This is the companion volume to the "Systematic Mineralogy," by the same author, published in 1881. As far as space admits Mr. Bauerman endeavors to describe all the more important mineral species. His remarks about the names of minerals and their derivations are well chosen; and both mining students and teachers of mineralogy should note the following paragraph:—"In the case of minerals worked as metallic ores, the ordinary commercial names should always be used where possible. Thus for all purposes copper pyrites, tinstone, and zinc blende are preferable to chalcopyrite, cassiterite, and sphalerite."

The classification adopted by the author "is in the main similar to that of Rammelsberg's 'Mineral-Chemie.'" Each description gives the form, the structure, the composition, and chemical characteristics, and concludes with the occurrence and distribution of the mineral. The crystallographic form is indicated both by Miller's notation and that of Naumann; and the figures of crystals are from the excellent wood blocks used originally for Brook and Miller's "Mineralogy."

As might be expected from the author's wide experience as a traveler, the parts relating to occurrence are generally quite as complete as is compatible with the size of the volume; but strange to say, under the head of copper pyrites, the author omits all mention of the great mines of the provinces of Huelva in Spain, and Alemtejo in Portugal. It is true that they are not forgotten by him when speaking of iron pyrites; but Rio Tinto, which produces more copper than any other mine in the world, surely deserves notice quite as much as Devon Great Consols, Mellanear, or South Caradon. We must here correct an error of the author, who places Buitron in Portugal, whereas it is in Spain; and the great Portuguese mine is at San Domingos, not at Pomaron, which is simply the port of shipment, about eleven miles from the actual workings. . . . . .

We regret that there are occasional errors of spelling in the names of minerals and places. Thus "Freieslebenite" appears several times without the second "e," though it stands correct in the index, and "Meconite" might puzzle the novice who had never heard of Meionite. However these are slight blemishes, and both they and the few other mistakes can easily be corrected in a second edition, which no doubt will be required, as Mr. Bauerman's manual is clear, compact, and handy, and is likely to be a favorite with students of mineralogy.—*Nature.*

MAGNETO-ELECTRIC AND DYNAMO-ELECTRIC MACHINES: THEIR CONSTRUCTION AND PRACTICAL APPLICATION TO ELECTRIC LIGHTING AND THE TRANSMISSION OF POWER. By DR. H. SCHELLEN, Director of the Real Gymnasium at Cologne, etc. Translated from the third German edition by Nathaniel S. Keith and Percy Neymann, Ph. D., with very large additions and notes relating to American machines, by NATHANIEL S. KEITH, Secretary of the American Institute of Electrical Engineers. Vol. I., with 353 illustrations. New York: D. Van Nostrand, 1884.

The extraordinary progress which has been made within the past few years in the industrial application of the dynamo-electric machine, especially in electro-metallurgy, electric illumination, and last, but by no means least in importance, the transmission of mechanical energy, has had the inevitable result of stimulating the production of an enormous volume of literature, relating to the subject, of more or less value—generally less. A succession of would-be authors, each anxious to be first in the field, have loaded the shelves of the electrician's library with books consisting mainly of a crude and ill-digested mass of extracts from the reports of exhibitions and the catalogues and advertising circulars of manufacturers, abounding in erroneous descriptions and still more erroneous theories. In these works we find repeated, again and again, the venerable blunders with which constant reiteration has rendered us so familiar, accompanied by equally venerable engravings of machines of which some never had an existence and others have sunk into deserved oblivion.

It is encouraging, however, to notice that a marked change in the character of electrical literature is becoming perceptible. Several works have recently been published of real and permanent value, not only to the theoretical electrician, but to the electrical engineer and the practical workman as well.

Dr. Schellen, the author of the present work, whose lamented decease was chronicled but a few days since, was director of a technical institution at Cologne, Germany, and is best known among electricians as the author of one of the earliest as well as the best treatises on the electric telegraph, which has gone through a great number of editions and still retains its place as one of the leading works on that subject. It would, perhaps, be difficult to name any one person better qualified to prepare a work of this particular kind than was Dr. Schellen. It is true that the character of the book shows him to be a theoretical rather than a practical man, for his lack of thorough familiarity with the conditions of actual work occasionally betrays him into obvious errors. In the present volume, which has been translated from the German by Nathaniel S. Keith and Percy Neymann, the original work has been carefully edited by Mr. Keith and many such errors corrected. The editor has also added a considerable amount of new and valuable matter descriptive of American machines and practice, which greatly increases the value of the work to American readers. The number of dynamo and magneto machines which have been devised by the ingenuity of inventors is so great as to render it hopeless to attempt to describe them all in a single work. In making a selection the author states that the considerations which have guided him have been either novelty in principal, extensive technical application, or a great degree of historical or theoretical interest. Part I. is devoted to a preliminary explanation of the physics of electricity and magnetism. This portion of the work is exceedingly well written, and contains matter of considerable value to the student. We do not remember to have seen any instance in which the reactions of the solenoid and movable core, so much used as a regulating device for arc lamps, is more lucidly explained and illustrated. The engravings in *fac-simile* of magnetic spectrum, showing the lines of magnetic force under different conditions, will well repay careful study and comparison. Part II. treats of methods of electric measurement and of measuring instruments. The portion relating to the dynamometer, and the methods of determining the power imparted to the dynamo is unusually full. The American editor has added descriptions of the Kent and Brackett dynamometers, the latter of which is unquestionably the most convenient and accurate apparatus yet devised for this purpose. The photometer and the manner of its use are also well described. The portion of the chapter relating to electrical measurement proper is, however, hardly what we have a right to expect in a work of this character, no reference being made to some of the most important instruments for measuring large currents, such, for example, as the graded galvanometers of Sir W. Thomson.

Parts III., IV. and V. are devoted to magneto

and dynamo electrical machines. In this work, for the first time, so far as we are aware, the evidence in favor of each of the several claimants to the discovery or invention of the self-exciting dynamo has been brought together in a form convenient for discussion and comparison. It is very well known that the discovery of this principle was announced independently about the same time by Dr. Siemens and Prof. Wheatstone, but it is not so well known that Moses G. Farmer, the well-known American electrician, was also an independent discoverer of the same principle. It appears that Siemens exhibited his machine, illustrating the principle in question in December, 1866, and prepared a paper which came before the meeting of the Berlin Academy of Sciences in the middle of January, 1867. The original paper translated from Poggendorff's *Annalen* is given in full. On February 14, 1867, Professor Wheatstone read a paper before the Royal Society on the same subject. The evidence in support of Professor Farmer's claim has never before, so far as we are aware, been published in full.

As a further contribution to the history of the dynamo, Mr. Keith has added a translation of the article of Dr. Paccinotti, published in 1864 in *Il Nuovo Cimento*, containing a description of his continuous current electro-magnetic machine.

The descriptions of the modern machines have been prepared with care, and are sufficiently well illustrated. We find, for the first time, descriptions of some of the modern American machines, which, although in extensive use, have not heretofore found their way into the books, such as the Thomson-Houston, the Fuller-Gramme, and the Weston constant potential machines.

The final chapter in the book is devoted to a classification of dynamo-electric machines, and a discussion of the theory involved in their construction and operation, which is mainly a compilation of the results of the investigations of Dr. O. Frolich and Marcel Deprez.

We must call attention to one serious fault in the translation or editing of this work, which, with a little care, might easily have been remedied. We refer to an indiscriminate use of the terms electromotive force, tension and intensity, which is eminently well calculated to confuse the ordinary reader. Take, for example, the statement on page 344, that the Weston armature is calculated to produce currents of low *tension*, but of great *intensity*. Here the the word "tension" is obviously used in the place of "potential" or "electromotive force," and "intensity" in place of "quantity." We might point out a number of other instances of the same kind. The weights and dimensions throughout the book are sometimes given in English measure and sometimes in metric measure, which is extremely inconvenient for the student. A better method, which we are glad to see is rapidly coming into use among scientific writers, is to give the weights and dimension in metric measures, followed by the English equivalent in brackets. This is especially proper in electrical work, inasmuch as the accepted electrical units are based on the metric system. We do not find that the value of the standard ohm as ultimately established by the Paris Congress, or the latest determination of the value of the ampere by Lord Rayleigh are given. These, perhaps, have been too recently published to admit of their incorporation in the body of the work, but might, we should think have been advantageously added in an appendix.

The mechanical execution of the work is excellent. The illustrations are abundant, and for the most part well designed, and, although to a certain extent lacking in finish, are, on the whole, very creditable.—*Electrician and Electrical Engineer.*

OPERATIONS OF THE ARMY UNDER BUELL—From June 10 to October 30, 1862, and the Buell Commission. By JAMES B. FRY. New York: D. Van Nostrand.

To those who served in the war of the Rebellion, the various monographs which are now coming out from time to time, on the particular battles or campaigns in which they may have taken part, furnish most interesting reading. This is, of course, especially the case when the monograph comes from one who has had unusual opportunity to know the facts about the campaign under discussion. Such monographs generally serve to show to those who were on the field, and participated in the movements and the fighting, how little they really knew about what seemingly went on under their own eyes.

Thus, to most of those who were actors in the campaign and battle of Perryville, General James B. Fry's "Operation of the Army under Buell" will give much new light, whether or no it shall change their previously formed opinion upon the merits or demerits of the chief commander. Many side lights are thrown upon the scene, and it is shown that not alone a commander's courage or capacity or genius, but many other things, enter into the determination of his success or failure in the field. His tact or want of tact in obtaining the good-will or incurring the enmity of some subordinate officer who chances to have the ear and the favor of some one in official authority, may have almost as much influence upon his career as his own capacity or incapacity.

In July of 1862, General Buell was in command of our forces in Northern Alabama and Central Tennessee. His orders were to repair his railroad communications, and then, if possible, capture Chattanooga. The Rebel authorities became alarmed, and collected a large force under Generals Bragg and Kirby Smith to oppose him. General Fry seems to prove conclusively that the Rebel armies greatly outnumbered General Buell's forces. In August they poured through the mountain passes of Eastern Tennessee, threatening Buell's left and rear, and endangering not only all Eastern Kentucky, but also the cities of Cincinnati and Louisville. Buell marched northward, concentrating his scattered troops, manœuvering for position, and offering battle at several points in Kentucky. Bragg was wary, and declined to fight, notwithstanding delay weakened him and strengthened his enemy. Buell

finally entered Louisville, was joined by many newly recruited and raw regiments, reorganized his army, and, although still outnumbered, early in October advanced rapidly upon Bragg. The latter fell back; and on the 8th of October was fought the battle of Perryville. Neither army was present on the field in full force, and though the battle was bloody and obstinate, it was seemingly indecisive. Buell expected the final conflict to begin on the following day, and prepared for it; but to his surprise, Bragg retreated during the night. Buell followed in pursuit, but found it impossible again to force his adversary to battle. As another Rebel force was then threatening Nashville, Buell left the pursuit at Crab Orchard, and on the 16th of October turned his army toward Nashville.

It is notorious that at this time there was great discontent in Buell's army. He had had to grapple, as a soldier, not only with military problems, but with all the troublesome questions growing out of the relations of his troops to the negro slaves and to their Rebel masters. Like most soldiers, he was a strict constructionist where laws and orders were concerned. He returned slaves which the laws did not yet allow him to free, and he punished with great severity all officers and men who were guilty of depredations upon Rebel property. This was hotly resented by his thinking bayonets, who had little respect for a discipline which conflicted with their most cherished political ideas. The discontent and almost insubordination which grew from these causes not only pervaded the army but was quickly communicated to influential persons in the North. General Fry seems to show pretty conclusively that there was little reason, up to this time, to criticize General Buell's military conduct of the campaign; and that the Government was at that time of the same opinion, is shown by the fact that on the 18th of October General Halleck, then the military adviser of the President at Washington, telegraphed to General Buell: "The rapid march of your army from Louisville and your victory at Perryville have given great satisfaction to the Government." Yet on the 24th orders were issued at Washington directing General Rosecrans to relieve General Buell of his command.

One may well ask, on what was this sudden change of opinion by the Government founded? Was it not caused by a single dispatch from one who is often called a "great war Governor"? And on what was that dispatch founded? Seemingly on the verbal report of "an officer just from Louisville." Who was that officer? what opportunity had he for full information? what was his capacity or fairness? what private grievance or resentment had he? These are things which history will never know; and yet his conversation with Governor Morton probably greatly changed, for better or for worse, the conduct of the war in the West. Here is the dispatch of Governor Morton, sent to President Lincoln on the night of the 21st—only two days before Buell's removal from command:

"An officer just from Louisville announces that Bragg has escaped with his army into East Tennessee, and that Buell is countermarching to Lebanon. * * * The butchery of our troops at Perryville was terrible. * * * Nothing but success, speedy and decided, will save our cause from utter destruction. In the Northwest, distrust and despair are seizing on the hearts of the people. O. P. Morton, Governor of Indiana."

The order for Buell's removal was dated only two days after this dispatch, but it was not made known to either the public or to General Buell until some days later. That it was quickly communicated to Governor Morton, however, is shown by the following dispatch, which was received by President Lincoln on the morning of the 25th:

"We were to start to-night to Washington to confer with you about Kentucky affairs. The removal of Buell and appointment of Rosecrans came not a moment too soon. * * * The history of the battle of Perryville and the campaign in Kentucky has never been told. The action you have taken renders our visit unnecessary."

This was signed "Richard Yates, Governor of Illinois," and "O. P. Morton, Governor of Indiana." Verily, as General Fry says, "this has a dictatorial ring." Evidently the "great war governors," who were supposed by the public to be busy putting men into the field, had something to do with taking men out of the field.

The fact is, great as were Buell's abilities and accomplishments as a soldier, he had never learned *tact*. Busied with the great end he had in view—the destruction of the Rebel army in his front—he was not careful about what opinions certain of his majors and colonels and brigadiers, who had the ears of the "war governors," might be forming of him. He did not see that his unmeasured words to an offender against discipline, and his protection of some Rebels' property, might be as potent factors in determining his own career as his success or failure in the field. He believed in discipline, and he enforced it upon all alike. He believed that the discipline of his own troops required that outrages upon Rebel property should be punished with severity, and that, as a military commander in the field, he had nothing to do with the freeing of the slaves of Rebel owners. His government had not yet undertaken this mission, or given him orders which would justify such action. He obeyed orders himself, and insisted on the obedience of others. He did not doubt that his motives and his actions would be understood. He was mistaken. But though he suffered, the army he trained never entirely lost some of the good qualities he gave it; and at least something of the service afterward rendered, something of the glory afterward gained, by the Army of the Cumberland, should be credited by his countrymen to General Don Carlos Buell. — Alexander C. McClurg *in the Dial.*

MISCELLANEOUS.

The "Two Manners of Motion of Water," shown by experiments, will be the subject of a lecture before the Royal Institution, March 28.

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A NEW METHOD OF SHAFT-SINKING THROUGH WATER-BEARING LOOSE MATERIALS.

By JAMES E. MILLS, B.S., Quincy, California.

Read at the Chicago Meeting of the American Institute of Mining Engineers.

In the work of exploring certain gold-bearing gravels in the American Valley, Plumas Co., California, entrusted to my charge by Prof. A. Agassiz, of Cambridge, and Q. A. Shaw, Esq., of Boston, it became necessary to sink a shaft through loose materials containing in some layers large quantities of water, and I have been compelled to devise a new method, which has proved successful and may be of service elsewhere.

The American Valley is a comparatively level tract of about 4,500 acres, surrounded with steep mountain slopes, which rise on the east, south and west to peaks of an elevation about 3,600 feet greater than that of the valley. The floor of the valley is of loose materials —gravels, sands, clays, etc.—and these rest in a rocky basin. The lip of the basin at the lowest point of its rim, where the waters leave the valley, is 165 feet higher than the bottom of it where the shaft struck the bed-rock, and the surface of the loose materials at the shaft is 45 feet higher than the lip. There is therefore, at the shaft, a thickness of 210 feet of gravels, sands and clays, resting in a bowl which receives water from a large area of mountain slopes. The shaft was sunk, by the method to be described, 20.7 feet further into the underlying bed-rock, making its whole depth 230.7 feet.

On geological grounds it was probable that the lower portions of the basin were filled with a mixture containing so much clay that water would pass through it slowly, and I tried to reach this comparatively compact material by the ordinary process of excavating and pumping out the water;* but at 14 feet below the surface the inflow of water became 67 cubic feet per minute, and was fast increasing, and the material was fine and was running in under the shoe and caving down outside of the shaft. It was evidently impracticable to sink to any considerable depth through such material, under the pressure existing when the water was pumped out of the shaft, even if the water should not exceed the practicable limits of pumping.

Exploration with drill was then made, and showed that for sixty feet the materials to be passed through were sands and gravels of a kind to run badly in places, and that below that depth, although the material, as a whole, was more

* The shaft started for this purpose is shown at A, Fig. 1 of the accompanying drawings. It was rectangular, and had an iron shoe, Aa, Fig, 1, which was pressed down with jack-screws as the excavation proceeded, and plank "cribbing" was built in as the shoe descended.

clayey, there were at intervals strata of loose, open sands and gravels as far as the drill went, which was to 170 feet.*

The depth was too great to permit the use of compressed air to balance the pressure of the water. The Kind-Chaudron process was not available, for the material would not stand unsupported for more than a few feet, in places not more than a few inches, much less for the whole depth. A caisson must be carried down with the excavation, and be kept pressed against the bottom.

The caisson adopted (B, Fig. 1 of the accompanying drawings) is a cylinder of 55 inches outside diameter, of wrought iron one-half inch thick, leaving inside diameter 4½ feet. It is made of rings 4 feet long, and the rings come together edge to edge, with edges accurately planed, and are joined together by butt-straps placed on the inside, 5 inches wide and ½ inch thick, to which the two adjoining rings are riveted. Each ring is of one sheet, the ends of which are accurately planed and brought together edge to edge, and connected at the vertical joint thus formed by a vertical butt strap of the same width and thickness as the horizontal one, and, like it, placed on the inside. The caisson is, therefore, a smooth cylinder on the outside, but on the inside the butt-straps project inward one-half inch. The lower edges of the horizontal straps are chamfered, the upper edges left horizontal. The vertical straps are thinned at the end to pass under the horizontal ones, so as to add to the projection of the latter not more than three-eighths inch. The rivets are three-quarters inch in diameter, countersunk at both ends, and 2½ inches apart from center to center in the rows, and the two rows at each joint are same distance apart. The caulking was all done at the edges of the butt-straps. The lower part of the cylinder, which was to withstand the greatest pressure, was tested under a pressure, applied to the outside, of 150 pounds to the square inch.

The rings were put together in pairs where they were manufactured, making sections 8 feet long, so that there remained one horizontal row of rivets to be driven, and one horizontal seam to be caulked, to each eight feet in length of caisson, at the shaft.

At the lower end of the caisson is a shoe (Ba, Fig. 1) of rolled steel one inch thick, welded at the ends of the sheet so as to make a continuous ring without vertical seam. Its lower edge is not quite horizontal, but beveled so that the outer surface of the ring is one-quarter inch longer than the inner surface, making a cutting edge. It is scarfed to the ring next above, one-half of its thickness being cut away all around for a length of three and a-quarter inches, and the ring above lapping by for the same length, so as to rest on the shoulder of the scarf, and have its outside surface flush with that of the ring.

At the upper edge of the caisson there was always a butt-strap, projecting half its width. It would not do to bring to bear on this butt-strap the pressure necessary to force down the caisson; and a wrought-iron band (Bb, Fig. 1), an inch thick and four inches wide was put there, resting on the edge of the main sheet, and surrounding the butt-strap, and projecting 1½ inch above the upper edges of the latter. The band was in halves, joined by bolts with nuts, as shown in the figure.

When the ground had been excavated and the caisson pressed down until its upper edge was 8 feet below the working floor, the band was taken off, another section riveted on, and the band put on top of the latter.

As the excavation proceeded, the caisson was forced down by hand with screws, reinforced toward the last by a falling weight.

The screws acted directly upon timbers 14 inches square on the end, resting on the band above mentioned. The bottom of the timbers, which rested directly on the band, was shod with 9 inches wide of wrought iron 1 inch thick. As the thread extended for only about 5 feet in length of the screws, blocking was put in between them and the lower timbers as the caisson went down, and removed when a new section was put on.

The screws, with their nuts and steps, are shown at C, Ca, Cb, Fig. 1, and need no further description, except that they were of strong cast-iron, called "gun-metal" in Boston, where they were made, and there were anti-friction buttons of hard

* There were geological data for concluding that the depth to bed-rock was not more than 250 feet.

steel in the steps. They were six in number, arranged, as shown in Fig. 1, in two sets of three each; but for the greatest part of the time only four were used, in two sets of two each. The nuts of each set were fixed into a timber 14 inches square on the end (D, Fig. 1), and this was held down against the thrust of the screws by four rods (E, Fig. 1) of round iron, 1½ inches in diameter, which passed through it and a similar timber (F, Fig. 1) placed under the main shaft-head timbers; the rods having nuts at both ends, and cast-iron washers, a foot square, between the nuts and the surface of the timbers. The shaft-head timbers bore the weight of the hoisting frame, and of a part of the building, and a load of gravel and clay, which was increased from time to time as became necessary.

One or more hydraulic presses, driven by the engine, would do the work much better, and at much less expense, than these screws. The pressure could then be kept even and constant while the excavation was going on.

The excavating was almost wholly done by a modification of the sand pump used in sinking artesian wells, which I will call the drill pump. It is shown at G, Fig. 1, and in Figs. 2, 3, 4. It is essentially a cylinder or barrel, with an annular drill at its lower edge, and a valve seated just above. It will be more fully described below. The flat drill shown in Fig. 10, was made to be used in ground too hard for the drill pump, and to break up large boulders, and was tried for these purposes a few times, but to little or no perceptible advantage. The drill-pump itself loosened the ground, broke up the boulders when too large to pass through the valve, and raised the material efficiently.

The auger stem (H, Fig. 1 and Fig. 5), jars (I, Fig. 1 and Fig. 6), sinker-bar (J, Fig. 1 and Fig. 7), rope-socket (K, Fig. 1 and Fig. 8), temper-screw and clamp (L, Fig. 1 and Fig. 9), and wrenches (Fig. 11), were the same as are used in the oil regions of Pennsylvania, where the tools were made, except that the auger-stem and sinker-bar were made shorter, and the necessary weight was secured by increased diameter, because the distance between the floor and the sheave (M, Fig. 1) was too short for tools of ordinary length.

The drill pump could not have a diameter near that of the shaft, like the sand pump of artesian wells, nor could it extend (like the drills used in artesian well-boring, and in the Kind-Chaudron process) across the shaft, so as to be brought to bear on the whole area of the bottom by being turned around. It was therefore hung from a movable point, so that its position could be shifted, and it could be placed over and dropped on to any part of the bottom of the shaft. This could hardly be accomplished with the walking-beam ordinarily in use in artesian well-boring; and the drill-rope, therefore, instead of being suspended from such a beam, was connected through the clamp and temper-screw with another rope (N, Fig. 1), which was attached by a loose wooden eye (O, Fig. 1) to the wrist-pin of the gear wheel through which the drilling motion was imparted to the tools.*

This latter rope, on its way to the gear wheel, passed over a rolling horizontal drum (P, Fig. 1, and A, Figs. 12, 13, 14), and between two vertical rollers (B, Figs. 12 and 13). The drum could be moved forward by a winch (Q, Fig. 1), and run back by a weight (R, Fig. 1), and the vertical rollers could be moved to right or left by a screw (C, Figs. 12, 13 and 14), and so the point of suspension of the tools could be placed over any part of the floor of the shaft where the drill pump was to work.

The hoisting-frame and machinery were the same that were put in and are now used for ordinary hoisting and pumping, except that for pumping, a larger gear wheel has been put in the place of the one which imparted drill motion to the tools. The winding-reel (S, Fig. 1) was operated by friction gear in raising the tools and load, and controlled by break in lowering them.

The tools were let down and the connecting rope attached by the clamp La, Fig. 1, and put into place by moving the drum and rollers; the main rope slackened above the clamp, and the machinery started, giving the lift-and-drop motion to the tools. When the drill pump was loaded, the connecting rope was unclamped from the main rope and discon-

* Such a connecting rope is in common use in sinking small artesian wells by horse-power, but it passes over a sheave fixed in position.

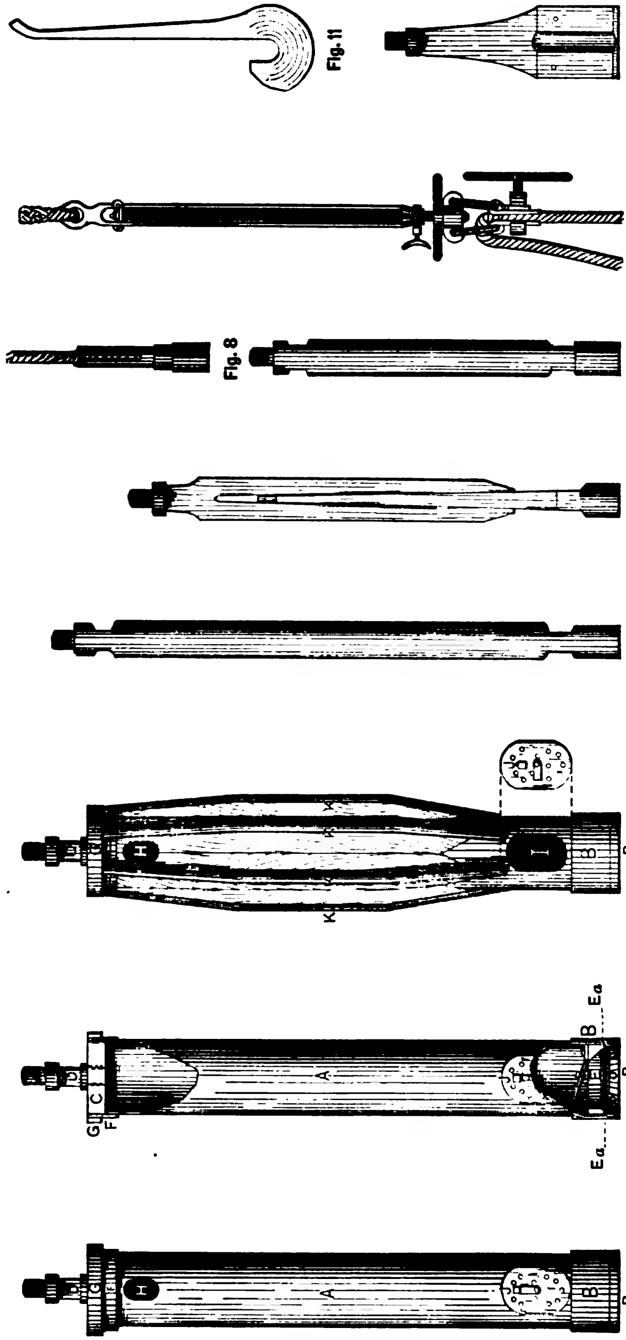


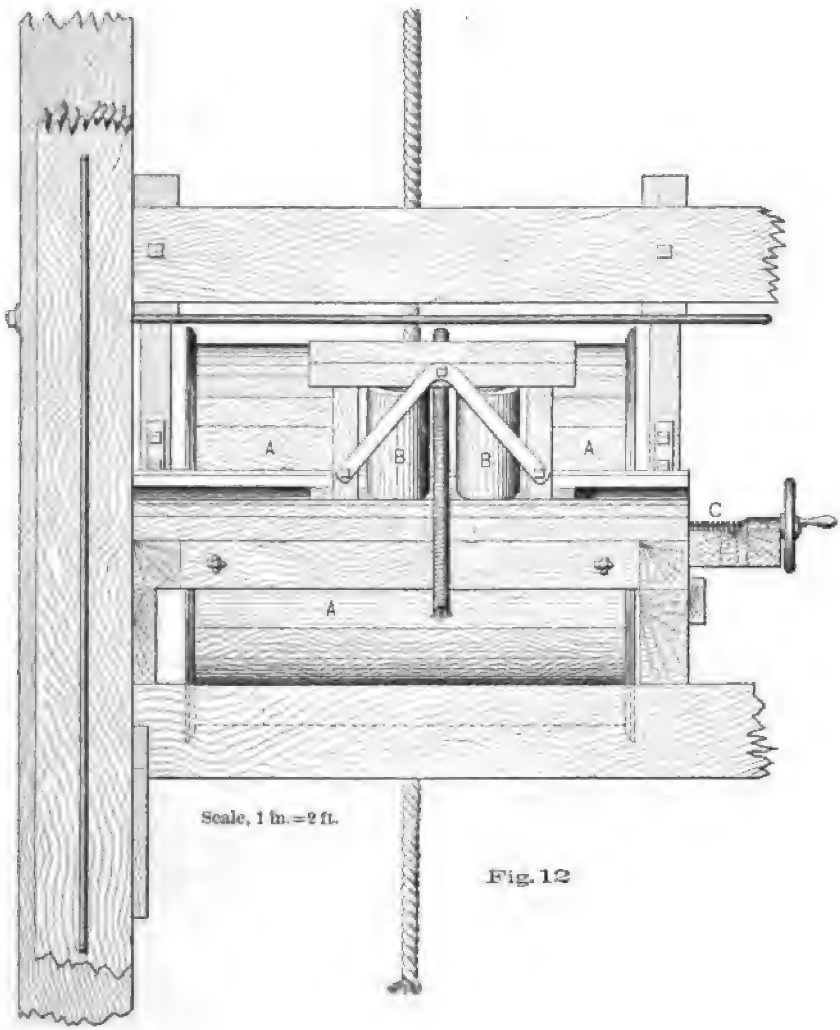
Fig. 11 Fig. 10 Fig. 9 Fig. 8 Fig. 7 Fig. 6 Fig. 5 Fig. 4 Fig. 3 Fig. 2

Scale, ⅜-inch = 1 foot.

nected with its driving wheel by slipping the wooden eye from the wrist-pin, the drum moved back, the tools raised, and the drill pump emptied.

The drill pump and its parts are shown at G, Fig. 1, and in Figs. 2, 3, 4. The whole length of the pump is 7 feet and 7 inches, besides the connecting pin (D, Figs. 2, 3, 4), which extends 10¼ inches above the head. The main barrel

(A, Figs. 2,3) is of wrought iron, ¼ inch thick, with welded vertical seam, is 7 feet 1 inch long, and its inside diameter is 1 foot. The shoe (B, Figs. 2, 3, 4) screws on to the barrel at its lower end. Its cutting edge is formed by a beveled steel ring (Ba, Figs. 2, 3, 4), which is fastened made more firm by shrinking an iron ring (G, Figs. 2, 3, 4) on to the band over the edge of the plate of wrought iron; and to make the joint more secure, a similar wrought-iron plate should be put into the upper end of the barrel, and a ring shrunk on to the band over its edge.



Scale, 1 in.=2 ft.

Fig. 12

with set screws to the iron of the shoe. The valve seat (E) is held in place by a projection (Ea, Fig. 3) at its lower edge, placed between the shoe and the lower edge of the main barrel. The head (C, Fig. 3) consists of a wrought-iron plate, 3 inches thick, held to the barrel by a band (F, Fig. 3) screwed on to it and on to the outside of the barrel. The joint was The pin (D, Figs. 2, 3, 4) by which the drill pump is attached to the auger-stem, is screwed into the iron plate of the head, as shown in Fig. 3. An opening (H, Figs. 1, 4) below the head let out the air as the drill pump descended into the shaft, and the water above the load during the drilling. The load was taken out of the barrel through the opening (I, Fig. 4) just

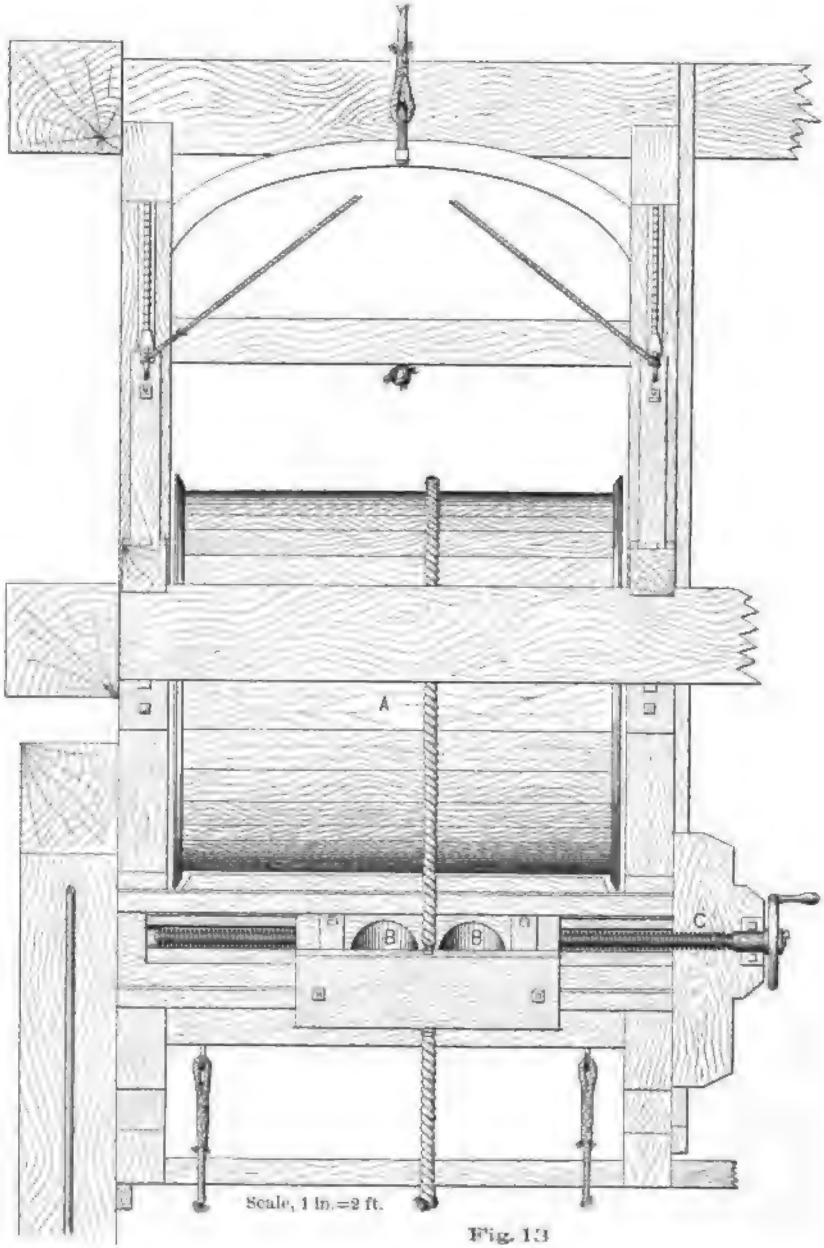
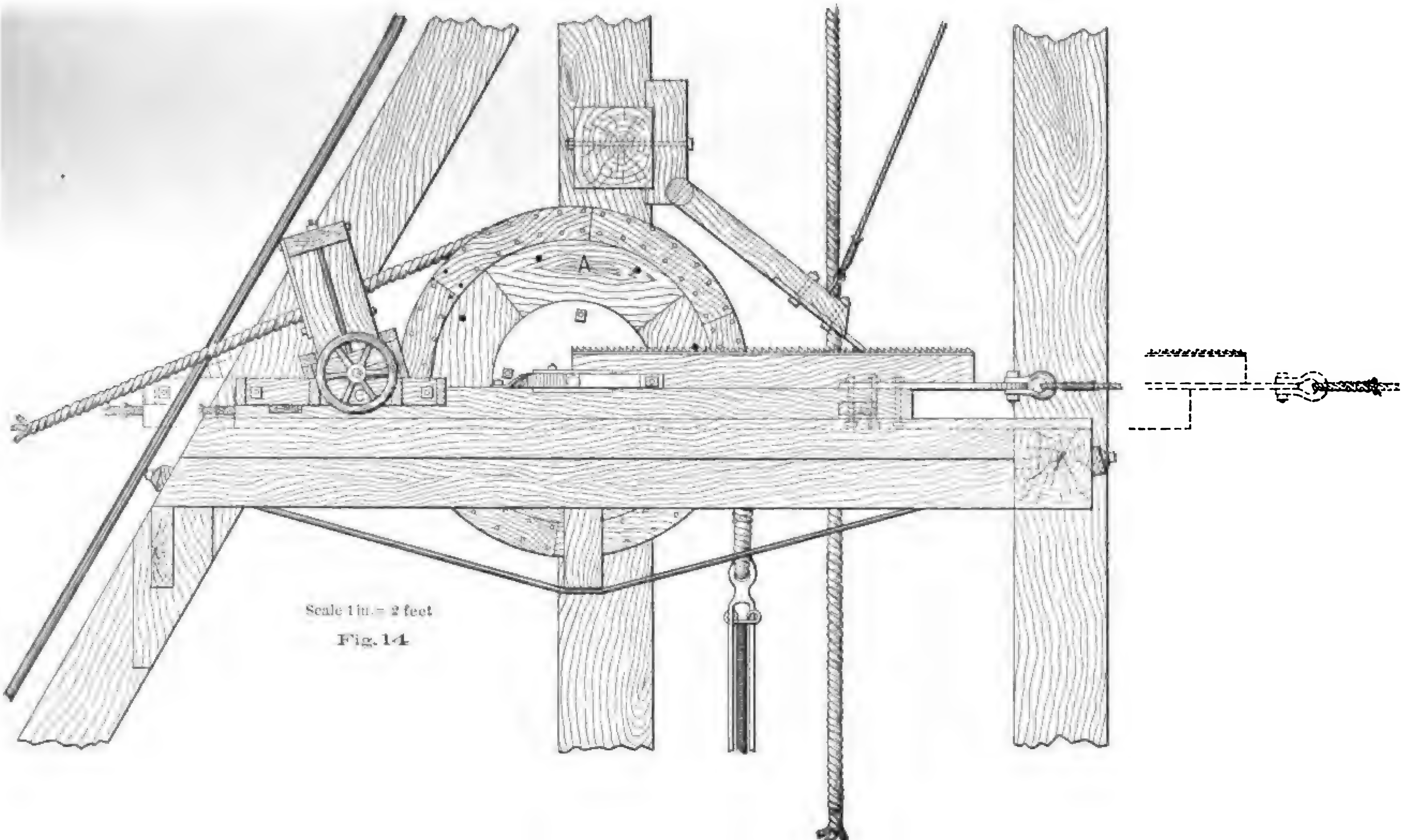


Fig. 13

over the shoe, with a small hoe made for the purpose. The opening was closed by the gate (J, Figs. 2, 3, 4). To prevent hitting the butt-straps of the caisson with the sharp edge of the shoe and the upper edge of the pump, the guard of wood and iron (K. Fig. 4) was put on. The weight of the tools as suspended was:

Pump	872½ lbs.
Auger-stem	692 "
Jars	385½ "
Sinker-bar	436 "
Rope-socket	54 "
Total	2,440 lbs.

To this is to be added the weight of



Scale 1 in. = 2 feet

Fig. 14

the rope, which increased with the depth. But the weight of the rope, rope-socket, sinker-bar and upper link of the jars added nothing directly to the force of the downward blow, and aided it only by overcoming the friction of the rope in passing through the water. In fact the sinker-bar was omitted for the greater part of the distance, but toward the last seemed to be of some service, especially when the water was thick with mud. The wood of the pump guard weighed about 72½ pounds when water-soaked, not far from the weight of the water it displaced, and neither increased nor lessened the force of the blow materially. There remained of the pump 800 pounds of iron, which, with the 692 lbs. of the auger-stem and the 193 pounds of the lower link of the jars, made a weight of iron 1,690 pounds. This, falling through water, is equal to 1,472 pounds falling through air, with some deduction to be made for the difference between the frictional resistance of air and water, and some further deduction for the greater weight and frictional resistance of the water when heavily charged with mud in suspension. The weight and consequent force of the blow increased as the pump became loaded, and at times the weight reached nearly the equivalent of a ton falling through air.

The lift and drop of the tools when drilling which was found best fitted for safe and efficient working of the machinery was 2 feet, and the number of drops 32 per minute.

When the drilling was going on, a man stood with his hand at the temper screw to let down the tools by turning the screw as the drill pump excavated, fast enough to render the blow effective, but not enough to permit the tools to topple over against the caisson. He could judge of the blow by the jar at his hand.

The length of time required to load the drill pump varied with the character and compactness of the material; but it was generally kept in motion at the bottom 20 minutes. The raising and lowering, connecting and disconnecting at the clamp and wrist-pin, taking out the gate and the load, and replacing the gate, took about ten minutes more, making 30 minutes to each charge, or two charges to the hour.

The area of the cross-section of the caisson, including its wall, is 16.5 square feet, and the area of cross-section of the drill pump 0.785 of a foot, or about [1]st of that of the caisson. The material in the drill pump was less compact than in place at the bottom of the shaft. A load of 1 foot in depth in the drill pump would lower the bottom of the shaft about ½ inch, and a load—quite often attained—of four feet in depth of the drill pump would lower the bottom of the shaft 2 inches, and 20 such loads in a day of ten hours would lower it 40 inches. The latter rate of sinking was sometimes attained, but the average rate was much lower.

The caisson was put into the rectangular shaft A, Fig. 1, which had already been sunk 14 feet, and the space outside of the caisson and within the rectangular shaft filled with clay. The only settling about the shaft observable at the surface was within the rectangular shaft. It was but little, and as it took place, clay was added at the top.

Before the work was fairly started, some days had been spent in becoming accustomed to the work and making needed alterations in the tools; and durthis time the caisson had been sunk 5 feet, or to 19 feet from the surface. The shaft and caisson were sunk the next 59.6 feet, or to 78.6 feet below the surface, in 41 working days of ten hours each, or at the rate of 1.45 feet per day. This included the time spent in putting on new sections to the caisson, which was 13.6 days.

At this depth (78.6 feet below surface), the water was taken out of the shaft. The material passed through had become more compact from about 60 feet downward, and I had expected to sink from here by ordinary methods; but the inflow of water at the bottom, which was at first 3.2 cubic feet a minute, increased in about four hours to 5.3 cubic feet per minute, and it was plainly best to continue on with the method that had succeeded so well thus far. The caisson was in good condition, whole and water-tight but slightly curved from end to end, so that the center at the bottom was about 4⅝ inches from a plumb-line dropped from the center at the top.

After a delay of some months in getting more sections of caisson, work was resumed, and the shaft was excavated and

the caisson sunk 102.4 feet further, or to 181 feet below natural surface in 91 days of ten hours each, actually spent upon the work. This included 22⅛ days spent in putting on new sections of caisson.

To this point the working force consisted of one skilled man to tend the engine, and two skilled drillers, and one or two laborers a portion of the time to aid at the screws, besides the engineer in charge. The working time was limited to ten hours a day, because the work was novel and it seemed hardly safe to have it go on in my absence, and also because I wished to observe carefully the character of the deposits passed through. The principal danger was that the excavations might be carried too far ahead of the caisson, and so cause caving and loosening of the material about the caisson. This would be obviated if hydraulic presses were substituted for screws as above suggested.

At the depth now reached (181 feet), the effect of the curve in the shaft began to be seriously felt; for the drill pump hanging vertically from the top could not be made to drop on to all parts of the floor, and left untouched a crescent-shaped area under the over-hanging portion of the cylinders. The material thus left had to be partly caved in and partly crowded in by increased pressure on the caisson. The force at the screws was increased, and consisted at times of six men; and a few weeks later the pressure of the screws was reinforced by blows of a ram consisting of a stick of timber 21 inches square and 22½ feet long, weighing, with eye-bolt, nut and washer by which it was suspended, 2,280 pounds. This was dropped two feet, thirty times a minute, on to a timber placed between the timbers on which the screws acted directly and the upper edge of the caisson.

Including the time consumed in putting on the ram, and putting on new sections, it took 23.5 days to sink the caisson the next 15.5 feet, to the depth of 196.5 feet below the surface. The excavation was 5.7 feet deeper.

The water was then again bailed out of the shaft. The curve of the shaft now left nearly half of the floor outside of the direct blow of the drill pump, and in the area thus out of reach were boulders of hard material and flattened shape, one of which was 0.44 cubic foot in bulk. These materials on the higher part of the floor were cut down by hand and thrown to the other side where the drill pump could reach them, and then taken out by the drill pump. In all, about four hours of time was spent in excavating by hand, and this was all the excavating done by hand in sinking the shaft.

At the lower edge the shoe showed a slight bulge, extending over about nine square inches of area, and projecting inwards about one-half inch at the middle of the bulge. Otherwise the caisson was unimpaired.

The inflow of water was varying, but at first averaged about 1.21 cubic feet per minute, and increased to about 2.34 cubic feet.

The next 13.5 feet brought the caisson to the surface of the bed-rock, at 210 feet below the natural surface. It took 23 days, including the time (4 days) spent in putting on two sections of caisson.

The bed-rock was a friable and rather soft clay slate, and was easily excavated with the drill pump. The caisson was sunk 20.7 feet in the bed-rock, to the depth of 230.7 feet below natural surface. To sink this 20.7 feet in bed-rock took 46 working days, including all the time of excavating, pressing caisson and putting on the sections of caisson.

After a month's delay the water was bailed out of the shaft. The caisson was in good condition, but somewhat deformed at its lower edge—not enough, however, to materially impair its usefulness. I think this deformation was caused by the bed-rock swelling and pressing against it unevenly.

The inflow of water varied, but averaged about 3.6 cubic feet per minute, or less than 5½ per cent. of the inflow at 14 feet from the surface. It came through the bed-rock clear, and was therefore not enlarging its channels.

The shaft had not only been sunk successfully through 210 feet of loose materials and 20.7 feet of rock, but the caisson had shut out the water of the upper loose gravels and sands and of the lower layers of similar materials had prevented the running in of sand, and left, to be contended with at the bottom, only a small inflow of clear water coming through rock.

A partition with ladder attached was put in as shown in Fig. 15, dividing the shaft into hoisting and pumping compartments, and a common "jack-head" pump, of 5 inches diameter and 4 feet stroke, was carried down at the same time with the partition.

A portion of the bed-rock was found loose and running, and where it was so, rods of round iron one inch in diameter and three feet long, pointed at the outer end, were driven out into the bed-rock through holes drilled through the shoe 2 inches above its lower edge, and 2 inches apart from center to center, and into the bed-rock as far as the rods were to go.

The shaft was then sunk by hand four feet below the lower edge of the shoe for a sump, and secured by staves driven around iron hoops. The staves were started about a temporary hoop of outside diameter 3 inches less than the inside diameter of the shoe, but the other hoops have the same inside diameter as the caisson, and when the staves were all in they were pushed outward and upward by the outer surface of the shoe so as to rest against it at their upper ends.

A row of iron rods like those before mentioned were driven over the space where the opening for the drift or gallery was to be made, and just above the second butt-strap counting from the bottom. The opening was cut out through the caisson between the lower edge of this butt-strap and the upper edge of the shoe. The plan of the opening and the timbers at the beginning of the drift are given in Fig. 16.

As long as the work is one of exploration, kibbles will answer for hoisting; but when necessary, a cage can be used to occupy the whole cross-section of the hoisting apartment, and when still more hoisting room is needed, another shaft can be sunk near by, and one of the two given up wholly to hoisting, and the other left for pump, ladder way and ventilating pipes; and, indeed, the capacity can be increased to any required extent by sinking a group of such shafts near one another, but with space enough between them to prevent breaking down or disturbing the ground between them while sinking. Shafts much larger than the one here described, indeed, of any ordinary size, could be sunk by the same method.

All the serious difficulty which this method of sinking encountered was caused by the curve of the caisson; and this is an avoidable difficulty. The use of hydraulic presses, as above suggested, would obviate the danger of excavating too far below the foot of the caisson, and

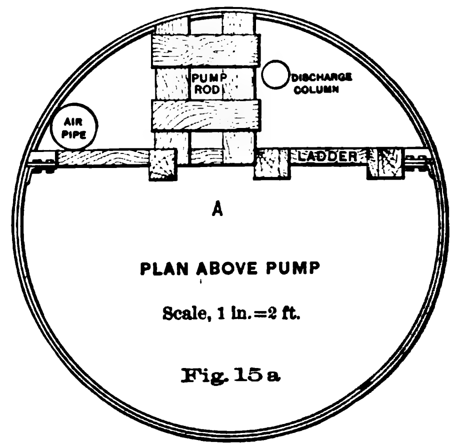


Fig. 15 a

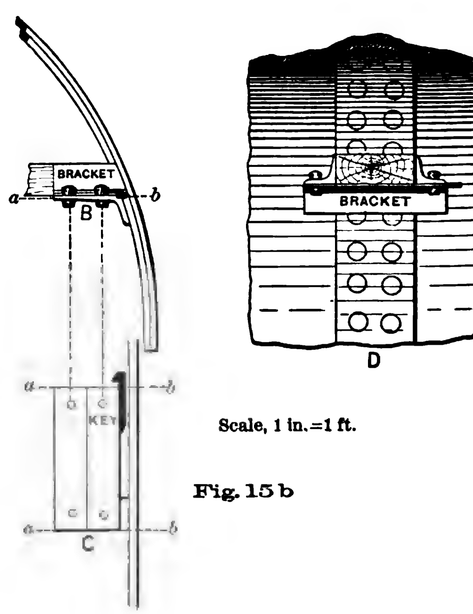


Fig. 15 b

would reduce the working force to three skilled hands.

The method is not only an efficient one where ordinary methods would fail, but it is also an economical, rapid and safe method of sinking through water-bear-

ing, loose materials, and could be applied to sinking caissons for bridge piers and other foundations, as well as to mining shafts and large artesian wells.

I have given above the time actually spent in sinking. The work was not, however, done continuously, but in intervals of time spent partly in waiting for new sections of caisson to be made and transported to the ground, but principally caused by delay in ordering them. The drilling began August 21, 1882, and ended November 29, 1883. The tools were contracted for in March, 1882, and at the same time 80 feet in length of the caisson, with shoe, screws, etc. The remaining sections of caisson were ordered in several lots. The reason for not obtaining enough to go to the bottom after the experience of a few feet had proved the efficiency of the excavating tools, was the lack of data for judging of the friction to be encountered. If the caisson had been perfectly vertical, I could now present definite data for determining the friction to be overcome under similar conditions; but on account of the curve in the shaft the necessary friction on the outer surface of the caisson cannot be separated from the added resistance of the materials at the bottom which were not reached directly by the drill pump, and I can therefore add little on the subject to what is above shown, namely, that to the depth of 230 feet the friction on the outer surface of such a caisson carried through such materials is overcome with a pressure easily applied and safely within the endurance of a wrought-iron cylinder, having the thickness of iron proportioned to area of its cross-section as in the one above described, and having a shoe of rolled steel, proportioned as the one described.

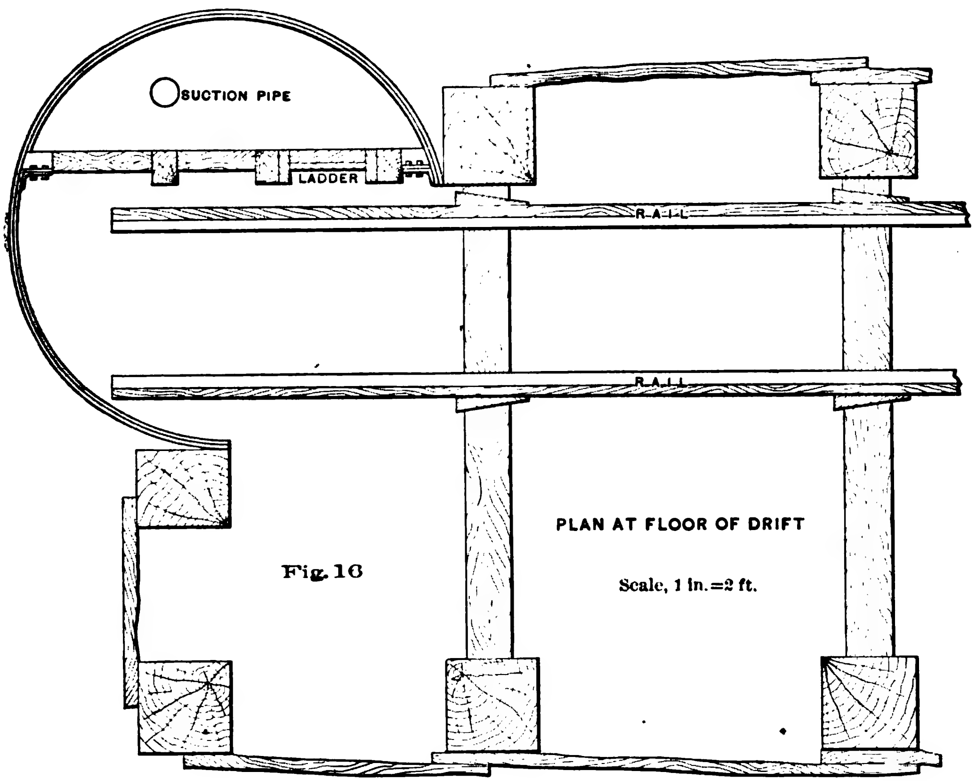


Fig. 16

INCANDESCENT LAMP ECONOMY.

By Assistant Engineer W. D. WEAVER, U. S. N.

Written for Van Nostrand's Engineering Magazine.

The cost of a given light from an incandescent lamp depends upon three quantities: the cost of the lamp, its life, and the cost of the energy to produce the light, the latter value being the entire cost of the energy actually delivered at the lamp, depreciation of plant and interest on capital being taken into consideration. The relation between these quantities may be formulated as follows:

Let a=the entire cost of one horse-power per hour *at the lamp.*

b=cost of lamp.

l=life of lamp.

x=candle power.

Then $\frac{b}{lx}$ is the hourly *lamp* cost of one candle power,

$\frac{CE}{745}\times a\times\frac{1}{x}$ is the hourly *energy* cost of the same,

and $\frac{b}{lx}+\frac{aCE}{745x}$ is the *entire* hourly cost of the unit of light.

If now the relation between the electrical energy transformed in a lamp and the resulting candle power were known, the economic conditions in respect to the life and cost of lamp and cost of horse power, could be determined by means of the above formula.

There is, however, a great diversity of opinion in regard to the law connecting these two variables. Dr. Higgs states that the candle power varies as the fourth power of the current, or as the square of the energy; Dr. Hagen (Dresden) gives the powers as five and three, respectively, while Mr. Preece, in a paper read before the British Association at Montreal, gave the law that the candle power varies as the sixth power of the current.

The plotted curve has evidently the form of curves represented by the equation $y=kx^n$, and its regularity suggests that the law is not so variable as to give reason for the diversity of values hitherto given, a conclusion justified by the result of the following investigation:

I.

In the calculations the data used were those of the careful experiments made at the Crystal Palace Electrical Exhibition, contained in an able report on the exhibition made to the Navy Department by Mr. F. J. Sprague, late U. S. N.

The values of n in the formula $y=kx^n$ were calculated for both energy and current by means of the following equations, in which P is the candle power, C the current, and E the electro-motive force between the lamp terminals:

$$n'=\frac{\log. P_2-\log. P_1}{\log. C_2+\log. E_2-\log. C_1-\log. E_1}$$

$$n''=\frac{\log. P_2-\log. P_1}{\log. C_2-\log. C_1}$$

The data used were those from experiments on 18 lamps,—6 Edison, 6 Swan, and 6 Maxim; the value of n was calculated for each successive experiment between about 5 candle power and the highest power noted that gave satisfactory results, and also for the extreme values of the candle power for each lamp.

The results are as given in the following table:

	Number of Determinations.	n' ($P=K\overline{CE}^{n'}$)	n'' ($P=KC^{n''}$)
Mean for successive values of candle power...	51	2.72	4.64
Mean for extreme values of candle power.....	18	2.71	4.70

Adopting as the exponent of the energy the value 2.70, the probable error with the above data is between +.07 and −.03. The variation of the value of the exponent of the current is much greater, however, and, adopting the value 4.70, the probable error is ±.30.

It would appear that n has a slightly greater value for the large filament or Maxim lamp than for the lamps with finer filaments; thus, with the former the mean values of n' and n'' are 2.75 and 4.97 respectively, and the variation small; this difference may be owing, however, to the more satisfactory data of the Maxim lamp, due to its larger range of candle-power.

Although the above values of n are probably correct within the limits given, other determinations should be made with smaller increments of candle power than in the experiments at the Crystal Palace, and the variation of the exponent n, if any, determined for different-sized filaments.

The following table will show the agreement of the calculated with the observed candle power. The figures at the head of the columns are from the report on the Crystal Palace Exhibition, and were taken by the committee from curves plotted with observed data. The other values were calculated from the equation $CE=KP^{\frac{1}{2.7}}$ in which k was determined for the normal candle power.

From the data of experiments made at the Munich Exhibition on the Gatehouse lamp, the following table was calculated, using the formula $CE=KP^{\frac{1}{2.7}}$:

Lamp.	Normal Candle Power.	k.	Candle Power.						
			5	10	15	20	30	60	90
Swan.....	20	21.36	5.52	10.40	15.30	20	30.18	61.51	97.60
Maxim....	20	26.98	6.18	10.67	15.17	20	29.95	58.30	94.63
Edison....	16	30.26	5.92	10.42	15	19.7	30.24	62.37	102.6

Observed C. P..	5.89	8.40	11.12	15.3	21.93	30.76	41.52
Calculated C. P.	6.87	9.01	11.67	15.3	20.3	27.3	38.3
Differences.....	.98	.61	.55	0	−19.3	−3.46	−3.12

As might have been expected from the difference of construction of the lamp, the agreement is not very close, and the differences show that the equation to the curve of this lamp is of a less degree, and by calculation from the data given was found to be $CE=KP^{1/6}$, and the constant k, 25.58.

II.

Knowing now the relation between the candle power and energy, we will return to our first formula,

$$\frac{b}{lx}+\frac{aCE}{745x} \qquad (1)$$

Expressing the law just deduced in the same notation, and substituting for CE in (1), we have finally for the whole cost of the unit light

$$y=\frac{b}{lx}+\frac{akx^{\frac{1}{2.7}}}{745x} \qquad (2)$$

With this equation, some interesting problems in incandescent lighting may be solved, and from it the following tables were deduced, showing the economic relation between the life of a lamp, its cost, and the cost of the energy furnished it. Table I. was constructed by assuming the life of a normal 20 candle

TABLE I.

$a=1$.

Total cost of unit light per hour, $y=\frac{100}{20\times1000}+\frac{1\times60}{745\times20}=.00902$.

Candle Power.	Cost of hourly H. P.	Watts. 2	2.5	3	3.5	4	4.5	5
10	1	2052	2607	3577	—	—	neg.	neg.
	2	—	neg.	neg.	neg.	neg.	neg.	neg.
	3	neg.	neg.	neg.	neg.	neg.	neg.	neg.
20	1	792	881	1000	1155	1364	1674	2160
	2	1367	2160	5138	neg.	neg.	neg.	neg.
	3	5138	neg.	neg.	neg.	neg.	neg.	neg.
30	1	479	518	564	618	684	766	869
	2	684	870	1195	1907	4706	neg.	neg.
	3	1195	2720	neg.	neg.	neg.	neg.	neg.
40	1	320	364	389	417	449	489	533
	2	420	533	654	846	1198	2132	—
	3	654	992	2053	neg.	neg.	neg.	neg.
50	1	266	280	295	313	334	355	380
	2	332	380	444	533	680	892	1344
	3	462	593	892	1801	neg.	neg.	neg.
60	1	216	226	237	249	262	276	292
	2	262	293	332	383	452	552	706
	3	382	414	552	827	1680	—	neg.
70	1	183	190	198	207	217	227	239
	2	216	239	266	300	344	403	488
	3	266	320	408	544	887	1806	neg.
80	1	158	163	170	177	185	192	200
	2	184	200	220	245	281	314	365
	3	221	259	318	398	541	858	2026
90	1	139	144	148	154	159	166	172
	2	160	173	188	207	228	255	290
	3	188	216	255	314	399	554	908
100	1	124	128	132	136	141	147	151
	2	141	151	163	178	195	218	240
	3	163	186	215	256	314	422	581

power lamp to be 1,000 hours, and that the lamp *when working at its normal capacity* would produce the normal light at the rate of 3 watts per candle power, the cost of the lamp being taken at $1.00, and the cost of an hourly horse-power at 1 cent; from these data y was calculated and the life of the same lamp at other candle powers, and of other lamps of different efficiencies and at different candle powers was found, the cost of the unit light being the same in each case and as deduced from the above data. The watts at the head of the columns represent the efficiency of the lamp, and are the number of watts required to produce the unit light when the lamp is being used at its normal power—20 candle power in each case; the figures 1, 2, 3 to the right of each candle power are the respective costs of an hourly horse-power, and enable a comparison to be made between the efficiencies at these different values, the cost of the unit light and of the lamp remaining the same. The other quantities are the lives that would have to be ob-

TABLE II.

$a=2.$

Total cost of unit light per hour, $y=.01305$.

Candle Power.	Cost of hourly H. P.	Watts. 2	2.5	3	3.5	4	4.5	5
10	1	1195	1272	1466	1724	2106	2696	3882
	2	2095	3822	—	neg.	neg.	neg.	neg.
	3	neg.	neg.	neg.	neg.	neg.	neg.	neg.
20	1	482	515	554	598	650	713	788
	2	650	788	1000	1367	2160	—	neg.
	3	1000	1674	3820	neg.	neg.	neg.	neg.
30	1	303	318	335	354	374	398	424
	2	374	424	489	577	704	902	1255
	3	473	634	902	1529	—	neg.	neg.
40	1	220	230	239	249	260	273	286
	2	261	286	318	358	409	480	571
	3	318	382	476	634	946	1908	—
50	1	173	179	185	192	199	207	215
	2	200	215	234	257	287	319	362
	3	235	270	319	410	510	695	1146
60	1	142	146	150	155	160	165	171
	2	160	171	184	199	216	236	267
	3	184	207	236	276	332	412	546
70	1	120	123	127	130	134	138	142
	2	134	142	152	162	174	186	205
	3	152	168	188	215	249	296	366
80	1	104	107	109	112	116	118	122
	2	115	121	128	136	145	156	167
	3	129	141	155	174	197	228	282
90	1	94	95	96	99	101	103	106
	2	101	106	111	118	125	132	141
	3	111	121	132	147	163	184	224
100	1	82	84	86	88	90	92	94
	2	90	94	98	103	109	116	122
	3	98	106	115	126	138	156	174

tained in order that the cost of the unit light may remain the same as given for each table and therefore represent the relative efficiencies of different lamps at different candle powers and at the given constant cost of the unit of light. Table II. was calculated in the same manner, but assuming the cost, y, of the unit light to be that resulting when the energy costs 2 cents per hourly horse-power, and in Table III. the latter value is taken at 3 cents.

For any other cost b' of a lamp, the quantities in each table should be multiplied by the ratio $\frac{b'}{100}$.

If a relation could be established between different candle powers of a lamp and the resulting lives, by substitution in (2) and differentiation, the value of the candle power could be obtained at which the cost of the unit light is a minimum. It is probable that an equation of the form $l=k(m-x)^n$ would approximately express this relation, m being the candle power at which the filament is practically

TABLE III.

$a=3$.

Total cost of unit light per hour=.01708.

Candle power.	Cost of hourly H. P.	Watts. 2	2.5	3	3.5	4	4.5	5
10	1	798	841	927	1019	1137	1293	1496
	2	1136	1505	2165	3246	—	neg.	neg.
	3	5163	—	neg.	neg.	neg.	neg.	neg.
20	1	347	364	383	403	427	452	482
	2	426	482	554	650	788	1000	1367
	3	554	718	1000	1674	5138	neg.	neg.
30	1	220	230	238	248	258	269	280
	2	258	280	307	340	380	431	498
	3	307	359	431	537	724	1095	2247
40	1	165	167	172	178	183	189	196
	2	183	196	210	227	246	270	297
	3	210	236	269	313	375	468	614
50	1	128	131	135	138	142	146	150
	2	142	150	159	169	182	194	209
	3	160	175	194	224	252	289	346
60	1	105	108	110	113	115	118	121
	2	115	121	127	134	142	150	160
	3	127	138	150	165	184	206	235
70	1	80	91	93	95	97	99	102
	2	97	101	106	111	117	123	130
	3	106	114	123	133	146	161	180
80	1	78	79	81	82	84	85	87
	2	84	87	91	95	99	103	109
	3	91	97	104	108	120	131	144
85	1	69	70	71	72	74	75	76
	2	74	76	79	82	86	89	93
	3	79	84	89	95	102	110	123
90	1	62	63	64	65	66	67	68
	2	66	68	70	73	75	79	81
	3	70	74	78	83	89	96	102

destroyed. As this important question from its commercial aspect appeals with especial force to those working large installations who have also the necessary facilities for making the experiments, it is probable that this final problem will soon be solved.

A PORTABLE MINING STORAGE BATTERY.—At a recent meeting of the Engineers' Club of Philadelphia, Mr. C. Henry Roney exhibited a portable storage battery for mining and exploring purposes, with small incandescent lamps. The battery was a modification of Plante's, devised by Dr. E. T. Starr, of Philadelphia, the electrodes consisting of V-shaped plates of sheet lead arranged over each other, the convexity downward, with a slight interval between them, their ends being attached to a lead frame by burned joints, the interstices between the plates being filled with finely divided metallic lead, exposing a large surface to oxidation and reduction when subjected to dynamic electric or voltaic energy, and, in turn, giving off a large percentage of the stored energy to incandescent lamps placed in the circuit. The battery is $3\frac{1}{4}$ inches long, $2\frac{3}{4}$ inches high, and $\frac{3}{4}$ inch thick, and is said to contain a small two-candle incandescent lamp at incandescence for about one hour. A battery sufficiently large to run an eight-candle lamp for ten or twelve hours would, consequently, not be too large or heavy to carry conveniently for mine or other underground exploration.

POSITIONS OF LIVE LOAD GIVING MAXIMUM STRAINS FOR SINGLE INTERSECTION TRUSSES—ANALYTICAL SOLUTION.

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Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

1. THE live load assumed below consists of two "consolidation" engines coupled, followed by cars weighing 2,240 lbs. per linear foot, the live load being concentrated upon points as given in the diagram on page 194.

The successive wheel loads are numbered from the left to the right in order w_1, w_2, ... up to the uniform car load.

The center of gravity of the two locomotives and tenders is easily found to be 49.25 feet from w_1 (the front wheel), and 54.15 feet from the front of the car load of 2,240 lbs. per foot.

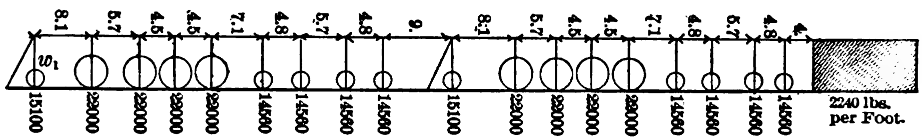
2. *For one truss*, the weights are ½ the above. The following table gives the successive weights for one truss, together with the total distances from w_1 and the total weights up to certain points.

The following little table of weights for feet and tenths of the uniform load of 1,120 lbs. per foot will prove a great convenience, in connection with the previous table, in rapidly estimating for any given length of load the corresponding weight.

Distance from w_1.	Load.	Total Loads.	Distance from w_1.	Total Loads.	Distance from w_1.	Total Loads.
Feet.	Lbs.	Lbs.	Feet.	Lbs.	Feet.	Lbs.
0	w_1 = 7,550	7,550	104	162,010	270	347,930
8.1	w_2 =11,000	18,550	106	164,250	280	359,130
13.8	w_3 =11,000	29,550	108	166,490	290	370,330
18.3	w_4 =11,000	40,550	110	168,730	300	381,530
22.8	w_5 =11,000	51,550	120	179,930	310	392,730
29.9	w_6 = 7,280	58,830	130	191,130	320	403,930
34.7	w_7 = 7,280	66,110	140	202,330	330	415,130
40.4	w_8 = 7,280	73,390	150	213,530	340	426,330
45.2	w_9 = 7,280	80,670	160	224,730	350	437,530
54.2	w_{10}= 7,550	88,220	170	235,930	360	448,730
62.3	w_{11}=11,000	99,220	180	247,130	370	459,930
68.0	w_{12}=11,000	110,220	190	258,330	380	471,130
72.5	w_{13}=11,000	121,220	200	269,530	390	482,330
77.0	w_{14}=11,000	132,220	210	280,730	400	493,530
84.1	w_{15}= 7,280	139,500	220	291,930	410	504,730
88.9	w_{16}= 7,280	146,780	230	303,130	420	515,930
94.6	w_{17}= 7,280	154,060	240	314,330	430	527,130
99.4	w_{18}= 7,280	161,340	250	325,530	440	538,330
103.4	——	161,340	260	336,730	450	549,530

Feet.	0	.1	.2	.3	.4	.5	.6	.7	.8	.9
0	0	112	224	336	448	560	672	784	896	1008
1	1120	1232	1344	1456	1568	1680	1792	1904	2016	2128
2	2240	2352	2464	2576	2688	2800	2912	3024	3136	3248
3	3360	3472	3584	3696	3808	3920	4032	4144	4256	4368
4	4480	4592	4704	4816	4928	5040	5152	5264	5376	5488
5	5600	5712	5824	5936	6048	6160	6272	6384	6496	6608
6	6720	6832	6944	7056	7168	7280	7392	7504	7616	7728
7	7840	7952	8064	8176	8288	8400	8512	8624	8736	8848
8	8960	9072	9184	9296	9408	9520	9632	9744	9856	9968
9	10080	10192	10304	10416	10528	10640	10752	10864	10976	11088

Fig. 1



POSITION OF LIVE LOAD CAUSING MAXIMUM MOMENTS.

3. The following investigation applies either to a beam or to a single intersection truss, with vertical members at each apex.

In the figure,

P $=(w_1+w_2+\ldots+w_n)=$ sum of live loads to left of B.

R $=$sum of total live loads on span when w_n is at B.

M $=$moment, when w_n is at B, about B.

M$'$=moment, when w_n is x to left of B, about B.

px=new load moving on as load is shifted x to left.

d =distance from B to left abutment.

l =length of span, $a=$ distance from w_n to w_{n+1}.

After the shifting, which moves w_n from B, to a point x to left of B, the left reaction V is increased by

$$R\frac{x}{l}+\frac{px^2}{2l}$$

and its moment by

$$d\left(R\frac{x}{l}+\frac{px^2}{2l}\right).$$

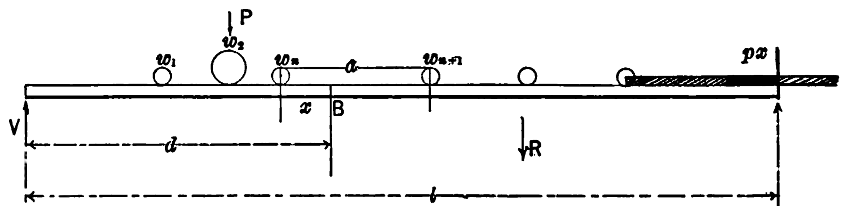


Fig. 2

The downward moment of P is increased by Px.

$$\therefore\ M'=M+\frac{a}{l}\left[R+\frac{px}{2}-P\frac{l}{d}\right]x\ .\ .\quad (1)$$

First. So long as the [] is + for $x=a$, the moment is increased by shifting the loads a to left, so that w_{n+1} rests at B, when by the same method we ascertain whether the moment is increased on shifting loads still further to left, so that w_{n+2} rests at B, and so on.

The [] may be minus for some value of x less than a, still if it is positive for $x=a$, M$'>$M and the moment is increased by the shifting.

Second. When however, P is increased, so that the [] becomes minus, even for $x=a$, M$'$ is greatest for $x=o$, or w_n at B, w_n being the weight just to left of B.

4. It is best to assume, at the start, such a position of the live load that the [] is plus, when as the loads shift to the left and new loads pass to the left of B the [] will eventually become minus. The instant this occurs, the nearest load on the left of B is the one that must rest at B to give the maximum moment at that point. The [] changes sign when

$$\frac{P}{R+\frac{pa}{2}}=\frac{d}{l}$$

or the ratio of the loads on the left of B to the whole load on the span, approximately, is equal to the ratio of the segment of the span to the left of B, to the whole span. This principle will indicate roughly how to place the loads for a first trial.

5. If the *new load* moving on the span is an *isolated wheel load* Q, whose distance from the right abutment is λ, when $x=a$; then the moment caused by it is

$$\frac{Q\lambda}{l}d=\frac{d}{l}\left(\frac{Q\lambda}{a}\right)a$$

which replaces the term $\frac{d}{l}\left(\frac{px}{2}\right)x$ in the formula. In this case so long as

$$\left(\mathrm{R}+\frac{\mathrm{Q}\lambda}{a}-\mathrm{P}\frac{l}{d}\right)$$

remains +, the live load is shifted to the left; when it changes to minus, the last load supposed to the left of B is then placed at B for a maximum moment.

It is not always necessary to compute the terms involving the new loads brought on the span by the shifting, as the proper conclusion can generally be derived without it. Still the labor of including these terms, when necessary, is trivial.

6. It is to be remembered that the above investigation applies exactly to a beam as well as to trusses, like the Pratt-Howe, triangular with suspenders or vertical posts at each apex, and in fact to any single system, where the moment about the apex B is the same as for a beam. It is obvious that in getting the moment for these trusses, that it is immaterial whether the loads are taken as they stand on the rails or as concentrated at the apices of the truss, for the moment of a resultant equals the sum of the moments of its components. For *all web members* inclined however, the downward moment about an apex, is generally different in the two cases. Thus in the Warren girder, if there is any live load on the chord piece under investigation, say to the right of B, it is not included in the above formula in the downward moment, whereas a part of this load is held up at the apex to the left of B, and hence should be so included. In fact, for a load placed at the middle of the chord piece, one-half of it is held at the left apex. We see, therefore, that the method is inapplicable to the loaded chord of a Warren girder, though it applies to the other chord and is probably sufficiently near in practice for both chords.

7. It is seen from the foregoing that we propose examining each apex in turn for its maximum moment. Where no new loads come on or move off during the entire shifting, we could at once find the *segments* of the span commanded by any particular wheel. The first analytical solution of this case, was given by the writer in this magazine, in the August, 1878, number, page 149 (also see *Van Nostrand's Science Series*, No. 38, page 69). This solution is in fact identical in its results with the one just exposed, except in the influence of new loads moving on—generally very small, though easily included.

8. To show the practical working of the above method, let us consider a span of 200 feet divided into 10 panels of 20 feet each. The apices are lettered, A, B, . . ., K, beginning at the left abutment. The results are put in tabular form for convenience; the first column gives the apex and the load causing the maximum moment about it when found, the next column gives w_n or the load first supposed at the apex and afterwards a distance a to left of the apex.

The length of R when w_n is at apex, value of a, the distance from w_n to w_{n+1}, weight of R, $\frac{pa}{2}$ and P, which follow in the remaining columns, are all quickly obtained from the two tables of loads given above.

From the discussion above when

$$\left(\mathrm{R}+\frac{\mathrm{P}a}{2}-\mathrm{P}\frac{l}{d}\right)$$

changes from + to —, the weight w_n corresponding to the last value is the one that should rest at the apex considered for maximum moment about that apex. As soon as found this weight is put under the corresponding apex in first column.

The weights have been written in full, though it suffices generally to express them to the nearest thousand pounds. The term $\frac{pa}{2}$ need not be computed except in doubtful cases. When the new load moving on is an isolated one, we replace $\frac{pa}{2}$ by $\frac{\mathrm{Q}\lambda}{a}$ as before explained.

A very short time will suffice to make out a table like the foregoing, so that the analytical method applied to this case is seen to be practical, and since it is absolutely correct in theory, it should prove satisfactory.

As the examination of a span of any length for maximum moments is made in exactly the same manner, it is not necessary to give other examples of the application of the method.

Apex.	w_n.	Length of R in feet.	a.	$R+\frac{pa}{2}$	$P\frac{l}{d}$
B	w_2	188.1	5.7	256,200+3,192>	$(w_1+w_2)10$=185,500
w_3	w_3	193.8	4.5	262,590+2,520<	$(w_1+..+w_3)10$=295,500
C	w_4	178.3	4.5	245,230+2,520>	$(w_1+..+w_4)'5$=202,750
w_5	w_5	182.8	7.1	250,270+3,976<	(1---5) 5=257,750
D	w_7	174.7	5.7	241,190+3,190>	(1---7)$\frac{10}{3}$=220,870
	w_8	180.4	4.8	247,580+2,688>	(1---8)$\frac{10}{3}$=244,630
w_9	w_9	185.2	9.	252,950+5,040<	(1---9)$\frac{10}{3}$=268,900
E	w_{11}	182.3	5.7	249,706+31,92>	(1---11)$\frac{10}{4}$=248,100
w_{12}	w_{12}	188.	4.5	256,090+2,520<	(1---12)$\frac{10}{4}$=275,550
F	w_{13}	172.5	4.5	238,730+2,520<	(1---13)2=242,440
w_{13}	w_{13}	168.	4.5	233,690+2,520>	(1---12)2=220,440

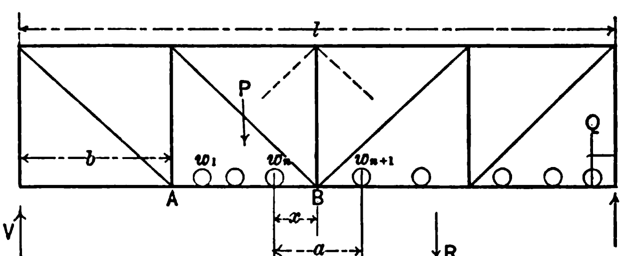


Fig. 3

POSITION OF LIVE LOAD GIVING MAXIMUM SHEAR.

9. In the following figure call

l =*length of span.*

b =*length of panel.*

$P=w_1+n\ldots.+w_n$ =*sum of live loads to left of* B.

R=*sum of live loads on span when w_n is at* B.

a =*distance from w_n to w_{n+1}.*

It will first be supposed that none of the loads are to the left of A, also that no new loads move on the span as the above load system is shifted to the left.

Let us take the position of the loads such that w_n is at B. If now we shift the entire system to the left of a distance x, the left reaction V, is increased by $R\frac{x}{l}$ and the reaction at A, or part of the loads ($P=w_1+\ldots+w_n$), held at A, is increased by $P\frac{x}{b}$; so that the *increase in the shear* is

$$S-S'=\frac{1}{l}\left[R-P\frac{l}{b}\right]x. \quad . \quad (1)$$

10. In case the [ ] is +, the shear is increased by the shifting to the left, otherwise it is decreased, in which last case w_n remains at B. When however the [ ] is +, S−S′ increases with x and is greatest when $x=a$, which moves w_{n+1} up to B at least. Other positions should similarly be tried, when it is very quickly determined what weight should rest at, or command,

an apex to give the maximum shear for any position of the live load in the panel just in front of that apex. By supposing P successively equal to (w_1), (w_1+w_2), $(w_1+w_2+w_3)$...., we readily deduce, that for those panels where

$$R \leqq w_1 \frac{l}{b} \text{ that } w_1 \textit{ rests at the right apex.}$$

For those panels where R exceeds $\left(w_1 \frac{l}{b}\right)$ but,

$$R \leqq (w_1+w_2)\frac{l}{b},\ w_2 \textit{ rests at the right apex.}$$

When R exceeds $(w_1+w_2)\frac{l}{b}$, but,

$$R \leqq (w_1+w_2+{}_3)\frac{l}{b},\ w_3 \textit{ rests at right apex,}$$

&c., &c.

11. For convenience we may call the portions of the span commanded by w_1, w_2, w_3, ..., respectively *first field, second field*, &c.

12. In applying the above principles, we first compute the right member of one of the above equalities and find the value of R from the table of loads equal to it; or (if there are none equal), less than, but nearest this value and record the length of load corresponding. Then if the length of load *from the weight commanding the apex, to the wheel load following the last one included in* R, is *equal to* a certain number of panel lengths, the field commanded by the weight extends so far, otherwise the field extends to the nearest apex to the right, where, generally R is less than before. When the supposed uniform load of 2,240 lbs. per foot is on the bridge, values of R can be obtained, exactly equal to the above right members, so that the length of load from the weight commanding the apex to the end of R is taken above, in ascertaining the extent of the field.

13. An example will render this plain. Take the span 200 feet, panel length 20 feet. Then w_1 commands when

$$R \leqq w_1 \frac{l}{b} = 75{,}500.$$

The next less value in table is

$$R = (w_1 + \ldots + w_3) = 73{,}390$$

corresponding to a length from w_1 to w_3 of 45.2 feet, hence w_1 commands apices 20′ and 40′ from right abutment. For second field commanded by w_2,

$$R \geqq (w_1+w_2)\ 10 = 185{,}500$$

This weight gives a length of R exactly 125 feet. Hence from w_2 to right abutment $= 116.9$; therefore w_2 commands apices 60, 80 and 100 feet from right. w_3 next commands up to

$$R \leqq (w_1+w_2+w_3)\,10 = 295{,}500,$$

or the balance of the span, as we see that R cannot attain to this value from tables for this span.

14. We have hitherto supposed that none of the loads, w_1, ..., w_n, pass to the left of A, or the apex in front of the one being considered, as rarely happens for usual panel lengths.

Should w_1 pass to left of A, when the loads are shifted a to left, formula (1) must be modified.

Call c the distance from w_1 to A, before the shifting (w_n being at B), then when the loads are shifted a to left, so that w_{n+1} rests at B, the shear as before is increased by

$$R\frac{a}{l},$$

but diminished by

$$\frac{w_1 c + (w_2 + \ldots . + w_n)\,a}{b}$$

since there is no increase in the downward effect of w_1 in the shear formula, when it passes A.

We have therefore,

$$S - S' = \frac{1}{l}\left[R - \left\{ w_1\frac{c}{a} + (w_2 + .. + w_n) \right\} \frac{l}{b}\right] a \quad . \quad . \quad (2)$$

15. When the [ ] is +, the shear is increased by the shifting, so that w_{n+1} rests at B, otherwise w_n commands B. In this formula a must not be less than c. In fact if we replace a by the variable x, then during the shifting to the left, till w_1 reaches A, the term $w_1\frac{c}{x}$ is replaced by w_1. When w_1 passes to the left of A, it remains $w_1\frac{c}{x}$ up to the limit $x=a$; hence if the [ ] is negative for $x=a$, it will continue so for any value of x less than a; hence when $x=o$, or w_n at B.

16. Next, let us consider the modifying influence of new loads coming on the span. Thus, let an isolated load Q just reach the right abutment when the loads have been shifted a distance x_1 to left.

At that instant by (1), the shear has been altered by

$$\frac{1}{l}\left(\text{R}-\text{P}\frac{l}{b}\right)x_1$$

Now if the loads are shifted y more to left,

$$\text{S}-\text{S}'=\frac{1}{l}\left\{\left(\text{R}-\text{P}\frac{l}{b}\right)(x_1+y)+\text{Q}y\right\}$$

$$=\frac{1}{l}\left[\left(\text{R}-\text{P}\frac{l}{b}\right)x_1+\left(\text{R}-\text{P}\frac{l}{b}+\text{Q}\right)y\right] \quad (3)$$

If (1) is positive, all the more is (3) positive, and both attain their maximum for $x=x_1+y=a$, or w_{n+1} at B.

If the $\square$ in (3) is negative for $y=o$, still if $(\text{R}-\text{P}\frac{l}{b}+\text{Q})$ is positive, a value may possibly be given to y less than $(a-x_1)$ that will cause (3) to become positive, when of course the shear increases with y up to its limit $(a-x_1)$, so that w_{n+1} rests at B. This is the only case where the conclusion derived from (1) is modified, and we shall find in practice that for such apices, the difference in shear due to taking w_n or w_{n+1} at apex is insignificant so that this investigation possesses only a theoretical interest, and need not generally be resorted to in practice, except for small spans.

For the case where the $\square$ in (3) and of course in (1) is negative for $y=a-x_1$ (or $x=a$) the results are the same, *i.e.*, w_n commands at B.

17. If the "new load" coming on is uniform and p pounds per foot, the left reaction is increased, when the loads are shifted x to left by $\frac{p\,x^2}{2l}$, and (1) is replaced by

$$\text{S}-\text{S}'=\frac{1}{l}\left(\text{R}-\text{P}\frac{l}{b}+\frac{px}{2}\right)x \quad . \quad . \quad (4)$$

If the $\square$ is + for $x\leq a$, (S−S′) increases with x, and is a maximum for $x=a$ or w_{n+1} at B.

If eq. (1) is negative still (4) may be positive, for a certain value of x, so that (1) would require w_n to rest at B, whilst (4) gives correctly w_{n+1} the command of that apex. The difference in shears is only a few hundred pounds though, at the outside, and diminishes as the span increases, being ¼ for a 400′ span what it is for a 100′ span.

18. For all cases, whether new loads coming on span are considered or not, we have seen that *for maximum shear, a wheel load is at the apex considered.*

19. Let us ascertain the modifications of the results in the last example when new loads coming on span are considered.

Thus w_1 was found to command the apex 40 feet from the right abutment. When in that position $\text{R}=w_1+\;..\;+w_7$ $=66110$. Now, suppose loads shifted 8.1 feet to left; w_8 moves on 7.7 feet and w_9 3.3 feet; the left reaction is then increased by

$$7280\,\frac{7.7+3.3}{200}=400\text{ lbs.},$$

and the shear by

$$\left(\frac{\text{R}}{l}-\frac{w_1}{b}\right)8.1+400=$$

$$(330-377)8.1+400=20\text{ pounds},$$

so that strictly the shear is increased 20 pounds by moving loads to left 8.1 feet, and w_1 commands apex 40′ from right. In practice this change is immaterial. The apices in the second and third fields above are unaltered by the consideration of the new loads; thus for w_3 at middle of span, R is 108.1 ft. long and $=166{,}600$, whence by (4),

$$\text{S}-\text{S}'=\tfrac{1}{200}[166{,}600-185{,}500+3192]x,$$
$$\therefore\ \text{S}-\text{S}'<0:$$

whence w_3 remains at central apex for maximum shear.

20. For a solid beam, the whole load P is minus in the shear formula, therefore since the whole system of loads must be shifted to the left a distance a, if at all, since the reaction is steadily increased without any increase in the minus terms, we have:

$$\text{S}-\text{S}'=\frac{1}{l}(\text{R}a+\text{Q}\lambda)-\text{P} \quad . \quad . \quad (5)$$

where Q represents the new load moving on the span and λ its lever arm about the right abutment when the loads have been shifted a distance a to the left. Compare DuBois's *Strains in Framed Structures*, p. 211, where this case is solved.

From (5) we see at once that when $\text{R}a+\text{Q}\lambda>\text{P}l$, the shear is increased by

moving w_{n+1} to B, otherwise w_n remains at B. The application is so obvious that an illustration was not considered necessary.

21. UNIFORM LOAD.—Suppose, in Fig. 3, the weights w_1, w_2 .... on span to be replaced by a uniform load extending a distance z to the left of B, to find the value of z, in order that the shear be a maximum. Call c=distance from B to right abutment.

Now the shear is *increased* on moving the uniform load to left so long as the reaction at left abutment of a small part of the load immediately in front is greater than the amount of this load supported at A, and *diminished* beyond the point where the reaction and part held at A are equal. At this last point, which marks the extremity of the load for maximum shear, we have—

$$\frac{z}{b}=\frac{c+z}{l}$$

$$\therefore\ z=\frac{c}{N-1}$$

$$\therefore\ c+z=\frac{Nc}{N-1}=\frac{c}{b}\ \frac{Nb}{N-1}$$

where N = number of panels in span $\therefore$ $Nb=l$, therefore, *length of load is*

$$c+z=\frac{c}{b}\cdot\frac{l}{N-1}.$$

Hence, divide the span into N−1 equal parts, then as c takes successively the the values b, $2b$, $3b$ ..., the load reaches from the right abutment to the 1st, 2d, ... divisions of the span for maximum shear. This result is obtained in another way in *Wood's Bridges*, page 126, edition of 1876. For a solid beam, the uniform load reaches to the point considered, as any load in front diminishes the shear. In fact, we see that as N increases indefinitely, that z, in a formula above, tends towards zero as its limit.

The method of apex loads gives a sufficiently near approximation in practice, and it looks like too great a refinement to specify any other, especially as the approximation gives a slight excess, and the assumed conditions of loading are but rarely realized in practice. In fact, for highway bridges, the uniform moving load, assumed in the calculations, is very rarely experienced, so that a higher unit strain is permissible than for railroad bridges, where the computed strains are frequently realized.

22. COMPUTATION OF STRESSES IN CHORD AND WEB MEMBERS.

After the position of the live load, giving maximum stresses is found, the computation for the live load *on one truss*, assumed in article 2, is very much facilitated by the use of the following table, giving the moment, about each

w	M.	w.	M.
w_1	0,000,000	w_{10}	2,585,858
w_2	61,155	w_{11}	3,300,440
w_3	166,890	w_{12}	3,865,994
w_4	299,865	w_{13}	4,361,984
w_5	482,340	w_{14}	4,907,474
w_6	848,345	w_{15}	5,846,236
w_7	1,130,729	w_{16}	6,515,836
w_8	1,507,556	w_{17}	7,352,482
w_9	1,859,828	w_{18}	8,091,970
		p	8,737,330

weight in turn, of the live loads to the left of that weight. In making the table, each successive moment was derived from the preceding, and checked independently at intervals and at the end. Thus to find the moment about p, the front of the uniform load from the preceding moment. We know $(w_1+w_2+ + \ ..+w_{18})$=161,340 pounds, and that it is 4 feet from w_{18} to p.

$\therefore\ M_p$ =8,091,970+161,340×4=8,737,-330.

By similar principles we find, the moment about a point x feet to right of p,

$$=8,737,330+161,340x+560x^2.$$

For any other live load than that assumed, and of course specifications vary in this regard, tables can be made out like the preceding, and the results written on a diagram of the loads, to scale. If the apex points of a given truss are laid off to the same scale, the diagram of loads can be placed alongside in position, and the proper distances, weights, moments, etc., taken off at a glance.

For the left *reaction*, we divide the moment about the right abutment by the length of span. This moment is found from the formula just given, when any of the uniform load is on the span, x rep-

resenting, the length of this uniform load on the span. When none of the uniform load is on the span, if a wheel load is at the right abutment, the preceding table gives the moment M at once. If the right abutment is y feet from the nearest wheel load w_n, the moment about the right abutment =

$$M_n + (w_1 + w_2 + \ldots + w_n)y.$$

Since the moment about any wheel of the load to the left of it is given directly by the table, we quickly find the moment of the live load about any apex of the truss required for the computation of chords.

For shear, the reaction is found in the same way. The load supported at the apex A (fig. 3) is found by dividing the proper moment in the table by a panel length. Subtracting this from the reaction, we have the shear.

All of this detail has been entered into in order that the article may prove of value in practice, particularly to those who rarely have occasion to make such computations.

THE TRUE GRAVIMETRIC CONSTANT.

By JACOB M. CLARK.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

PROPOSITION.

THE space through which a body at mean latitude near the surface of the earth, *in vacuo*, descends by virtue of the accelerating force of gravity, in $\frac{1}{1,000}$ of an hour, is precisely 2,500 geometric inches =100 geometric cubits = the side of a geometric square acre, so near as we know the constant.

[The geometric inch is taken at $\frac{1}{1,000,000,000}$ part of twice the earth's polar axis, and is $1\frac{1}{1,000}$ English inches, very nearly.]

The decimal correlation shown in the following table is contained in the existing expressions, founded on Newton's law; but is concealed by the use of sexagesimal notation, and of a 12-inch foot, founded on an inch slightly out of correlation with the geometric constant for latitude 45°. It does not appear except by dividing the circle, both for time and angular measure, *geometrically* (*i. e.*, by 24×10^n), and taking the unit of linear measure at a decimal of the earth's semi-polar axis.

If the English inch is used, the same expressions appear very nearly—we may say exactly—for latitude 30°. But for scientific use, the constant for latitude 45° is vastly to be preferred, both by reasons of its exact decimal relation to the semi-polar axis and of the superior simplicity with which the correction for latitude is applied.

Time in thousandths of an hour.	Acquired velocity, cubits.	Squares of the time.	Total descent, cubits.	Ratio of spaces in intervals.	Descent in separate of time, cubits.
1	200	1	100	1	100
2	400	4	400	3	300
3	600	9	900	5	500
4	800	16	1,600	7	700
5	1,000	25	2,500	9	900
6	1,200	36	3,600	11	1,100
7	1,400	49	4,900	13	1,300
8	1,600	64	6,400	15	1,500
9	1,800	81	8,100	17	1,700
10	2,000	100	10,000	19	1,900

That is

		Cubits.		Acre, Sides.
In $\frac{1}{10,000}$ of an hour, the total descent	=	1	=	.01
in $\frac{1}{1,000}$ " " "	=	100	=	1
in $\frac{1}{100}$ " " "	=	10,000	=	100
in $\frac{1}{10}$ " " "	=	1,000,000	=	10,000
and in one hour equal to 10 times the earth's semi-axis	=	100,000,000	=	1,000,000

THE SCIENTIFIC STUDY OF NAVAL ARCHITECTURE.*

By FRANCIS ELGAR, John Elder Professor of Naval Architecture.

From the "Nautical Magazine."

We have met together to-day upon no ordinary occasion. The institution of the John Elder Chair of Naval Architecture in this University is, in more senses than one, an unique and important event. No British University has ever before included in its curriculum anything like a complete treatment of the science of naval architecture; and this is also the first time that mercantile naval architects and shipbuilders have had an opportunity of commencing a course of scientific training which is intended to be specially and fully adapted to their requirements. Unlike nearly all the professors whom I have now the honor to call my colleagues, I have no predecessors, the records or traditions of whose life and work I can appeal to for encouragement, or inspiration, or as a pattern for imitation.

Although that is the case, however, so far as this Chair is concerned, many of the subjects which are to be taught from it have already had a far abler exponent in this University than I can ever aspire to become. The late Professor Macquorn Rankine was for many years one of the greatest authorities upon the sciences related to naval architecture and marine engineering. His immense ability and energy enabled him to achieve results in these departments of knowledge which would have made him famous had he attempted nothing else. But when we remember that what he thus did constituted but a small portion of his life-work, and was only connected with a few out of the many subjects comprised under the head of "Civil Engineering and Applied Mechanics," with all of which he dealt, we may truly marvel at what he accomplished for naval architecture. I cannot speak from personal experience of Professor Rankine's teaching in this University; but I am grateful for the opportunity of paying an humble tribute to his memory, in this place where he was so well known and so highly esteemed, by referring to what he did elsewhere.

Professor Rankine was a lecturer at the Royal School of Naval Architecture and Marine Engineering, at which I was a student; and he was one of the ablest members of, and most regular attendants at, the Institution of Naval Architects in London. There were very able men in London in those days at both the institutions I have named; and great advances were being made in the science of naval architecture, and in its application to the practical requirements of shipbuilders. Problems which had previously baffled all attempts at solution, even when made by some of the most eminent mathematicians of this and the previous century, were at length yielding to the genius and methods of modern investigators. The way was led by the late Mr. William Froude, with a paper upon "The Rolling of Ships," read before the Institution of Naval Architects in 1861.

Mr. Froude brought forward in this paper what, in his own words, "assumes to be a tolerably complete theoretical elucidation of a difficult and intricate subject, which has hitherto been treated as if unapproachable by the methods of regular investigation." I hardly need pause to say that Mr. Froude accom-

* Inaugural address, delivered in the University of Glasgow, November 11, 1884.

plished his self-imposed and formidable task with marvelous success. He thereby initiated a new era in the history of naval architecture, and one in which Professor Rankine was among the most prominent and distinguished figures. Professor Rankine helped more than any one to confirm and extend Mr. Froude's theories of wave motion and of the rolling of ships; and he also devoted himself with great ability and thoroughness to an investigation of the laws which govern the resistance of ships, and to the whole question of marine propulsion.

Professor Rankine impressed the students in London with the most profound respect and admiration for his great powers, and for the original and masterly way in which he dealt with everything he touched; while he possessed the rare gift of rousing their energies by his personal enthusiasm and charm of manner. No man with whom the students of naval architecture then came in contact had so powerful and elevating an influence upon their minds as Professor Rankine; and there is no one to whom they owe more, either for what he directly taught or for the mental stimulus they received by contact with him. It is a peculiar gratification to me, remembering what Professor Rankine was to the students of naval architecture of my time, to be able to bear testimony here to the high regard we all had for him, and to the high esteem in which we hold his memory.

It has formerly been thought necessary by some, who have been called upon to advocate or initiate courses of scientific teaching of naval architecture, to defend their position argumentatively, and even apologetically. I do not feel, however, that the circumstances of the present case require from me anything of the nature of an *apologia*. It will, perhaps, be more appropriate to the occasion, and more instructive to you, if I pass on at once to attempt to show how and when the recent irresistible demand for improved scientific knowledge, in virtue of which we are here to-day, arose in the Mercantile Marine; and shipbuilders became convinced, not merely of the desirability, but of the absolute necessity, of long and severe courses of preparatory scientific study, such as their forefathers, to all appearances, got on fairly well without. Perhaps a few discursive remarks upon these and other topics of general interest to the shipbuilding profession may also prove more acceptable to you to-day than a dissertation upon purely technical or abstract subjects.

The demand for scientific training in naval architecture is a comparatively modern one so far as the Mercantile Marine is concerned. It has long existed, however, in connection with the requirements of the Royal Navy. Even so long ago as Sir Walter Raleigh's day the builders of ships of war were reproached for want of technical knowledge, and for the errors and failures consequent upou it. That celebrated author, in his "Discourse on the Royal Navy and Sea Service," calls attention to the injurious effect upon a vessel's sailing qualities which is produced by over-immersion. He says "that the ship-wrights be not deceived herein (as for the most part they have ever been) they must be sure that the ship sinck no deeper into the water than they promise, for otherwise the bow and quarter will utterly spoile her sayling." Such complaints did not apply to the war-ships of this country only, for we find one of the earliest French writers upon naval architecture, Père l'Hoste, saying in 1697 of the ships of the French Navy, "It cannot be denied that the art of constructing ships, which is so necessary to the State, is the least perfect of all the arts. . . . Chance has so much to do with construction that the ships which are built with the greatest care, are commonly the worst, and those which are built carelessly are sometimes the best. Thus the largest ships are often the most defective, and more good ships are seen amongst the merchantmen than in the Royal Navy."

It was in the construction of war-ships that the greatest difficulties formerly arose, and that the need of scientific knowledge and improved methods of design first became manifest. Merchantmen were smaller in size, and much more simple and uniform in type and proportions, than war-ships, until quite recent times. If we look at the Mercantile Marine of fifty years ago we find that there were then about 750 merchant vessels built in each year. Out of these 750 vessels, only about 40 were above 300 tons, old builders' measurement, and no more

than 10 exceeded 500 tons. The whole number of ships composing the British Mercantile Marine in 1830 amounted to 19,110, of which but 168 were above 500 tons measurement. Thus more than 99 per cent. of our merchant vessels were at that time of less than 500 tons measurement. Very few were ever built of over 130 feet in length.* Mr. James Laing, of Sunderland, has been good enough to furnish me with particulars of a vessel which was built by his father, for his own use as a shipowner, in 1815. She is interesting as having been one of the first of the free traders to Calcutta after the breaking up in that year of the monopoly so long enjoyed by the East India Company. The length of this vessel was 109 feet 9 inches, breadth 29 feet 7 inches, and depth 20 feet 6 inches; the tonnage being 414. This is a typical illustration of a fine merchantman of that period, such as was employed upon the longest sea voyages. She was named the *Caledonia*, and successfully performed the voyage to India and back in about 10½ months, which was considered good work in those days.

It is clear that mercantile shipbuilders could not have been much troubled up to that time by difficult or novel problems in naval construction. They were simply occupied with the building of wooden craft of comparatively uniform types and proportions, and of so small a size that not one per cent. of the whole number exceeded 500 tons in measurement. The case was quite different, and had long been different, with the constructors of war ships. From the days of Henry VII. until now, successive Sovereigns and Governments of this country have striven in a more or less degree, first to rival, and afterwards to surpass other naval Powers in the strength and excellence of their ships of war. The efforts made at various times with this view have not always been wise nor successful. Large ships which could carry numerous guns, and carry them high above the water level with safety, and without serious detriment to sailing qualities, constituted the great and ever-growing, but very difficult, requirement of the naval authorities; and formed the problem which was continually proving a stumbling-block to successive generations of naval constructors. Henry VII. built several large vessels, one of which measured, according to various accounts, from 1,000 to 1,500 tons. In the fleet that sailed to meet the Spanish Armada there were two ships which exceeded 1,000 tons in burthen. James I. built a vessel which measured 1,400 tons; while the *Royal Sovereign*, built by Charles I. in 1637, was said to be "just so many tons in burthen as there have been years since our blessed Saviour's Incarnation, namely, 1637, and not one under or over."

Coming to more recent times we find that, at the commencement of the present century, British first-rate ships of the line had increased in size to 2,000, and even to 2,500, tons measurement. These vessels were heavily laden with top hamper, in the shape of three tiers of decks with numerous guns upon them. They frequently proved to be over-draught and unstable upon trial, and deficient in weatherly qualities at sea. No questions ever came before mercantile shipbuilders in that day of such magnitude, difficulty, or complexity, as those which war-ship constructors had to deal with. Mercantile shipbuilders produced small craft of a class whose qualities had been thoroughly tested by experience and were well understood by the owners and masters who had to work them. War-ship constructors, on the other hand, were the designers of comparatively huge vessels, whose type and loading introduced elements of special difficulty and danger.

Instability required to be carefully guarded against in war-ships of the description referred to, but our naval constructors did not then understand the principles upon which stability depends. Ships frequently proved so unstable when completed that all the remedies which could be devised, such as carrying extra ballast and doubling the planking at the water line, had to be applied. It is true that mathematical treatises upon the stability of ships had long been published in France and elsewhere; but they had been of no effect in improving the practice of naval architecture in this country. Even in France, where Daniel Bernouilli obtained a prize offered by

* Annals of Lloyd's Register, 1884.

the *Société Royale des Sciences* in the year 1757, for a *mémoire* in which the statical conditions of stability are clearly stated, high authorities upon naval architecture were unable, thirty years after, to account for a deficiency of stability in three ships that were remarkably crank.* In this country, where naval designers were much more deficient in scientific knowledge than in France, and where war ships were notoriously inferior to those of the French, mistakes were more general, and were frequently more fundamental and serious.

Mr. Wilson, a member of the first School of Naval Architecture, gives † an instructive and interesting account of the cutting down of a 64-gun two-decked ship of the line to a frigate of 38 guns, in the year 1794. He says that "so culpably ignorant were the English constructors that this operation, so well calculated, when properly conducted, to produce a good ship, was a complete failure. Seven feet of the upper part of the topsides, together with a deck and guns, weighing about 160 tons, were removed, by which her stability was greatly increased; but by a complete absurdity the sails were reduced one-sixth in area. In her first voyage the rolling was so excessive that she sprang several sets of topmasts. To mitigate this evil, in 1795 her masts and yards were increased to their original size; but as there was no decrease of ballast, she was still a very uneasy ship, and as a necessary result, her wear and tear were excessive.

Other sixty-fours were cut down, masted and ballasted in exactly the same manner, and, it need scarcely be added, experienced similar misfortunes; and although they were improved by enlarging their masts and yards, they were still bad ships. Had their transformations been scientifically conducted, a class of frigates would have been continued in the Navy, capable from their size, of coping with the large American frigates; and thus the disasters we experienced in the late war, from the superior force of that nation, would, without doubt, have been, not merely avoided, but turned into occurrences of a quite opposite character.

* L'art de la Marine. M. Romme.

† Papers upon Naval Architecture and other subjects connected with Naval Science. 1827-1833.

I may here say in passing that one of the chief reasons why shipbuilders, shipmasters and others who require to understand the principles upon which a ship's qualities depend, often remain for long periods in ignorance of the published information upon the subject is perhaps to be found in the fact that the style of treatment commonly adopted in works upon naval architecture is one which presupposes and requires advanced mathematical and highly technical knowledge in the readers. The celebrated mathematician, Euler, said in 1773, in the dedication of his work entitled, "Thé rie Complette de la Construction et de la Manœuvre des Vaisseaux," that "although forty years have elapsed since mathematicians have labored with some success, yet their discoveries are so much enveloped in profound calculations that mariners have scarce been able to derive any benefit from them." This reproach still attaches, I fear, to many writings upon naval architecture; and in removing the cause for it more may probably be done for the benefit and enlightment of students than in any other way. The great want of the time, in this department of science, is elementary explanations of principles and processes stated in clear and precise language, but freed as much as possible from advanced mathematical methods and terms, and from perplexing technicalities.

The difficulties of war-ship construction had become so overwhelming at the end of the last century owing to the above-named, and other causes that radical measures for remedying them could no longer be delayed. No such difficulties had arisen, however, be it observed, even at a much later date, in connection with the ships of the Mercantile Marine. Several attempts were made to improve the existing state of things in the Royal Navy, and to promote the spread of scientific knowledge of naval architecture in this country. These resulted in the establishment of the first School of Naval Architecture by the Admiralty, at Portsmouth, in the year 1811. Dr. Inman, the principal of the school, said, in an official document, printed by order of the House of Commons in 1833, that at that period "scarcely a single individual in this country knew correctly even the first element of the displacement of one

of our numerous ships, either light or load." So far as the Mercantile Marine was concerned this may not then have been of much practical importance, but in the Navy the case was very different.

The first School of Naval Architecture remained in operation during more than twenty years, and trained about forty students. The second School of Naval Architecture was founded at Portsmouth by the Admiralty in 1848, with Dr. Woolley as the Principal. This school had only a brief existence, and was closed in the course of a very few years. The third School of Naval Architecture, which also included marine engineering, was opened at South Kensington in 1864, and is now united with the Royal Naval College at Greenwich. The whole of these schools were instituted and carried on for the special purpose of training up war-ship designers and calculators for the work of the Royal Navy; and many students, highly trained in mathematics, and in the special work of war-ship construction, were educated in them. The members of the first School of Naval Architecture were for a long time the victims of professional jealousies and prejudices, and had to contend against the strong opposition of the old class of officers at the Admiralty and in the Dockyards. They were kept in subordinate positions until late in life; and it became the custom for the First Lord of the Admiralty to state annually, from his place in Parliament, that these "young men" (men between 40 and 50 years of age) "though gentlemen, and men of education, yet want experience, and therefore cannot be promoted." The members of the later schools of naval architecture have not had similar difficulties to contend with to anything like the same extent. Some now occupy the highest positions in Her Majesty's service, and have done much to advance the science of naval architecture, and especially to bring about great and long needed improvements in war-ship design.

The demand for improved scientific knowledge of naval architecture has thus existed in connection with the Royal Navy from a very early period, and was long of a most pressing character. The first attempts to supply it had already been made by the establishment of the first School of Naval Architecture, long before the Mercantile Marine was even affected. No difficulties approaching in any degree to those met with by war-ship designers arose to vex the souls of mercantile shipbuilders till after the modern age of steam shipping was entered upon, and till new types of vessels of enormous sizes and novel proportions were designed, in the construction of which previous shipbuilding experience was an unsafe or an insufficient guide.

It is interesting in this connection to observe that the Admiralty practice and that of the Mercantile Marine appear to have been very similar, even down to the present time, in dealing with small craft of old-fashioned types and proportions, which existed in sufficient numbers to enable the qualities of any one vessel to be inferred from those which others were known by experience to possess. At the court martial held to enquire into the cause of the capsizing of the small wooden frigate *Eurydice* in 1878, Mr. Barnaby, the Director of Naval Construction, said that she was inclined under his direction on the 11th May, 1877, not for the purpose of discovering whether she was a stable ship, but as a matter of scientific interest, because, so far as he knew, no sailing frigate or larger sailing ship had ever been inclined in the history of the Royal Navy. Mr. Barnaby explained that, "The reason for this must be the same as that which rules the present practice in the merchant Navy. There are about 5,000,000 tons of registered sailing ships in Great Britain, and it is not the practice of any owners to incline their ships." At the inquiry into the loss of H.M.S. *Atalanta*, another small wooden frigate, two years later, in 1880, Mr. Barnaby said: "The ship was never inclined so as to have the center of gravity ascertained. Her stability was known only in the sense that she was like, or nearly like, other ships whose behavior is well known." These statements show that the practice of the Admiralty and that of the Mercantile Marine have been very similar throughout in regard to the construction of ships of ordinary type, which do not appear to contain elements of special difficulty or danger. It was the development of peculiar types of ships,

possessing uncommon or abnormal features, which first made elaborate scientific treatment necessary in both cases, and to which it has latterly been applied. The exceptional difficulties caused by such development arose much earlier in the Royal Navy than in the Mercantile Marine, and therefore had to be dealt with at an earlier period, and at a time when the science of naval architecture was not nearly so advanced as it now is.

Fifty years ago the use of iron for ship construction and the employment of steam propulsion had only been attempted in a few vessels that were employed in coasting or river trades. As regards iron, few persons imagined that it was the material of the future for shipbuilding purposes. Although its use had, for some time, been advocated by a few able and far-seeing men, and some small craft had been constructed of it, the public and the great body of shipbuilders refused, in 1830, to believe that the wooden walls of old England were to be supplanted by a material that would naturally sink. "Who ever heard," it was derisively asked, "of iron floating?" The chief constructor of one of our Royal Naval Dockyards said to Mr. Scott Russell, with a feeling so strong, and with indignation so natural, that the latter never forgot it, "Don't talk to me about iron ships; it is contrary to nature." Steam propulsion was making progress, but it was not yet considered suitable for over-sea trades. Mr. David Napier had made engines of 200 horse-power; and lines of steamers were plying between Liverpool and the Clyde, and between London and Edinburgh. It was not thought possible, however, to make long voyages by means of steam propulsion. Men of high scientific reputation and position believed, in 1835, that in the then state of the marine engine the project of making a voyage by steam alone directly from New York to Liverpool was perfectly chimerical, and that persons might as well talk of "making a voyage from New York or Liverpool to the moon."

The shipbuilders of the old school held back as long as possible from taking the leap in the dark which was involved by commencing the production of iron steamers. The way was at first led, not by the great shipbuilders of the day, but by eminent engineers, such as Napier, Fairbairn, Brunel, Scott Russell, and others, who investigated and solved the leading structural and other problems involved in this great revolution in shipbuilding. It was at the request of Mr. Brunel, and for his guidance in designing the *Great Eastern*, that his friend Mr. William Froude commenced in 1856 his investigations into the laws of motion of a ship among waves. It was not till after mercantile shipbuilders began to build vessels of far greater sizes than those prevalent fifty years ago that the present demand for improved scientific knowledge began to be felt by them. The fact is that the modern changes in shipbuilding practice which followed upon the substitution of iron for wood as the material of construction, and the use of steam propulsion, not only increased the difficulties of manufacture, but they have gradually brought about a change in the shipbuilder's position with reference to his work, and in the nature of his responsibility for it.

Formerly a shipbuilder was merely the builder of a ship, in reality as well as in name. No better mechanics ever existed, nor men more skilled in the geometry and other practical sciences which bore directly upon their work, than many of the shipbuilders of the past. They were perfect masters of what they undertook to do, and possessed a vast amount of special knowledge and ingenuity. Many of the methods by which irregularly-shaped pieces of timber were prepared to their requisite forms involved geometrical processes of an extremely complicated and difficult kind. Some of the problems that were dealt with in practice by the old school of shipwrights would now puzzle many advanced students of descriptive and solid geometry. Shipbuilding was then a highly-developed mechanical art, much of the knowledge of which is not now required, and is, consequently, fast dying out.

The business of the shipbuilder used to be limited to the production of ships of the dimensions and description required by owners; and to building them of good sound timber, well fitted and fastened together. No elaborate calculations were requisite for determining whether one of these vessels would stand up when light, or be stable when laden with cer-

tain cargoes; nor were any but the roughest approximate methods necessary even for estimating the displacement or carrying capacity. Owners and masters, as well as the builders, had ample materials, derived from experience and from the observation of many similar vessels, by which to form their own judgments upon such points. Usually these ships would not stand up, when fully rigged and light, without ballast, and judging from the proportions given to them they must also have required ballast when laden with cargoes which were not composed of heavy dead-weight. In most cases iron kentledge was provided for them.

Few of these vessels would shift without ballast, except such as were of the old collier type, and were specially built for the coal trade. It is notorious, however, that the chief reason why so many of the ships of that day were crank, is to be found in the operation of the old tonnage laws, which took breadth into account in estimating tonnage, and ignored depth. Ships were built of great relative depths in proportion to their breadths, and initial stability was deliberately sacrificed in order to reduce the tonnage measurement. The instability of these ships was of such a character, however, owing to their form and proportions, that it could be dealt with and corrected in a practical manner, by means of the trained judgment and experience of the masters and stevedores. Any deficiency of stability was fully indicated by initial tenderness, and the curing of it was simply a question of putting ballast into the bottom. The sail-carrying power at sea usually furnished a good test of stability; and the experience thus gained was practically utilized in loading and ballasting vessels of all sizes and classes. It is important to observe that the instability which these vessels possessed was not of that dangerous and treacherous quality which exists in many modern steamers, and which renders them liable to capsize without previously giving obvious indications by which those on board may be sufficiently trusted to judge of their danger.

The mercantile shipbuilder of the present day has problems of a very different and much more complex and difficult character to deal with than his predecessors, and that is why the necessity for improved scientific knowledge is now so strongly felt. Many of the details of the mechanical work of construction are really simpler, and do not call for the exercise of the builder's personal skill and ingenuity, so far as the hull of the ship is concerned, to such an extent as formerly. What is now required of him is to predict with great accuracy, the weights of complicated iron and steel structures, of various types and sizes, with all their intricate fittings and machinery; the weight of cargo that such structures will carry at sea; the stability they will possess in different conditions of loading, and the treatment necessary to ensure a safe amount of stability being preserved upon all occasions; the amount of steam-power and the rate of coal consumption required to maintain given speeds at sea; and very frequently, the strength that is possessed by the hull to resist the straining action of waves.

Problems like these may now be put before a shipbuilder any day for solution, or, if he neglects to consider them for himself, when constructing certain types of vessels, he may afterwards be held to blame in the event of some unforeseen failure or casualty occurring. Disasters to ships that were once unhesitatingly, and even reverently, attributed to the "act of God" are now seen to be controllable, in many instances, by man, if such knowledge and foresight as he has the power of acquiring, be applied to the purpose. We now, perhaps, suffer occasionally from a reaction towards the opposite extreme, and too much may sometimes be expected of shipbuilders and shipowners in the way of preventing disasters at sea. It does not always appear to be sufficiently borne in mind that, whatever advances may have been made in the application of scientific knowledge and of practical mechanical skill to the construction of ships, men have not yet acquired the power of absolutely dominating all those vast and indefinable forces which nature frequently brings into play upon the ocean.

It is true that shipbuilders still build many ships, as formerly, to detailed specifications, prepared and furnished by the owners, but, even then, they appear liable to responsibilities which, though at pres-

ent very unsettled and indeterminate in their scope and character, and often quite unintended or unexpected by any-one, are not the less heavy and real. The time has arrived when it is evident that naval architects and shipbuilders require to possess a thorough knowledge of those natural laws upon which the qualities of ships and their safety at sea depend. Such knowledge is necessary, not only to prevent error or disaster in extreme cases, but for the more ordinary and commonplace, though not unimportant, purpose of enabling the requirements of a specification, or the stringent guarantees that are often contained in contracts, to be fulfilled in the simplest and most economical manner that is consistent with the stipulated degree of efficiency. It would be impossible for me to enumerate at the present time all the questions in which sound scientific principles are of importance to the naval architect of the present day, and with which he should endeavor to become acquainted. It is a knowledge of principles rather than of results that he should mainly aim at acquiring; because his information requires to be of that well-grounded, broad, and general character which is readily and directly applicable to novel and everchanging circumstances; and which may be acted upon with certainty and promptitude in difficult cases. Dr. Woolley stated this with great force and clearness in a paper read before the Institution of Naval Architects in 1864; and though he was then specially addressing the constructors of war ships, I cannot find any words more applicable to the present requirements of mercantile naval architecture. He said that the only way in which superiority in shipbuilding can be acquired is, "by possessing a class of shipbuilders trained in mathematical science with the powers of their minds invigorated and strengthened by a profound and severe course of study, able to deal with questions to which altered conditions are continually giving rise, not by trial and error—which is most frequently but another name for failure—not with the hesitating and trembling hand of the superficial sciolist—but with the firm grasp and bold readiness of the man profoundly skilled in the scientific principles of all kinds which may be made available to the art of naval construction, who feels himself thoroughly at home in them, and has acquired such power as to enable him to apply his principles readily and exactly, without fear of failure or of overlooking one principle while anxious to give effect to another."

It has sometimes been asked why, if the necessity for improved scientific knowledge has really been felt in the Mercantile Marine, students have not availed themselves of the educational facilities held out by the late Royal School of Naval Architecture, and by the Royal Naval College at Greenwich? The answer to this question is, to my mind, conclusive, and is only one which furnishes just cause for discouragement to ourselves. The Admiralty Schools of Naval Architecture, and the present Royal Naval College, were organized for the express purpose of supplying the special requirements of the Admiralty service. High mathematical attainments have been expected of all students before entering the college; and the training given to those who have entered has been of an advanced mathematical, and, so far as naval architecture is concerned, of too restricted and special a character for the practical purposes of non-Admiralty students. The instructors and lecturers in naval architecture have, without exception, been able and accomplished naval architects, but they have been specialists in Admiralty war-ship design. This, in my opinion, is alone sufficient to account for much of the want of confidence that has been shown by private shipbuilders in the suitability, for their purposes, of the training offered by the naval College.

The reason why the Admiralty Schools have, in the language of official authorities, been "hopeless failures," so far as the Mercantile Marine is concerned, is because they were too exclusively naval in their character and work, and because no adequate attempts were made to adapt them to the requirements of non-Admiralty students. The differences between the processes that are adopted in the Royal Navy and in the Mercantile Marine in the designing of ships are radical, and can only be properly appreciated by those who happen to be intimately acquainted with both. In the Mercantile Marine economy of time and labor is the chief aim of a designer; and short meth-

ods of calculation or of temporary approximation, which are but little appreciated in the Admiralty service, are employed for the purpose of enabling the work of construction to be quickly commenced and rapidly proceeded with. Economy of time and of cost of production, and how to secure these advantages, are among the chief subjects which mercantile naval architects require to study, and upon the practice of which their success mainly depends. Long periods are frequently occupied in investigating and arranging the details of war-ship design which cannot be obtained in the Mercantile Marine, and which, if insisted upon, would prove an effectual bar to progress in business.

If we consider the practical work of the shipyard, an accurate and full knowledge of which is invaluable to the naval architect, it will be seen how unsuitable is mere Admiralty teaching to the requirements of the Mercantile Marine by a comparison of the costs of labor in the two cases. The work upon the structural iron or steel portions of the hull of a vessel, which in the ships of the Navy often costs, according to the best information I can obtain, £20 per ton of weight, is carried out upon so much more economical and efficient a system in the Mercantile Marine, both as regards the time and labor expended, that in vessels which are at least equal in strength and durability to those of the Royal Navy, as is proved by the work they do, the cost of labor often amounts to no more than £5 per ton of weight. The time element is an equally important factor in the two classes of work, but this is a point which I cannot now pause to consider. The figures for cost of labor that I have given relate to vessels of as similar construction as possible, with water-tight double bottoms. The difference in cost of production is largely attributable to tedious and costly systems of work that are still cherished in the Royal Navy, but which have been long obsolete in the Mercantile Marine, and been supplanted by improved methods.

It is the two circumstances of the growing necessity felt by mercantile shipbuilders for scientific training in naval architecture, and the failure of the Royal Naval College to furnish such training as they require that have mainly led to the foundation of the John Elder Chair of Naval Architecture. And now the question arises of the method we are going to adopt, and of the kind of training we shall endeavor to give here. This will depend greatly upon the intelligence and energy of the students, and upon the amount of mathematical and general scientific knowledge with which they may be furnished when they come here. The course will be adapted, as far as possible, to their state of knowledge and to their practical necessities. But I cannot undertake to describe its scope in detail, nor to define its limits with precision, without some previous experience of the students. I have not come here with hard-and-fast ideas, nor with a cut-and-dried programme. Had I done so, our progress might thereby have been hampered or wrongly directed—it could hardly have been facilitated. I shall endeavor to help the students, to the best of my ability, to acquire a sound and scientific basis for such knowledge of shipbuilding and engineering as they may already possess—and the more they have the better—and to go forward to a complete study of those scientific principles upon the knowledge of which their success in life will greatly depend. At the same time, while insisting in the most unqualified manner upon the absolute necessity for scientific study, I must warn them against supposing that mere attendance at these classes during one, two, or any number of sessions is going to enable a student to become a competent naval architect or engineer. All that can be given here are intellectual tools with which to work with greatly-increased ease and precision in the practical operations of ship design and construction. Theoretical principles, and the manner in which they can be utilized with advantage in practice, will be taught; but it requires very much more to make a man a naval architect than knowledge that may be acquired within an University, however clever or hard-working a student he may be.

As an example of the training best adapted for producing good naval architects or engineers, and as a pattern which all students of these classes may study, and strive to copy with advantage, I cannot do better than refer to the great engineer after whom this Chair has been

named. Mr. John Elder always displayed great talent and application in the study of mathematics, which he diligently pursued in the High School of Glasgow; but he was prevented by a naturally delicate constitution from receiving any University education, except such as was obtained by attendance at the class of Civil Engineering in the old college. He studied privately, however, with great ardor, and acquired a large and varied amount of scientific knowledge, which was also complete and exact, and free from the defects in thoroughness and accuracy which so often beset self-taught scholars. John Elder served an apprenticeship of five years as an engineer in the works of Mr. Robert Napier, working in the pattern-shop, foundry and drawing-office. He afterwards worked as a pattern-maker at Bolton-le-Moors, and as a draughtsman at the great Grimsby Docks. His next situation was that of chief draughtsman under Mr. Napier, which he left three years afterwards to become a partner in the firm of Messrs. Randolph, Elder & Co.

The point to which I now wish particularly to draw your attention is the long and arduous practical training that Mr. Elder went through, It was this combined with his complete scientific knowledge and undoubted natural genius, which enabled him to achieve his great successes in after-life. The highest scientific knowledge attainable is of little use to the naval architect unless it exists in combination with good judgment and practical mechanical skill. Mr. Elder owed both his professional and commercial success to a rare combination of qualities. Prof. Rankine says in the memoir he wrote of him that the different qualifications possessed by Mr. John Elder "are so seldom found united in one man, that the tendency of popular opinion is to regard them as incompatible, and to look especially upon the knowledge, skill, and enterprise which lead an engineer to adopt new or unusual improvements in practice, as being fraught with danger to his success in business, and so no doubt they are, unless regulated by commercial sagacity."

There is, unfortunately, too great a tendency sometimes displayed by enthusiasts in the cause of technical education to elevate mathematical and scientific training above its true position, high as is that to which it is legitimately entitled, and to rely too exclusively upon the results of such training for guidance and power in the performance of large and intricate mechanical operations. It is a *sine qua non* for the modern Naval Architect, although, at the same time, it is by no means sufficient for all his numerous and varied requirements. It is even of little real practical use, unless there underlies it an intimate personal acquaintance with the mechanical operations of the shipyard and engine works, and with the properties and capabilities of the materials there dealt with. Together with this, there must likewise be the faculty, which is more essential than any, and which may be highly cultivated by all open, liberal, and intelligent minds.

"Good sense, which only is the gift of heaven,
And though no science, fairly worth the seven."

I shall not detain you any longer upon the present occasion. We shall commence our regular course of study to-morrow. It will be one which will be adapted, so far as I know how, to your practical needs, and to your present state of knowledge. I hope that the noble profession of naval architecture may one day reckon some of my present students among its chief ornaments; and that the Chair which bears the great and honored name of John Elder may be helpful in training up naval architects and marine engineers to rival him in all that is worthy, good and great.

In a paper on the influence of punching on mild steel, published in the *St. Petersburg Journal* and "Proc. Inst. C. E.," M. W. Beck-Gerard says:—"That although a search for incipient cracks proved fruitless, he has, he believes, for the first time, observed certain markings on the polished surfaces of the plates around the cold punched holes. Visible to the naked eye, and surrounding the holes, were bunches of lines starting tangentially to the holes, and curving slightly toward them. These lines branch out in opposite directions and intersect with some degree of regularity. They do not appear in the vicinity of drilled holes, but are distinct in cold punched holes reamed out. In forged iron they did not appear, although they were most distinct in the softest steel, and vanished when the metal reached the hardness due to 0.6 per cent. of carbon. An increase of thickness in the plate caused a corresponding increase in the number and clearness of the lines, upon which the shape of the hole was also found to have an effect."

A STANDARD METHOD OF STEAM-BOILER TRIALS.

Report of Committee to the American Society of Mechanical Engineers.

From Advanced Copy of Transactions of the American Society of Mechanical Engineers.

I.

1. The importance of establishing a method of trial of steam-boilers that should determine their steaming capacity under any given set of conditions, and their economy in the use of fuel is so thoroughly understood and so definitely recognized by engineers engaged in the design and construction, or management and use, of them, that it has been thought, by all, that some system of testing should be settled upon, for general use, which may be relied upon to give all the facts needed in relation to the performance of boilers, with substantial accuracy, and yet with least possible expenditure of time and money, and a method which may be adopted by any fairly skillful engineer, without the use, so far as it can be avoided, of unusual forms of apparatus.

It has been the duty of your committee to examine carefully the methods of testing boilers now practiced, to consider to what extent they present advantages or disadvantages, and finally to frame a Code of Instructions, embodying what they consider to be the best methods of experiment and the most satisfactory plan of working up and stating results. In this labor they have met with all the difficulties which usually attend an attempt to reconcile the opposing views of those who are acknowledged to be authorities on the subject, and to combine the various advantages possessed by systems in use among such members of the profession. Their object has been, not to prescribe a regulation method of test that shall be considered as representing the most complete possible system, and as giving results exact to the degree that would be satisfactory in purely scientific work, but to propose a code for daily use by the practising engineer which may be relied upon for substantial accuracy, to limits of error within the range of commercial requirements, one that may be adopted by any engineer deserving of a place within the ranks of the profession, and one that may be followed closely under ordinary circumstances of every-day experience.

It has, however, also been attempted to present, independently, a view of the refinements of recent practice in this matter which may be of service to the engineer who finds it desirable and possible to attempt work of scientific exactness, and of the utmost possible completeness.

2. The object of a trial of a steam-boiler, as your committee understands it, is to determine with great precision what is the quantity of steam that a boiler can supply continuously and regularly under definitely prescribed conditions; what is the condition, and therefore the commercial value, of that steam; what is the amount of fuel demanded to produce that steam supply; what is the character of the combustion, and what are the actual conditions of operation of the boiler when at work all of which should be presented in a report stating the results thus determined. The conditions prescribed for one trial may differ greatly from those demanded for another trial of the same, or of another boiler, and those differences of circumstances are often the essential matters to be studied, and their effect noted upon the performance of the boiler which is the subject of the report. In any case, however, it is assumed that the conditions under which the boiler is to be worked are to be definitely stated, and the engineer conducting the experiments is expected to ascertain as exactly as possible the facts which go to determine the performance of the boiler, and to state them with accuracy, conciseness and thoroughness.

In the attempt to ascertain those facts by observation of the actual performances of the boiler, the engineer meets with some serious difficulties, and finds it necessary to use the most perfect apparatus, and to exercise the utmost care and skill. In even so simple a matter as the weighing of coal and the measure-

ment of water, errors are often found where least expected, and they may make their appearance even in the work of painstaking and experienced practitioners. In conducting a steam-boiler trial, the weight of the water supplied to the boiler must be exactly determined; the weight of the fuel consumed must be similarly obtained; the state of the steam made must be determined, and those quantities must be noted at such frequent intervals, during the test, that the log will exhibit every irregularity of operation, and its effect upon the performance of the apparatus. To secure thoroughly satisfactory results, it is also necessary to know whether the combustion is perfect or imperfect, and to what extent the character of the combustion, as well as the other conditions and facts noted, are due to the excellences or the defects of the boiler, and what to external conditions.

3. In the tests of boilers made in earlier times, these determinations were made with comparative crudeness of method, and the results of such methods were such as would be considered to-day grossly inaccurate. The coal consumed was in large part estimated, and no pains were taken to ascertain the amount of unevaporated water carried over with the steam. It thus often happened that results were reported that were far beyond the utmost possible efficiency; the evaporation of water was sometimes reported at a higher figure than theoretical perfection would yield; and it has only been within a very recent period that it has been possible to judge what is the real performance of the standard types of steam-boiler, under ordinary circumstances, from the reports published, in many cases, as the work of engineers of reputation.

A great change has been gradually taking place both in the sentiments, and in the practice, of engineers engaged in this department of professional work, and it has come to be considered that the exact determination of power and economy of a steam-boiler demands the exercise of all the care, skill and perfection of method, and of apparatus required in the prosecution of any purely scientific investigation. It is now demanded that the weights of fuel and of water, the perfection of the combustion, the quality of the steam, and the temperatures of feed water and of furnace flue, shall be determined with an accuracy that shall be within the limits of error of good instruments; that, wherever possible, a system shall be adopted which shall permit of checking and verification of the reported results, and which shall make it as nearly as possible certain that no error can enter the work without prompt detection and correction. It is further demanded that all important work of this kind shall be done in substantially the same way, in order that comparisons may be easily made without the necessity of going through long and troublesome calculations in the effort to reduce the reports to be compared to a common basis.

4. This sentiment, and these demands, can evidently be complied with only by the establishment of some standard unit of measure of the power of the boiler, and of evaporative efficiency, and some definite and standard method of conducting the test. This standard unit of measure must be simple, easily defined, and convenient in application; the standard method of trial of boilers must be prescribed by a code of rules so concise, and yet so definite, that every member of the profession may be able to adopt them. The scheme must also be so complete that, if carefully and exactly followed, the precise value of the boiler may be ascertained with certainty. The method of record of facts determined must be such as will exhibit all the essential quantities in tabular form, and unobscured by the introduction of unessential figures.

5. Such a code of rules has been proposed by a joint committee of the Union of German Engineers and of the Central Union of Associations for the care of Steam-Boilers, and this set of regulations may be considered as the embodiment of the best ideas of our Continental colleagues on this subject. Your committee have examined this document with care, and find themselves in full accord with its proposers in the main, while obliged to offer some modifications of the scheme which are thought to make it more effective and more acceptable to American engineers. The Code of Rules for Use in Trials of Steam-Boilers which your committee proposes is herewith submitted

and will be found appended to this report.

6. The first provision of the code is that the object of the test to be made shall be precisely stated, and carefully kept in view during the whole trial, and during the preparation of the report. This object may be the determination of the steaming capacity, of the maximum efficiency, or of the quality of steam supplied by the boiler under specified conditions; or it may be the comparison of the qualities of various fuels. These objects cannot all be attained at one time, and maximum steaming capacity and maximum economy of fuel are, almost invariably, if not always, the result of incompatible conditions. The method of handling the steam generator will therefore differ as one or the other of these objects is to be sought.

It is next provided that the boiler to be tested shall be exactly measured, in order that data may be obtained for subsequent calculations. These measurements should be taken before the trial, not only because that is usually the most convenient time, but also because this preliminary measurement may sometimes lead to the discovery of defects of construction, as well as of proportions, that may suggest modifications of the plan of test previously laid down. The boiler is then to be put in the best possible order, in every respect, so that its observed merits or defects may not be obscured by accidental conditions having no relation to such merits or defects.

7. It is provided that an understanding shall be reached, before the trial, in regard to the kind of fuel to be used. Neglect of this precaution sometimes leads to needless misunderstandings, and avoidable criticism of the results reported. It is proposed that, where no reason of controlling importance exists to the contrary, the best obtainable coal shall be selected, for the reason that it is thought that a boiler can be better judged, and the results of its trial may be more satisfactorily compared with similar trials of other boilers when the very best work of which it is capable is done by it. The differences between separate lots of the best coals are less than the differences between separate lots of inferior fuels, and the comparison is thus less difficult where the former are used. To secure still more exact knowledge of the influence of the quality of the fuel upon the performance of the boiler, it is considered advisable to have an analysis made of the coal used, in all cases in which it can be done.

8. The establishment of the correctness of all the apparatus to be employed in the test is the first of the preliminaries to their use. The standardization of the instruments is a matter of supreme importance, since upon their accuracy the whole work of the engineer is dependent. It is also a work demanding, in most cases, unusual skill and care, and, to be satisfactory, must generally be performed either at the manufacturer's or at the office of the engineer conducting the trial. The scales can usually be standardized by the official sealer of weights and measures, and sealed by him; the water meters, if used, can be readily tested by the use of the scales so sealed; the thermometers are, as a rule, best tested by their makers, and should be sent to the maker for test immediately before, and directly after, the test, The engineer often has a carefully preserved standard with which they may be compared in his own office. The same remarks apply to the examination of the gauges used, which should be standardized both before and after their use. The apparatus used in connection with the calorimeter, in the determination of the quality of the steam made, demand exceptional care in this process; they are rarely of sufficient delicacy and accuracy to give perfectly satisfactory single determinations, even at the best, and the use of ordinary commercial instruments, carelessly standardized, or not at all, cannot be too strongly deprecated. Where it is unavoidable, the use of coarsely graduated thermometers and roughly constructed scales may be permitted, but only then when a very large number of observations are taken, and an average thus obtained which may be fairly expected to fall within reasonable limits of error—say within one per cent.

9. The precautions to be taken before beginning a trial are prescribed in some detail, since your committee consider them of great importance, and have known of serious embarrassment arising by their neglect.

The method of starting and of stop-

ping the trial is prescribed in a form which seems to your committee best as a whole. This is a very important matter, and yet is one upon which engineers of experience and acknowledged authority are not in complete accord. Your committee, for this, and also for the other reason, that the plan here proposed may not be always practicable, prescribe a second or alternative method, which may be adopted for such cases, or, where the engineer conducting the test is confident of being able to do better work than by the first of the two methods. The principles to be adhered to in this matter, as in every other detail of the operation of testing a boiler, are easily specified, but they are not always as easy of practice. All conditions should be as exactly the same at the beginning and at the end of the test as they can possibly be made. The period of the trial, and the times of stopping and of starting, should be capable of being exactly fixed, and the method of test should be such as should permit of the commencement and the end occurring at these exactly defined times, or, as an alternative, they should be such that the work done by the boiler during the less precisely determinable time of beginning and ending of the trial should be as nearly as possible *nil*, so that a slight error as to time may not appreciably affect the results. The "Standard Method" proposed by your committee is considered to meet these requirements as fully as any method in use. The alternative method is regarded as the next best.

10. During the trial, the essential provision should be the preservation of the utmost possible uniformity of working conditions throughout the whole period of the trial. Every irregularity gives rise to more or less loss of efficiency and to uncertainty in regard to the correctness of the reported figures. The nearer the working of the boiler is kept to the final average for the trial, the better.

11. Your committee consider the method of keeping the record of the test as no less important than the method of test itself. Perfect uniformity of operation within the boiler-room, and maximum efficiency of boiler, are best attainable where a system of record is adopted which allows of that regularity being shown at all times; and records in proper form are the best possible security against errors of observation. The committee are unanimous in recommending that graphical methods be adopted wherever it is found practicable to employ them. Such methods of record also exhibit most satisfactorily the accordance with, or the deviation from, the uniformity of operation considered so desirable on the score of efficiency and accuracy. Your committee present a form of record-blank which they consider as concise as is ever desirable in any important trial; and would prefer, in special cases, a more, rather than a less, complete record.

12. It is proposed by your committee as desirable that, when practicable, analyses of the escaping gases should be made. This is an operation of great simplicity, and can easily be made familiar to any engineer who chooses to take the trouble of learning it. If, for any reason, it is not found convenient to make the analysis in the office of the engineer, he can readily have the work done, at little expense, by intrusting his samples to a chemist of known skill and reliability. This provision is made as a part of the code, on the ground that it is only by a knowledge of the proportion of the constituents of the flue-gases that it can be determined whether the combustion is complete, whether the products of combustion are diluted with excess of air, and whether the fuel used has been so burned as to give its best effect. Such analyses also enable the engineer to ascertain the best method of burning the fuel. The code prescribes the precautions to be taken when this detail is carried into effect.

13. The establishment of the value of the "Unit of Evaporation," and that of the "Commercial Horse-Power" of the boiler, are matters which have been considered by your committee to be of essential importance to the settlement of a thoroughly complete standard method of trial, and of a perfectly satisfactory system of reporting results.

It has been evident to every observer that the sentiment above alluded to, as having arisen among engineers during the present generation, in favor of reducing the whole matter of testing boilers to an acknowledged standard system, has led to the endeavor, on the part of

the most able among practitioners, to determine standards with which to compare results obtained in such trials. The two most essential standards are those just referred to. The trials of boilers are made under a wide range of actual conditions, the steam pressure, the temperature of feed-water, the rate of combustion and of evaporation, and, in fact, every other variable condition, differing in any two trials to such an extent that direct comparison of the totals obtained, as a matter of information relating to the relative value of the boilers, or of the fuel used, becomes out of the question. It has thus gradually come to be the custom to reduce all results to the common standard of weight of water evaporated by the unit weight of fuel, the evaporation being considered to have taken place at mean atmospheric pressure, and at the temperature due that pressure, the feed-water being also assumed to have been supplied at that temperature. This is, in technical language, said to be the "equivalent evaporation from and at the boiling point" (212° Fahr.). This standard has now become so generally and so indisputably incorporated into the science and the practice of steam engineering that your committee, even were they acquainted with any other equally satisfactory unit, would hesitate to recommend anything else. They would simply express their approval of the adoption, and recommend the permanent retention, of this, which, as has been previously proposed, they would denominate the "*Unit of Evaporation*," *i. e.*, one pound of water at 212° F. evaporated into steam of the same temperature. This is equivalent to the utilization of 965.7 British thermal units per pound of water so evaporated. The relative economy of the boiler would then, as is customary, be expressed by the number of units of evaporation obtained per pound of combustible.

14. The character and magnitude of the unit to be chosen to express the "power" of the steam-boiler is not as well settled; and your committee find themselves compelled to take up, in this matter, a subject which has attracted much attention among engineers, and which remains nevertheless, unsettled. It is evident that, since the boiler is simply an apparatus for the generation of steam, and since the province of the steam-engine is to develop power from that steam, by the conversion of heat into mechanical energy; and since, furthermore, the engine develops power with a degree of efficiency which may vary enormously with differences in construction and operation of that machine, it cannot be properly said that we have any natural unit of power for rating steam-boilers. The most nearly scientific system of power rating yet proposed is perhaps that which considers the power of a boiler to be that expended by it in driving all the steam which it makes out against the pressure of the atmosphere, a system which does not, however, meet the wants of engineers. What is needed is a standard unit of boiler-power which may be used commercially in rating boilers, and in specifications prescribing the power to be demanded by the purchaser and guaranteed by the vender. It is evident that such a unit would not, if established, serve as a gauge of the power to be actually obtained from any given combination of engine and boiler, since the power so obtained must be measured by the indicator at the engine, and not at the boiler, and since in so measuring power, the economy and efficiency of the boiler would be elements left entirely out of the account. The best that can be done is obviously to assume a set of practically attainable conditions under which it would be fair to assume that the boiler may be properly expected to be operated in average good practice, and to take the power so obtainable as the measure of its power to be used in commercial and engineering transactions. The unit which has been most generally assumed, up to the present time, is the weight of steam demanded per horse-power per hour by a fairly good steam-engine. The magnitude of this quantity has been gradually and constantly decreasing from the earliest period of the history of the steam-engine. In the time of Watt, one cubic foot of water per hour per horse-power was thought a fair allowance; at the middle of the present century, ten pounds of coal was still not an unusual figure for the consumption per hour per horse-power, and five pounds, equivalent to about forty pounds of feed-water, was a good allowance for the best engines.

After the introduction of the modern forms of expansively working engines, this last figure was reduced twenty-five per cent., and the most recent improvements have still further lessened the consumption of fuel and of steam. By general consent, it seems likely that the unit which will meet with final acceptance for general purposes, in the estimation of boiler-power, is not far from thirty pounds of dry steam per horse-power per hour. This represents the performance of good mill engines of the non-condensing type. Large engines, with condensers, or compounded cylinders, will do better by from twenty to thirty per cent. Your committee have concluded to recommend thirty pounds as the unit of boiler-power.

15. But it remains to be determined under what circumstances this figure shall be taken as standard. It is on this subject that practitioners, and the members of your committee, as well, are not fully agreed. Nevertheless, it is, in their opinion, advisable that some definite set of conditions be prescribed to be taken as standard without waiting for complete accordance of opinion throughout the profession.

The Committee of Judges of the Centennial Exhibition, to whom the trials of competing boilers at that exhibition were intrusted, met with this same problem, and finally agreed to solve it, at least so far as the work of that committee was concerned, by the adoption of the unit, 30 *pounds of water evaporated into dry steam per hour from feed water at* 100° *Fahrenheit, and under a pressure of seventy pounds per square inch above the atmosphere*, these conditions being considered by them to represent fairly average practice. The quantity of heat demanded to evaporate a pound of water under these conditions is 1110.2 British thermal units, or 1.1496 units of evaporation (such as are here adopted and proposed for general use). The unit of power proposed is thus equivalent to the development of 33,305 heat units per hour, or 34,488 units of evaporation. The arguments in favor of the retention of this unit of power without modification are: (1) It is, to a certain extent, established, being the only unit proposed by authority, up to the present time, which has been accepted to any important extent by practitioners. (2) It is considered by its proposers, and probably by engineers generally, to fairly represent good average practice in the application of steam-power, as exhibited in the operation of engines and boilers under ordinary actual working conditions. Both of these arguments are deemed by your committee to be valid and deserving of careful consideration. The abandonment of an already established standard is always confusing, and should not be permitted without the most cogent of reasons.

Another standard unit, which has been proposed to your committee, and strongly urged as preferable to the above, is that represented by the evaporation of thirty pounds of feed-water into dry steam "*from and at the boiling point*," at mean atmospheric pressure (212° F.). The arguments in favor of this unit are the following: (1) In the determination of the unit of evaporation to be used in steam-boiler practice, it has been generally, and probably unanimously, decided by engineers that the evaporation shall be reckoned as having been effected at the boiling point from water assumed also to be supplied at that temperature, and that one pound thus evaporated shall be the unit. This being the established unit of evaporation, consistency and convenience both dictate that the power of the boiler should be expressed in the same unit, or some handy multiple thereof; (2) It is submitted that the reduction of this unit to an exact multiple of the unit of evaporation will greatly facilitate calculations, inasmuch as the work done by the boiler is to be reduced to the same standard of feed-temperature and temperature of evaporation; (3) By the adoption of this unit, the trouble and risk of error coming from the attempt to use a factor as proposed above, differing from the multiple of the already accepted factor by 14.96 per cent., may be entirely avoided; (4) The unit last proposed is equivalent to 26.09 pounds of water evaporated from 100° Fahr. into steam at 70 pounds pressure, and is claimed to be itself more nearly representative of good average practice than the centennial unit.

Your committee has carefully weighed the arguments relating to these standards, as they were presented in writing

by their respective advocates, and, after due consideration, has determined to accept the Centennial Standard, the first above mentioned,, and to recommend that in all standard trials the commercial horse-power be taken as *an evaporation of* 30 *pounds of water per hour from a feed-water temperature of* 100° *Fahr. into steam at* 70 *pounds gauge pressure*, which shall be considered to be equal to 34½ units of evaporation, that is, to 34½ pounds of water evaporated from a feed-water temperature of 212° Fahr. into steam at the same temperature. This standard is equal to 33,305 thermal units per hour.*

It is the opinion of this committee that a boiler rated at any stated number of horse-powers should be capable of developing that power with easy firing, moderate draught and ordinary fuel, while exhibiting good economy; and further, that the boiler should be capable of developing at least one-third more than its rated power to meet emergencies at times when maximum economy is not the most important object to be attained.

Any increase of temperature derived from a feed-water heater acted upon by the products of combustion escaping from a boiler should not be credited to the evaporative efficiency of the boiler, except by agreement; and in the latter case accurate tests can be made only with feed-water of the average temperature used during the regular operation of the boiler.

The code presented by your committee is necessarily, as has been already indicated, condensed to the utmost possible extent consistent with exactness, and essential completeness. In matters of detail, it must be left to the engineer to carry out the evident spirit and intent of the code by devising his own methods; and it may be expected that every engineer will be competent to supplement the directions here given, as far as is necessary.

* According to the tables in Porter's "Treatise on the Richards Steam-Engine Indicator," which tables the committee would recommend for general acceptance by engineers, an evaporation of 30 pounds of water from 100° F., into steam at 70 pounds pressure is equal to an evaporation of 34.488 pounds from and at 212°; and an evaporation of 34½ pounds from and at 212° F., is equal to 30.010 pounds from 100° F., into steam at 70 pounds pressure.

The "unit of evaporation" being equal to 965.7 thermal units, the commercial horse-power=34,488×965.7 =33,305 thermal units.

In order, however, to exhibit the extent to which he may work up such details, and to present the views of the members of the committee more fully, both in matters in which they agree and in those in which differences of views exist, an appendix is added to the report, in which memoranda written out by them are given describing details of work more fully than they are given in the code, and expressing individual opinions in regard to such matters as have seemed to each of such importance as to demand special notice. Each of these notes is signed with the initials of the writer.

Respectfully submitted:

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CHAS. E. EMERY,
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Committee.

CODE OF RULES FOR BOILER TESTS.

PRELIMINARIES TO A TEST.

I. *In preparing for* and conducting trials of steam-boilers, the specific object of the proposed trial should be clearly defined and steadily kept in view. (Appendix 1.)

II. *Measure and record the dimensions*, position, etc., of grate and heating surfaces, flues and chimneys, proportion of air space in the grate surface, kind of draught, natural or forced.

III. *Put the boiler in good condition.* Have heating surface clean inside and out, grate bars and sides of furnace free from clinkers, dust and ashes removed from back connections, leaks in masonry stopped, and all obstructions to draught removed. See that the damper will open to full extent, and that it may be closed when desired. Test for leaks in masonry by firing a little smoky fuel and immediately closing damper. The smoke will then escape through the leaks.

IV. *Having an understanding with the parties* in whose interest the test is to be made as to the character of the coal to be used. The coal must be dry, or, if wet, a sample must be dried carefully and a determination of the amount of moisture in the coal made, and the calculation of the results of the test corrected accordingly.

Wherever possible, the test should be made with standard coal of a known quality. For that portion of the country east of the Allegheny Mountains good anthracite egg coal or Cumberland semi-bituminous coal may be taken as the standard for making tests. West of the Allegheny Mountains and east of the Missouri River, Pittsburgh lump coal may be used*

V. *In all important tests* a sample of coal should be selected for chemical analysis.

VI. *Establish the correctness of all apparatus* used in the test for weighing and measuring. These are:

1. Scales for weighing coal, ashes and water.
2. Tanks, or water meters for measuring water. Water meters as a rule should only be used as a check on other measurements. For accurate work, the water should be weighed or measured in a tank. (Appendix VI. and VII.)
3. Thermometers and pyrometers for taking temperatures of air, steam, feed-water, waste gases, etc. (Appendix X. to XIII.)
4. Pressure gauges, draft gauges, etc. (Appendix IX, XIV, and XV.)

VII. *Before beginning a test*, the boiler and chimney should be thoroughly heated to their usual working temperature. If the boiler is new, it should be in continuous use at least a week before testing, so as to dry the mortar thoroughly and heat the walls.

VIII. Before beginning a test, the boiler and connections should be free from leaks, and all water connections, including blow and extra-feed pipes, should be disconnected or stopped with blank flanges, except the particular pipe through which water is to be fed to the boiler during the trial. In locations where the reliability of the power is so important that an extra feed pipe must be kept in position, and in general when for any other reason water pipes other than the feed pipes cannot be disconnected, such pipes may be drilled so as to leave openings in their lower sides, which should be kept open throughout the test as a means of detecting leaks, or accidental or unauthorized opening of valves. During the test the blow-off pipe should remain exposed.

If an injector is used it must receive steam directly from the boiler being tested, and not from a steam pipe, or from any other boiler.

See that the steam pipe is so arranged that water of condensation cannot run back into the boiler. If the steam pipe has such an inclination that the water of condensation from any portion of the steam-pipe system may run back into the boiler, it must be trapped so as to prevent this water getting into the boiler without being measured.

STARTING AND STOPPING A TEST.

IX. A test should last at least ten hours of continuous running, and twenty-four hours whenever practicable. The conditions of the boiler and furnace in all respects should be, as nearly as possible, the same at the end as at the beginning of the test. The steam pressure should be the same, the water level the same, the fire upon the grates should be the same in quantity and condition, and the walls, flues, etc., should be of the same temperature. To secure as near an approximation to exact uniformity as possible in conditions of the fire and in temperatures of the walls and flues, the following method of starting and stopping a test should be adopted:

X. *Standard Method.*—Steam being raised to the working pressure, remove rapidly all the fire from the grate, close the damper, clean the ash pit, and as quickly as possible start a new fire with weighed wood and coal, noting the time of starting the test and the height of the water level while the water is in a quiescent state, just before lighting the fire.

At the end of the test, remove the whole fire, clean the grates and ash pit, and note the water level when the water is in a quiescent state; record the time of hauling the fire as the end of the test. The water level should be as nearly as possible the same as at the beginning of the test. If it is not the same, a correction should be made by computation, and not by operating pump after test is completed. It will generally be necessary to

* These coals are selected because they are about the only coals which contain the essentials of excellence of quality, adaptability to various kinds of furnaces, grates, boilers, and methods of firing, and wide distribution and general accessibility in the markets.

regulate the discharge of steam from the boiler tested by means of the stop valve for a time while fires are being hauled at the beginning and at the end of the test, in order to keep the steam pressure in the boiler at those times up to the average during the test.

XI. *Alternate Method.*—Instead of the Standard Method above described, the following may be employed where local conditions render it necessary.

At the regular time for slicing and cleaning fires have them burned rather low, as is usual before cleaning, and then thoroughly cleaned; note the amount of coal left on the grate as nearly as it can be estimated; note the pressure of steam and the height of the water level—which should be at the medium height to be carried throughout the test—at the same time; and note this time as the time of starting the test. Fresh coal, which has been weighed, should now be fired. The ash pits should be thoroughly cleaned at once after starting. Before the end of the test the fires should be burned low, just as before the start, and the fires cleaned in such a manner as to leave the same amount of fire, and in the same condition, on the grates as at the start. The water level and steam pressure should be brought to the same point as at the start. The water level and steam pressure should be brougnt to the same point as at the start, and the time of the ending of the test should be noted just before fresh coal is fired.

DURING THE TEST.

XII. *Keep the Conditions Uniform.*—The boiler should be run continuously, without stopping for meal times or for rise or fall of pressure of steam due to change of demand for steam. The draught being adjusted to the rate of evaporation or combustion desired before the test is begun, it should be retained constant during the test by means of the damper.

If the boiler is not connected to the same steam-pipe with other boilers, an extra outlet for steam with valve in same should be provided, so that in case the pressure should rise to that at which the safety valve is set, it may be reduced to the desired point by opening the extra outlet, without checking the fires.

If the boiler is connected to a main steam-pipe with other boilers, the safety valve on the boiler being tested should be set a few pounds higher than those of the other boilers, so that in case of a rise in pressure the other boilers may blow off, and the pressure be reduced by closing their dampers, allowing the damper of the boiler being tested to remain open, and firing as usual.

All the conditions should be kept as nearly uniform as possible, such as force of draught, pressure of steam, and height of water. The time of cleaning the fires will depend upon the character of the fuel, the rapidity of combustion, and the kind of grates. When very good coal is used, and the combustion not too rapid, a ten-hour test may be run without any cleaning of the grates, other than just before the beginning and just before the end of the test. But in case the grates have to be cleaned during the test, the intervals between one cleaning and another should be uniform.

XIII. *Keeping the Records.*—The coal should be weighed and delivered to the firemen in equal portions, each sufficient for about one hour's run, and a fresh portion should not be delivered until the previous one has all been fired. The time required to consume each portion should be noted, the time being recorded at the instant of firing the first of each new portion. It is desirable that at the same time the amount of water fed into the boiler should be accurately noted and recorded, including the height of the water in the boiler, and the average pressure of steam and temperature of feed during the time. By thus recording the amount of water evaporated by successive portions of coal, the record of the test may be divided into several divisions, if desired, at the end of the test, to discover the degree of uniformity of combustion, evaporation and economy at different stages of the test. (Appendix II. and III.)

XIV. *Priming Tests.*—In all tests in which accuracy of results is important, calorimeter tests should be made of the percentage of moisture in the steam, or of the degree of superheating. At least ten such tests should be made during the trial of the boiler, or as many as to reduce the probable average error to less than one per cent., and the final records of the boiler test corrected according to

the average results of the calorimeter tests.

On account of the difficulty of securing accuracy in these tests, the greatest care should be taken in the measurements of weights and temperatures. The thermometers should be accurate to within a tenth of a degree, and the scales on which the water is weighed to within one hundredth of a pound. (Appendix XVII. to XXI.

XV. ANALYSES OF GASES.—MEASUREMENT OF AIR SUPPLY, ETC.

In tests for purposes of scientific research, in which the determination of all the variables entering into the test is desired, certain observations should be made which are in general not necessary in tests for commercial purposes. These are the measurement of the air supply, the determination of its contained moisture, the measurement and analysis of the flue gases, the determination of the amount of heat lost by radiation, of the amount of infiltration of air through the setting, the direct determination by calorimeter experiments of the absolute heating value of the fuel, and (by condensation of all the steam made by the boiler) of the total heat imparted to the water.

The analysis of the flue gases is an especially valuable method of determining the relative value of different methods of firing, or of different kinds of furnaces. In making these analyses great care should be taken to procure average samples—since the composition is apt to vary at different points of the flue—(Appendix XVI.)—and the analyses should be intrusted only to a thoroughly competent chemist, who is provided with complete and accurate apparatus.

As the determinations of the other variables mentioned above are not likely to be undertaken except by engineers of high scientific attainments, and as apparatus for making them is likely to be improved in the course of scientific research, it is not deemed advisable to include in this code any specific directions for making them.

XVI. RECORD OF THE TEST.

A "log" of the test should be kept on properly prepared blanks, containing headings as follows:

Time.	Pressures.			Temperatures.					Fuel.		Feed Water.	
	Barometer.	Steam Gauge.	Draught Gauge.	External Air.	Boiler Room.	Flue.	Feed Water.	Steam.	Time.	lbs.	Time.	lbs. or cubic ft.

XVII. REPORTING THE TRIAL.

The final results should be recorded upon a properly prepared blank, and include as many of the following items as are adapted for the specific object for which the trial is made. The items marked with a * may be omitted for ordinary trials, but are desirable for comparison with similar data from other sources.

Results of the trials of a..

boiler at...

To determine..

1. Date of trial			
2. Duration of trial	hours.		
DIMENSIONS AND PROPORTIONS.			
Leave space for complete description. (See Appendix XXIII.)			
3. Grate surface......wide......long......area	sq. ft.		
4. Water heating surface	sq. ft.		
5. Superheating surface	sq. ft.		
6. Ratio of water heating surface to grate surface			
AVERAGE PRESSURES.			
7. Steam pressure in boiler, by gauge	lbs.		
*8. Absolute steam pressure	lbs.		
*9. Atmospheric pressure per barometer	in.		
10. Force of draught in inches of water	in.		
AVERAGE TEMPERATURES.			
*11. Of external air	deg.		
*12. Of fire-room	deg.		
*13. Of steam	deg.		
14. Of escaping gases	deg.		
15. Of feed-water	deg.		
FUEL.			
16. Total amount of coal consumed†	lbs.		
17. Moisture in coal	per cent.		
18. Dry coal consumed	lbs.		
19. Total refuse, dry..........pounds =	per cent.		
20. Total combustible (dry weight of coal, Item 18, less refuse, Item 19)	lbs.		
*21. Dry coal consumed per hour	lbs.		
*22. Combustible consumed per hour	lbs.		
RESULTS OF CALORIMETRIC TESTS.			
23. Quality of steam, dry steam being taken as unity			
24. Percentage of moisture in steam	per cent.		
25. No. of degrees superheated	deg.		
WATER.			
26. Total weight of water pumped into boiler and apparently evaporated‡	lbs.		
27. Water actually evaporated, corrected for quality of steam§	lbs.		
28. Equivalent water evaporated into dry steam from and at 212° F.	lbs.		
*29. Equivalent total heat derived from fuel in British thermal units§	B. T. U.		
30. Equivalent water evaporated into dry steam from and at 212° F. per hour	lbs.		
ECONOMIC EVAPORATION.			
31. Water actually evaporated per pound of dry coal, from actual pressure and temperature§	lbs.		
32. Equivalent water evaporated per pound of dry coal from and at 212° F.	lbs.		
33. Equivalent water evaporated per pound of combustible from and at 212° F.	lbs.		
COMMERCIAL EVAPORATION.			
34. Equivalent water evaporated per pound of dry coal, with one-sixth refuse, at 70 pounds gauge pressure, from temperature of 100° F.=Item 33 multiplied by 0.7249	lbs.		
RATE OF COMBUSTION.			
35. Dry coal actually burned per sq. foot of grate surface per hour	lbs.		
*36. Consumption of dry coal per hour. Coal assumed with one-sixth refuse.§ Per sq. ft. of grate surface	lbs.		
*37. Consumption of dry coal per hour. Coal assumed with one-sixth refuse.§ Per sq. ft. of water heating surface	lbs.		
*38. Consumption of dry coal per hour. Coal assumed with one-sixth refuse.§ Per sq. ft. of least area for draught.	lbs.		

RATE OF EVAPORATION.			
39. Water evaporated from and at 212° F. per sq. ft. of heating surface per hour..................................	lbs.		
*40. Water evaporated per hour from temperature of 100° F. into steam of 70 pounds gauge pressure.§ — Per sq. ft. of grate surface.....	lbs.		
*41. Water evaporated per hour from temperature of 100° F. into steam of 70 pounds gauge pressure.§ — Per sq. ft. of water heating surface...................... .	lbs.		
*42. Water evaporated per hour from temperature of 100° F. into steam of 70 pounds gauge pressure.§ — Per sq. ft. of least area for draught....................	lbs.		
COMMERCIAL HORSE-POWER.			
43. On basis of thirty pounds of water per hour evaporated from temperature of 100° F. into steam of 70 pounds gauge pressure, (=34½ lbs. from and at 212°)§.......	H. P.		
44. Horse-power, builders' rating at....square feet per horse-power	H. P.		
45. Per cent. developed above, or below, rating§.......	per cent.		

* See reference in paragraph preceding table.

† Including equivalent of wood used in lighting fire. 1 pound of wood equals 0.4 pound coal. Not including unburnt coal withdrawn from fire at end of test.

‡ Corrected for inequality of water level and of steam pressure at beginning and end of test.

§ The following shows how some of the items in the above table are derived from others:

Item 27 = Item 26 × Item 23.
Item 28 = Item 27 × Factor of evaporation.

Factor of evaporation $= \frac{H-h}{965.7}$, H and h being respectively the total heat units in steam of the average observed pressure and in water of the average observed temperature of feed, as obtained from tables of the properties of steam and water.

Item 29 = Item 27 × $(H-h)$.
Item 31 = Item 27 ÷ Item 18.
Item 32 = Item 28 ÷ Item 18 or = Item 31 × Factor of evaporation.
Item 33 = Item 28 ÷ Item 20 or = Item 32 ÷ (per cent. 100−Item 19).
Items 36 to 38. First term = Item 20 × [illegible].
Items 40 to 42. First term = Item 39 × 0.8698.

Item 43 = Item 29 × 0.00003 or $= \frac{\text{Item 30}}{34\frac{1}{2}}$.

Item 45 $= \frac{\text{Difference of Items 43 and 44}}{\text{Item 44.}}$

ON THE RESISTANCE OF LOCOMOTIVES AND TRAINS—THE EFFICIENCY OF LOCOMOTIVES AND THEIR CONSUMPTION OF WATER AND FUEL.

By Professor A. FRANK.

Translated from "Organ für die Fortschritte des Eisenbahnwesens," for Abst. of the Inst. of Civil Engineers.

The first is a theoretical paper, but entirely founded on practical experiments. The author refers first to the formulas given by Messrs. Guillemin, Guebhard and Dieudonné, in their work on the resistance of trains (Paris, 1868,) and by Von Röckl, of the Bavarian State Railways, in the "Organ für die Fortschritte des Eisenbahnwesens" for 1880. The formulas of the former authors are as follows:—

1*a*. For goods trains with speeds of 12 to 32 kilos per hour, and oil lubrication.

$$r=1.65+0.05\ V.$$

2*a*. Passenger trains with speeds of 32 to 50 kilometers per hour.

$$r=1.8+0.08\,V+\frac{0.009\ F\ V^2}{Q}.$$

3*a*. Passenger trains with speeds of 50 to 65 kilometers per hour.

$$r = 1.8 + 0.08\,V + \frac{0.006\,F\,V^2}{Q}$$

4*a* Express trains with speeds of 70 kilometers per hour and upwards.

$$r = 1.8 + 0.14\,V + \frac{0.004\,F\,V^2}{Q}.$$

Here V is the speed in kilometers per hour, F is the end section of the carriages (taken at 5 square meters), Q is the total weight of the train, excluding engine and tenders in tonnes, and r is the resistance in kilograms per tonne. For locomotives the results of experiments were given, but no formula was developed.

Von Röckl's formulas are as follows:—

1*b*. For Locomotives,

$$k_1 = 0.005 \ + 0.00000021\ V^2.$$

2*b*. For Carriages,

$$k_2 = 0.0025 + 0.00000021\ V^2.$$

Here V is the speed in kilometers per hour, and k_1 k_2 are the resistances in kilograms per kilogram of gross weight.

These formulas all refer to a straight horizontal track; on a curve the coefficients in 1*b* and 2*b* are to be increased by k_3, where

$$k_3 = \frac{0.6504}{R - 55}.$$

Here R is the radius of the curve in meters.

The writer first considers the case of a locomotive and tender, without a train, running on a straight horizontal road, not under steam and so slowly that the resistance of the air may be neglected. The sources of resistance are then,

1. The sliding friction of all the moving parts;
2. The rolling friction between the wheels and the rails.

The amount of the former depends chiefly on the load, on the distance moved by the sliding parts as compared with the engine as a whole, and on the coefficient of friction for the sliding pressures. It is greatly influenced by the nature of the lubricants and the care in lubrication. It is less as the wheel-diameter is larger, and is generally less, therefore, in passenger than goods engines. On the whole it may be expressed by m Q, where Q is the weight, and m a coefficient determined by experiment.

If the locomotive be now supposed to be under steam, there will be a further resistance, due to the increased friction of the slides, etc. This will be independent of the speed, and may be denoted by S. If the speed is increased, the resistance of the air must be taken into account; this will be represented by the following equation:

$$p = l\ F\ V^2,$$

where F is the surface meeting the air, and l a coefficient determined by experiment. Next must be considered the resistance arising from unevenness in the road; this will produce alternate risings and fallings of the wheels, bendings of the springs, and variations in the friction of the axles, &c. Assuming that the vertical motion varies as the square of the speed, the resistances thus produced will vary in the same proportion. The same will be true of the resistances occasioned by horizontal oscillations. Finally there may be shocks due to flat places in the tires, or in elastic portions of the rails. The resistances thus occasioned may also be taken to vary as the square of the speed. The same will apply exactly to the case of a carriage or wagon, except as relates to the resistance occasioned by the steam. Finally the increase in this latter resistance, due to the engine having a train attached, may be considered proportional to the total work to be done. If, then, this increase be denoted by the letter i, the final equation for the resistance W of a complete train will be of the form

$$W = (1 + i)\ (m\,Q + S + B\,V^2)$$

This equation does not agree with any of those given at the beginning. Those of Guillemin contain a term involving V, which is the reason why they are obliged to give different formulas for different speeds; but if V be put equal to nothing, values will be obtained for the resistance (neglecting that of the air) which are much too small, and which moreover are less for goods than for passenger trains. On the other hand Von Rockl's experiments contain no term varying as V, but have a term varying as V^2. This cannot be correct, as neither the resistance of the air nor that due to shocks varies in this ratio. Omitting this term the weight of resistance for locomotives is 0.005, and for carriages 0.0025. The latter value agrees well with the author's experiments and with an earlier formula by Harding. The former

is much higher than the author's value, who found that a passenger-engine would begin to move on a slope of 0.32 per 100 and a goods-engine on one at 0.39 per 100. To show the weakness of these formulas the writer takes an actual example, in which the value of the resistance was only 652 kilograms, whilst Von Rockl's formulas would give 3,752 kilograms.

The mode of conducting the experiments must therefore be considered. Guillemin employed two methods. In the first, the engine or carriage was brought up to a given speed, and then allowed to run till it stopped. The resistance was then calculated from the following formula:—

$$(M+m)\frac{V^2}{2}=W\,s,$$

where M is the mass of the train, m a quantity depending on the mass of the wheels, V the initial speed, s the distance run, and W the mean resistance. This method, however, was found to produce great discrepancies and difficulties, and was soon abandoned. The other method was to use a dynamometer placed between the tender and the train behind it; but this is defective, because the pressure of the air on the hauling engine is not taken into account, and because at high speeds the vibrations of the dynamometer make its readings inaccurate. Von Rockl's experiments were specially devoted to curves, and were made upon six different curves, laid near the central station at Munich. Beside each line was an electric wire, with a current breaker at every 20 meters, connected with a chronograph. The train was launched upon the curve at a known speed, and the time of passing each successive 20 meters was taken. Such a method may give results fairly applicable to cases of shunting, but not to the ordinary running of trains as hauled by locomotives; and is also subject to the difficulties which attended Guillemin's first method. The author's method was to place the locomotive or train on a straight incline of 1 in 200, and allow it to run down from a fixed initial velocity. The weight of the train then acted to accelerate, and the friction, air-resistance, &c., to retard the speed; which therefore increased up to a certain limit and then became constant. This constant speed was about 13.5 meters per second for trains consisting of a locomotive with tender and 5 to 7 wagons; 10.8 for passenger-locomotives alone; and 8.5 for goods-locomotives alone. With goods-trains it varied according to the number and weight of the wagons, but was not allowed to exceed 12.5 meters per second. The experiments were made on an incline of about 9,000 meters long, with only a short horizontal length in the middle. It is evident that, when at the constant speed, the resistance is equal to the component of the weight, the train not being under steam. This resistance can thus be easily calculated, and inserted in the equation found above, viz.—

$$W=m\,Q+B\,v^2.$$

Since W and Q are known, only one of the values m and B is required to be known in order to determine the other.

A mathematical investigation is given, by means of which the problem can be fully worked out, if the train moves upon a straight incline. To this must be added the effect due to curves, where such occurs, and for this purpose the writer uses the coefficient given above, as determined by Von Röckl from more than 2,000 experiments. The mode of introducing this coefficient into the equation is investigated, and then is found a complete practical formula, which can be applied to the author's experiments. The actual mode of taking these was to bring the engine or train up to the proper speed by its own steam, and at about 100 meters before the first point of observation, to shut off the steam and throw the lever to mid-gear. The time of traversing each 500 meters was then carefully noted, and the mean speed taken therefrom. The largest number of experiments was made with a passenger-engine "Fuse." The earlier ones were made when the engine had been some time at work. It was then taken into the shops and the bearings changed. The result was to produce a considerable increase in the resistance, and diminution in the constant speed generally attained. This speed, however, was found to increase steadily in subsequent experiments, until it nearly reached its original value. Tables and diagrams with this and with other engines and trains are given, and also full particulars of the engines themselves. The results on the whole were uniform, but with some variations, arising

mainly from variations in the wind. In order to compare these results with the author's formulas, an average experiment was taken, and the curves of speed calculated according to the author's formulas, using the known elements of weight, velocity, &c. These curves are found to agree very closely with those plotted from the experimental results.

The mode of working out mathematically, from the results of the experiments, the values of the coefficient, m, is then given at length. On the whole it appears that for four-coupled passenger-engines $m=0.0032$; for six-coupled goods-engines $m=0.0038$; and for carriages and wagons $m=0.0025$. These values are to be used in the following equation, which represents the resistance of a train in the most general manner.

$$W=Q_1 m_1+Q_2 m_2 \pm (Q_1+Q_2)$$
$$\sin. a+(Q_1+Q_2)\frac{0.6504}{R-55}+l(F_1+F_2)v^2.$$

Here Q_1 is the weight of the engine and tender, Q_2 the weight of the train, a the angle of inclination of the track, R the radius of the curve, v the speed, m_1 m_2 the coefficients of resistance for the engine or train, l a coefficient for wind-resistance (which may be taken as 0.1225), F_1 the end-section of the train (which may be taken at 8 square meters), and F_2 the whole surface exposed to the wind, by all the vehicles of the train together. This latter figure depends of course upon the size and character of each vehicle, and upon the extent to which it is sheltered by the vehicle in front of it. It can only be calculated empirically, and the writer gives the following figures as the allowance of surface he has found best to correspond with his results.

	Square Meter.
For luggage vans	1.7
" carriages and covered goods wagons.	0.5
" open wagons, loaded	0.4
" " empty	1.0
" carriages or covered wagons following an open wagon	1.0

All these quantities are expressed in meters or kilograms, and it is assumed that the lubrication throughout is with oil. To test this equation the author applies it to several of the experiments recorded by Guillemin, and finds the correspondence to be exceedingly close.

The writer was also employed by the railways of Alsace-Lorraine to make a series of experiments, with the view of determining the consumption of fuel on their locomotives. For this purpose it was resolved to ascertain the amount of water evaporated per kilogram of coal, and then to measure accurately the water used both with locomotives alone and complete trains. For this purpose gauge-glasses were fixed on each side of the locomotive and of the tender, and so arranged that the water-level could be accurately taken, and thus the consumption determined. The waste water from the injector was also collected and measured. As the water evaporated represents a definite quantity of work done, this gave an opportunity of comparing the effective work of engines under various circumstances in comparison with the water used.

The resistances to an engine when running without steam have been considered elsewhere. When the engine is under steam, these are increased by the facts that the slide-valves work under the steam-pressure, and that this pressure is also brought upon the eccentric straps, connnecting-rods, ends, &c. Let p be the mean pressure in the valve-chest, o the area of the valve, and f the coefficient of friction, then the friction of the valve is $p\,o\,f$. Let s be the travel of the valve per revolution, and D the diameter of the driving-wheel; then the work done on the two valves per meter run is given by

$$S_1=\frac{2\,p\,o\,f\,s}{D\pi}.$$

The thrust $p\,o\,f$ needed to move the valves is taken by the two eccentric-straps. Let E be the diameter of these, and f_1 the coefficient of friction; then the work done on friction in the two eccentric-straps per meter run is given by—

$$S_2=\frac{2\,p\,o\,f\,f_1\,E}{D}.$$

With regard to the driving gear, there has to be considered the friction between the cross-head and the slide-bars between the cross-head and the connecting-rod, between the connecting rod and the crank-pin, and between the coupling-rods and their pins. Let r be the radius of the crank, l the length of the piston-rod, d_1 the diameter of the cross-head journal, d_2 that of the connecting-rod bearing, and d_3

that of the coupling-rod bearing, f the coefficient of friction for longitudinal motion, f_1 ditto for rotary motion, P the mean pressure on the piston. Then there result the following equations for the work done per revolution of the driving-wheel:—

Friction between cross-head and slide-bars,
$$L_1=\frac{f P r^2 \pi}{l};$$

Friction between cross-head and connecting-rod, $L_2=2 f P d_1 \frac{r}{l}$;

Friction between connecting-rod and crank-pin, $L_3=f_1 P d_3 \pi$.

For the friction of the coupling-rods distinction must be made between four-coupled and six-coupled engines. In the first, half the pressure, or $\frac{P}{2}$, comes on each coupled axle; and therefore,
$$L_4=f_1 P d_3 \pi.$$

In the second, one-third of the pressure only comes on each coupled axle, and the sum of these pressures on the driving-axle. If d_4 is the diameter of the bearing for the latter,
$$L_4=\tfrac{4}{3} f_1 P \pi (d_3+d_4).$$

Dividing the sum of these four values (L_1 L_2 L_3 L_4) by the total work, $L=4rP$, the work done in friction can be compared with the whole work done on the engine. The various symbols used must of course have the values belonging to the special engine considered. For a particular six-coupled goods-engine, the writer calculates that the work thus done on friction due to the steam, is only about 4 per cent. Adding this to the work required for overcoming other resistances, as described in the former abstract, the author obtains a general expression for the work done by an engine in taking a train between any two stations at a given speed, over any given curves, and with any number of stoppages, &c. Numerous experiments were made, both with goods and passenger-engines, to test this expression; the coefficient of resistance m having been determined as in the former abstract. The length of the run and the weight of water evaporated were always taken, and thus the work done per kilogram of water could be determined.

Some of the results are given in the following Table:

TABLE OF RESULTS.

Number of Experiments.	Kind of Engine.	Vehicles in Trains, as under— Carriages.	Van.	Covered Wagons.	Empty Trucks.	Loaded Trucks.	Water Expended.	Average Speed.	Work Done.	Work per Kilogram of Water.
							Kilograms per sec.	Meters per sec.	H.P.	Kilogram-meters.
	Passenger..									
1	"	—	—	—	—	—	0.399	7.06	80.5	15,166
2	"	—	—	—	—	—	0 406	6.76	85.4	15,745
3	"	—	—	—	—	—	0.244	10.83	44.0	13,547
4	"	—	—	—	—	—	0.273	10.50	55.5	15,245
5	"	—	—	—	—	—	0.278	11.48	57.6	15,525
7	"	—	—	—	—	—	0.375	13.20	76.0	15,199
9	"	—	—	—	—	—	0.486	16.60	118.1	18,214
11	"	—	—	—	—	—	0.557	18.20	139.2	18,723
13	"	—	—	—	—	—	0.528	18.20	138.5	19,651
14	"	—	—	—	—	—	0.590	18.40	156.6	19,926
16	"	6	1	—	—	—	0.543	12.03	136.8	18,877
17	"	6	1	—	—	—	0.959	17.07	244.1	19,081
1	Goods......	—	—	—	—	—	0.280	8.60	46.46	12,420
2	"	—	—	—	—	—	0.274	8.88	57.66	15,786
3	"	—	—	—	—	—	0 287	9 54	51.77	13,519
4	"	—	—	—	—	—	0.341	9.78	65.35	14,382
6	"	—	1	9	19	4	0.491	8.00	171.70	26,253
7	"	—	1	—	—	41	0.560	7.04	179.30	24,014
8	"	—	1	—	—	41	0.566	7.26	189.80	25,126

The experiments embrace as will be seen, very different conditions; the speed with the passenger-engine varying from 7.06 meters per second to 18.4. and the power expended from 44 to 244 H. P.; and in the goods-engine to nearly the same extent.

The three last columns of the tables enable a law to be laid down on the relation of effective work to water expended. Let W be the water expended in kilograms per second, and N the H. P. expended in overcoming resistance. Take the first as ordinates, and the latter as abscisses, and plot the results of the experiments. It will be found that they approximate to two straight lines; that for the passenger-engine corresponding to the equation

$$W=\frac{N}{300}+0.1,$$

and that for the goods-engine to the equation

$$W=\frac{N}{500}+0.18.$$

They give the same water expended for 60 H. P. (viz. 0.3); for greater values of N the goods-engine shows the better results; for less values the passenger-engine.

If x be put for the effective work in kilogrammeters per kilogram of steam evaporated, there is obtained the relation (since 1 H. P. =75 kilogrammeters)

$$x\,W=75\,N.$$

Whence result, using the two formulas above, the two following equations—

$$\text{For passenger-engines } x=\frac{22{,}500\ N}{N+30};$$

$$\text{For goods-engines } \quad x=\frac{37{,}500\ N}{N+90}.$$

From these formulas may be shown clearly how small is the influence of the speed on the useful effect, and how this effect increases with the work done per second.

Of course at very low speeds the cooling of the cylinder would diminish the useful effect, and at very high speeds the loss of pressure in the cylinder, and the quantity of priming water, would have a similar tendency. But, in practice, these elements are not greater than the ordinary errors of observation. Thus in Nos. 13 and 16 of the Table, where N=138.5 and 136.8, the water expended is 0.528 and 0.543 respectively, or very nearly equal; whilst the speed is 18.2 in the one case, and 12.03 in the other. The above equations may therefore be used quite independently of the speed. With their help it is easy to calculate the work done, and water expended, in sending a known train at a known speed over any particular length of line, on which the curves and inclines are given. Knowing the water expended, the fuel burnt is easily obtained, since the relation between them remains almost constant as long as the quality of the fuel and the conditions do not change. Elaborate experiments made by Wöhler in 1879, and published in the "Centralblatt für Bauverwaltung," 1882, show that, on an average, the weight of water evaporated is seven times the weight of coal burnt.

From these results, the conclusion is drawn that it is desirable to give to each engine the heaviest train it will draw, not only because it is thereby better employed, but because (as seen by the form of the equations above) the useful effect increases with the work done. Of course the engine must not be overloaded, so as to cause too much priming, &c. Again, a succession of rising and falling gradients have a very small effect on the work done, provided that the speed in descending has not to be checked by brakes. Of course, however, the load to be put on an engine must be calculated with reference to the steepest gradient it has to surmount, and the maximum resistance it may encounter on that gradient. The resistances due to curves and gradients respectively are known, and in each railway a maximum value for the sum of these resistances should be assumed, taking the place of what is now called the ruling gradient.

Again, the result that the useful effect is practically independent of speed has important consequences. As the speed diminishes, the resistance of the air diminishes in a much higher proportion, and the tractive power at the same time increases. Hence, lowering the speed is very effective in overcoming great resistances, although a limit is of course set to this by the limited adhesive force between tires and rails. Taking the coefficient of friction as 1 by 7, the limit of tractive force for the goods-engine in the above experiments was 5,500 kilograms, and for the passenger-engine 3,143 kilograms.

The greatest power of the two engines was, for the former, 25,500 kilogram-meters per second, and for the latter 21,000. Dividing these quantities by the former, the minimum speed for the goods-engine is 4.5 meters per second, and for the passenger-engine, 6.7. This minimum speed being fixed, and the other elements of the question known, it is easy to find an equation for the greatest number of vehicles that can be hauled by a given engine over a given line. The results are worked out in the following Table, in which the gradients are those on a straight road, and must be lowered proportionately if there is a curve at the same place.

Vehicle.	Minimum Speed.	Maximum Gradient.							
		$\frac{1}{500}$	$\frac{1}{400}$	$\frac{1}{300}$	$\frac{1}{200}$	$\frac{1}{150}$	$\frac{1}{100}$	$\frac{1}{80}$	$\frac{1}{50}$
	Meters per sec.								
Carriages, express trains.......	14.0	—	—	11.3	8.2	6.1	3.3	2.0	—
" ordinary "	10.0	—	—	22.0	16.3	12.8	8.2	6.1	2.5
Covered goods-wagons, loaded.	4.5	62.7	56.2	48.1	36.5	29.2	20.6	16.6	10.0
" " empty.	4.5	—	—	—	—	67.5	47.8	38.6	23 2
Open wagons, loaded.........	4.5	73.3	65.6	56.1	42.6	34.1	24 1	19.4	11.6
" empty..........	4.5	—	—	—	—	—	69.8	56.6	34.2

In the case of a level road, the same equations give the following values for the numbers of vehicles that can be hauled at given speeds.

——	Speed.	Number of Vehicles.
	Meters per sec.	
Carriages, express.......	20.0	8 0
" ordinary......	16.0	18.8
Covered wagons, loaded.	7.5	61.7
Open wagons, loaded....	7.5	72.5

These figures agree well with experience, and show how rapidly the admissible load diminishes as the speed and gradients are increased; and also the great importance of noting whether the wagons are loaded or empty. Although taken from particular experiments, the equations are general, and may be used for solving any questions upon the movement of trains over railways.

THE FIGURE OF THE EARTH.

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SECTION I.—HISTORICAL.

THE progress of the science of astronomy is the continued triumph of the powers of the intellect over the first erroneous conceptions of the senses; and its history is so allied to that of the human mind that we cannot help feeling a strong inclination to know at what time and by what people the hypotheses upon which the science is based were first advanced.

The value of an hypothesis is estimated by the number of difficult phenomena it explains. From this standpoint few suppositions can be found to have been more important than that which assigned to the earth its approximate figure and magnitude.

Little is known of the early history of this hypothesis, for it is enveloped in those dark ages of antiquity when the revolutions of empires were imperfectly recorded, not to speak of the calm speculations of quiet and thoughtful men.

To its first inhabitants the earth must have appeared as an extended fixed plane, the extremities of which apparently supported the vast dome of the heavens.

Among the ancients the prevailing opinion was that the surface of the earth was flat, that the visible horizon was the boundary of the earth, and the ocean the boundary of the horizon; that the earth and heavens were the whole visible universe, and that all beneath the earth was Hades.

The progress of knowledge concerning the figure of the earth has from the earliest times been closely connected with the study of astronomy. As in this latter science, first impressions are abandoned, and all conclusions are in striking contradiction to those of superficial observation, so, as man progressed, the earth became divested of its flattened shape and character of fixidity, and was shown to be a globular body turning swiftly upon its own axis, and moving through space with great rapidity.

The idea of the earth being a globe is now so familiar to us that arguments in proof of it are almost unnecessary. Yet familiar as this fact now is, many ages must have elapsed before it was universally received. So difficult was it to conceive how the inhabitants of the opposite hemisphere could exist with their heads downwards, that we find St. Augustine in the 5th century vehemently contending against the possibility of the existence of an antipodes.

Evidently ignorant of many important physical laws, the early suppositions of the ancients regarding the figure of the earth are in a great measure ludicrous. Being but imperfectly acquainted with the science of astronomy, and their observations being rude and inaccurate, they were led to base their theories upon false assumptions. It might be well to except from this sweeping statement the theories of the early Eastern astronomers. The earliest astronomical records that can be conceived of as authentic are found in China, and go back as far as 800 B. C., when we find eclipses observed and registered. This would naturally lead us to conclude that the observers were acquainted with the fact that the sun and heavenly bodies were visible to people towards the east sooner than people towards the west. As this could not occur unless the earth were curved, we may assume with almost a certainty that the globular figure of the earth was knwon in China at least 800 years B. C.

It is to be regretted that we have no record of the speculations of the early inhabitants of the eastern countries upon this subject. Astronomy was evidently cultivated as a science, and whatever may have been their suppositions as to the figure of the earth, we may suppose that at the first they were led to conclude that the heavens were spherical by observing that those stars sufficiently elevated towards the north pole performed their entire revolution around the pole without interruption; from which it might by an easy inference be concluded that the other stars, though concealed from view, pursued their course in the same manner. When once the theory of a revolving heaven was accepted, it would be comparatively an easy matter to conclude that the earth was globular.

The only records of early researches in connection with the figure of the earth come to us from Greece. The discovery that the earth is not a plane is ascribed to Thales, of Miletus, B. C., 640. Anaximander B. C. 570, Anaxagoras B. C. 460, claimed a cylindrical shape for the earth, estimating the height as three times the diameter, the land and water being on the upper base. Plato B. C. 400 called the earth a cube. Aristotle advanced the theory that the earth was spherical.

It may be well to understand at this point that when we speak of the figure of the earth, we mean the figure which would be assumed by the earth were it covered entirely with water, and more specifically water at mean sea level.* As for the inequalities on the surface of the earth owing to mountains and valleys, they are of no moment in the estimation of the general figure, being of much less account with respect to relative proportion than the asperities on the surface of an orange with regard to the orange itself. Upon an artificial globe of 6⅚ feet in diameter, Mount Chimborazo† would be represented by a grain of sand less than 1-20 of an inch in thickness.

As we have previously stated, Aristotle was the first to assume the spherical hypothesis. The active curiosity of man, however, did not rest contented with having assumed that the earth was a

* Scientifically speaking the Figure of the Earth is interpreted as meaning the mean surface of the sea imagined to percolate the continents by canals.

† 21,424 feet above mean tide.

sphere, but proceeded to ascertain the exact dimensions of the planet. In the course of his discussion Aristotle states, as does Archimedes (B. C. 250), that mathematicians estimated the circumference of the earth at 300,000 stadia.

The first approximation to the magnitude of the earth, however inaccurate, must have been at that time a most important addition to the stock of natural knowledge, and, indeed, except with a view to some very refined scientific investigation, the general idea which the ancients had of the magnitude of the earth differs but little from that of the moderns, for we are so incapable of the appreciation of number or magnitude when either exceeds a certain limit, that the difference between their results and ours makes little or no difference in the general idea which we hold as to the size of the earth.

Eratosthenes, B. C. 230, was apparently the first to conceive a method for the deduction of the length of the circumference of the earth. Although his results are probably sadly inaccurate, the method which he adopted is in essence identical with that followed at the present time.

As it is impossible for us to occupy a position from which the earth may be viewed as a whole and compared with some standard of measure, we are compelled to resort to geometrical principles in the determination of its figure and magnitude. The problem is rendered more difficult by the fact of there being no fixed landmarks or standard lines upon the surface of the earth indicating aliquot parts of the earth's circumference.

It therefore becomes necessary to refer our situation on the earth to objects external to our own planet. Such marks are afforded by the heavenly bodies.

By observations of the meridian altitudes of stars, and from their known polar distances, we determine the altitude of the pole-star, which is also the latitude of the place at which the observations are made.

Let us suppose then, that we wish to determine the length, on the surface, of one degree of the earth's circumference. Let us suppose also that we know the distance between two places on the *same* meridian. Then having determined the latitudes of the two places, their difference may be taken as representing the angle at the center of the earth corresponding to the measured distance on the surface. Dividing the distance by the angle we find the length of a meridian arc equivalent to one degree of latitude, and this multiplied by 360 gives us the length of the earth's circumference.

Where local difficulties compel the observers to deviate, in the measurement of the distance, from the line of the meridian, the amount of the deviation must be noted. A very simple calculation will enable us to reduce the measured distance to the corresponding length on the meridian. It seems hardly necessary to add that this measurement must be made with the greatest care and accuracy, for an error in the measured length of one degree is multiplied 360 times in the circumference and nearly 58 times in the deduced radius of the earth.

Such in its simplest form is the geodetic operation called the measurement of an arc of a meridian, and is in essence the method employed by Eratosthenes. He knew that at Syene, in S. Egypt, on the day of the summer solstice at mid-day, objects cast no shadows, whence he concluded that the sun was in the zenith. In Alexandria, at the same period, he observed that the sun made an angle with the vertical of 7° 12′ or $\frac{1}{50}$th of a circumference. Assuming Alexandria to be directly north of Syene,* he concluded the length of the circumference to be 50 times the distance between these two places, or 250,000 stadia. Of course this determination was very imperfect, for with the instruments of his time he was compelled to neglect the diameter of the sun in the determination of declination. This occasioned an error of $\frac{1}{2}$° in the length of the celestial arc at Syene. The measurement of the distance between Syene and Alexandria was probably also very inaccurate.

The next attempt to solve the problem was made by Posidonius. B. C. 90. Instead of using the sun for the determination of the difference of latitude, he found the celestial arc by means of the star Canopus. At Rhodes this star when on the meridian is just visible above the horizon, while at Alexandria its meridian

* The error of this assumption was about three degrees.

altitude is 7° 30′. The distance between the two places being known, he deduced 240,000 stadia as the circumference of the earth.

Ptolemy, an astronomer, A. D. 160, in his treatise on Geography, gives 500 stadia as the length of a degree. This value would give for total length of the circumference 180,000 stadia—a result widely different from any previously deduced.

Unfortunately the degree of approximation attained in these results cannot be known, as we have no value for the length of the *stadium*. It probably had different values dependent upon time and place.

In the year 819 the Caliph Almamoun caused the astronomers of Bagdad to measure an arc of the meridian on the plains of Mesopotamia by means of wooden rods. Authorities differ as to the resulting deduction of the length of a degree; some claiming that, failing in their own observations owing to insurmountable obstacles, the Arabians adopted the result of the Grecian astronomer Ptolemy. Others hold that they found for the length of a degree 56⅔ Arabian miles, or approximately 71 English miles.

From this time until the revival of letters, interest in this subject seems to have disappeared. Speculation was at a standstill for 700 years. Former theories and suppositions were forgotten and lost amid the social storms of the middle ages. Man was again ignorant of the form and dimensions of the planet which had been assigned to him in the immensity of space.

Early in the 15th century the question of the form of the earth began a second time to attract the attention of thoughtful men. The prevalent idea was that the earth was a plane. This time it was not the philosophers but the navigators who looked with doubt upon this supposition. Columbus fearlessly asserted the earth to be globular, and after the voyage of Magellan around the earth, the globular hypothesis was once more accepted. Immediately endeavors were made to determine the size of the earth. In 1525 Fernel made a determination of the length of a degree by deducing the difference of latitude between Paris and Amiens, and measuring the distance by observing the number of revolutions made by his coach wheel in traveling from one place to the other. From these observations he deduced the length of one degree to be 57,050 toises or 364,960 English feet. (The toise—an old French measure—is practically equal to 1.949 meters, or 6.3946 English feet.)

In 1617 Willebrord Snell conceived the idea of the deduction of the length of distances by means of a series of triangles measured from a known base. This was the first instance of the application of the invaluable principle of trigonometrical surveying which, since that time, has become general in all extensive surveys.

Snell measured his base line upon the frozen surface of the meadows between Leyden and Soeterwood. The angles he measured by means of a quadrant of 5½ feet radius. His result for the length of a degree was 55,020 toises, or approximately 66.63 miles.

In 1633 Norwood, in England, adopting a method similar to that of Fernel's deduced 57,424 toises for the length of a degree.

We are now brought down to the time of Picard, whose invaluable adaptation of the telescope to circular instruments for measuring angles marks an era in the progress of geodetic science. Hitherto the measurement of angles was roughly made by the use of sights similar, only much more unreliable, to those used on rifles. Picard first introduced spider lines in the focus of telescopes, whereby a far higher degree of precision in determination of the position of a distant point may be obtained by covering the point in question with the intersection of spider lines, which is so placed as to be exactly in the center line of the telescope. In his determination of the length of a degree he used the trigonometrical method, measuring twice a base line of nearly seven miles in length. His measurement is the first executed with anything like scientific precision. He even calculated the error produced by his instrument being out of the center of his station, and determined his difference of latitude by means of the zenith distance of a star in Cassiopeia measured with a sector. At this time the effect of aberration, refraction and nutation were unknown, nevertheless his result of 57,060 toises, or

nearly 69.76 miles is marvelously near that of later determinations.

The measurement of an arc of the meridian, although the most reliable, is not the only method by which the figure of the earth upon the spherical hypothesis may be deduced. One simple expedient consists in determining the dip or angle of depression of the horizon. Take, for instance, the case of a mountain near the sea coast.. Knowing the height of the mountain above the sea, and the angle of depression from its top to the horizon, we can, by an easy mathematical formula, deduce the following equation in which r is the radius of the earth, h the height of the mountain, and d the distance from the mountain to the horizon:

$$r^2=(r+h)^2-d^2.$$

This principle was applied more than 200 years ago at Mount Edgecombe, and since that time at Ben Nevis.

Or, again, we may by the application of the following proposition form a proportion from which the diameter of the earth may be found—the earth's diameter bears the same proportion to the distance of the visible horizon from the eye as that distance does to the height of the eye above the sea level.

Both these methods, of course, only furnish means of determining the size of the earth with a rough approximation. Refraction bends the visual lines out of the truly rectilinear direction, and, therefore, introduces a serious error in the result.

Up to 1690 astronomers supposed the form of the earth to be nearly that of a perfect sphere, and consequently the *length of degrees in all latitudes precisely equal.*

In 1690-1718, J. and D. Cassini published results showing that although the measures of meridional arcs made in various parts of the globe agreed sufficiently to prove that the supposition of a spherical figure is not very remote from the truth, yet exhibited discordances far greater than could be attributable to errors of observation, and which rendered it evident that the spherical hypothesis was untenable. Immediately upon this discovery, new interest was awakened in the subject. The works of previous scientists upon this subject were carefully examined, and, as a result, it became known that Picard, as early as 1671, in his work on the figure of the earth, mentions a conjecture proposed to the French Academy that, supposing the diurnal motion of the earth, heavy bodies should descend with less force at the equator than at the poles; and that, for the same reason, there should be a variation in the length of the pendulum vibrating seconds in different latitudes, for the time of oscillation of a pendulum of constant length depends upon the intensity of the force of gravity.

In the same year Richer was sent to Cayenne, in equatorial S. A., and was especially charged by the Academy to observe the length of the pendulum vibrating seconds. On his return he stated that the difference between the seconds pendulum at Paris and Cayenne was one line and a quarter, that at Cayenne being the shorter. Moreover, the clock which Richer took to Cayenne, having been adjusted to beat seconds at Paris, retarded two minutes a day at Cayenne, so that no doubt remained of the diminution of the force of gravity at the equator.* This, as it was the first direct proof of the diurnal motion of the earth, was also what led Huygens to suspect that there was a protuberance of the equatorial parts of the earth and a corresponding depression of the poles Cassini had already observed this phenomenon in the figure of Jupiter, which analogy strongly favored the supposition of a similar peculiarity in the shape of the earth.† Since, then, it was evident that the meridian section of the earth was not a circle, what was the next simplest supposition that could be made respecting the nature of the meridian. In the flattening of a round figure at two opposite points, and its protuberance at points rectangularly situated to the former, we recognize the distinguishing feature of the elliptic form. Thus mathematicians, after discarding the spherical hypothesis assumed the meridian to be an ellipse. The geometrical properties of that curve

* See Newton's *Principia*, Book III.

† The difference of the diameters of Jupiter amounts almost to 1-10th. and when we compare the exact measure of this depression, the dimensions of Jupiter and the time of his rotation, with like phenomena connected with the earth, we find for this latter planet a proportional depression of 1-888th, which is very nearly identical with the value deduced from the great French measurement.

enabled them to assign the proportion between the lengths of the axes which would correspond to any proposed rate of variation in its curvature, as well as to fix upon the absolute lengths corresponding to any assigned length of a degree in a given latitude.

Spheroids are generated by the revolution of ellipses about one or another of their axes. Every ellipse has two axes, one passing through the *foci* is called the major axis, while the other—perpendicular to the major axis at its middle point —is called the minor axis. When the ellipse revolves about its major axis it generates what is called an *oblate spheroid*, and when it revolves about its minor axis the figure generated is named a *prolate spheroid*. The *ellipticity* is the amount of variation of the form of the spheroid from a sphere of like content, or the amount of flattening at the poles. This is expressed by dividing the difference of the semi-major and minor axes by semi-major axis.

The *eccentricity* of an ellipse is equal to the distance from the center to one of its foci divided by the semi-major axis.

Huygens was the first person who attempted to determine the figure of the earth by direct calculation, but in his investigation he assumes that the whole of the attractive force resides in the center of the earth, and that its power varies as the square of the distance. This hypothesis, since the discovery of the law of universal gravitation, has been found inadmissible, and therefore his results were largely in error.

In the course of the discussion of Cassini's observation of the variation in length of the second pendulum in different latitudes and consequent diminution of the force of gravity at the equator it was claimed that this diminution might be due to the counteracting effect of the centrifugal force occasioned by the rotation of the earth. Newton* showed that even after making allowances for this effect, the difference between the force of gravity at Paris and Cayenne was too great for the spherical hypothesis, and further, upon the assumption that the earth is a homogeneous fluid, and supposing its density to be the same throughout the whole mass, and assuming that the constituent molecules attract one another in proportion to the inverse square of the distance, he demonstrated that, in consequence of rotation, the earth would assume the form of an *oblate spheroid*, whose ellipticity would amount to $\frac{1}{230}$th.

Clairaut was the first to advance a general solution of this problem adapted to the hypothesis of a variable density. He proved that, if the density of the strata of which the earth is composed increases towards the center, the ellipticity will be less than in the hypothesis of Newton, and greater than in that of Huygens; and, again, that the sum of the fraction representing the ellipticity and the fraction expressing the augmentation of gravity at the poles will always make a constant quantity, which is equal to $\frac{5}{2}$ of the fraction which expresses the proportion which exists between the centrifugal force and gravity at the equator. It is by means of this theorem we are enabled to ascertain the figure of the earth by means of pendulum experiments.

These theoretical determinations of Huygens, Newton, and Clairaut were, upon the completion of surveys made by Cassini in France, found to be at variance with his results. He found the length of one degree of a meridian south of Paris to be 57.092 toises, while north of the city it was only 56.960 toises. This led to the conclusion that the earth is a *prolate* spheroid. Here, of course was material for a controversy; in view of this fact the French Academy sent out two expeditions to make measurements that would definitely settle the matter. These expeditions set out in 1735; Bouguer, Godin and La Condamine proceeded to Peru, and after ten years' work they measured an arc of above 3° between the parallels 2′ 31″ N., and 3° 4′ 32″ S. latitude. Maupertius, Clairaut, Camas and Le Monnier, arriving in Lapland, measured an arc of 57 minutes, and returned within 16 months. The results deduced from these observations concurred in proving that the degrees of the meridian increase very sensibly in length from the equator to the high latitudes, and from this time dates the undisputed conclusion that the earth is an *oblate* spheroid, rather than a sphere or prolate spheroid.

The deviation from the spherical form is evidently very slight, the difference be-

* *Principia*, Book III.

tween the equatorial and polar diameter being only 27 miles. As an illustration, on a globe 24 inches in equatorial diameter, and on which the thickness of a sheet of writing paper would represent the elevation of the lands above the waters, the polar axis would be 23.928 inches, or in other words, the difference between the polar and equatorial axes would be but one-fourteenth of an inch. For this reason the spherical hypothesis is sufficiently accurate for many purposes. When this hypothesis is used in geodetical operations the radius of the earth as a sphere is taken as the average of all the radii of the spheroid. This radius is equal to 6.370 kilometers, or 3.958 miles. In the determination of the mean length of an arc of 1°, $\frac{1}{90}$ of the length of an elliptical quadrant of the spheroid is taken. Various values for this quadrant have been computed by different mathematicians. The one deduced by Bessel has been in long use for geodetical computations, and is very nearly the mean of the values found by other investigators. The mean length of one degree is, according to Bessel, 111.121 meters, or 69.043 miles.

It is thus seen that when the spherical hypothesis is applied the assumed sphere is one having an equal volume with that of the oblate spheroid.

There is, however, in these values, a serious inconsistency, for the quadrant of a circle corresponding to the above mean radius is nearly 6 kilometers greater than Bessel's value used in the above determination of the length of 1°. For this reason the value sometimes used for the radius is that of a circle whose circumference is equal to the circumference of a meridian ellipse, or 3.956 miles = 6.367 kilometers. This value is 3 kilometers too small, but the error is unavoidable.

That the science of the mathematicians had described in a general way the figures of our globe, was sufficient to satisfy the curiosity of the ordinary individual, but not the zeal of scientists for exact knowledge; they further endeavored to obtain the precise amount of the depression at the poles, whose existence had been proven by so many experiments. Material was accumulated, new arcs were measured, but the difficulty of an exact determination only increased. The different measures of degree lengths gave varying values for this depression upon the oblate-spheroidal hypothesis. An Italian mathematician, named Frisi, showed the variation of the calculated depression very clearly by a comparison of the measures then known. The following is a list of the arcs used by him in his computations, and also of the astronomers to whom we are indebted for their determination:

Country.	Latitude where the measurement commenced.	Value of the degree measured.	Observers.
Peru	0d —0m	56.753 tois.	Bouguer, La Condamine, etc.
Cape of Good Hope	33—18	57.107 "	Lacaille.
Pennsylvania	39—12	56.888 "	Mason and Dixon.
Ecclesiastical State	43—01	56.979 "	Boscovich and Maire.
France	43—31	57.048 "	Cassini and Lacaille.
Piedmont	44—44	57.137 "	Beccaria.
France	45—45	57.050 "	Cassini and Lacaille.
Hungary	45—57	56.881 "	Liesganig.
Austria	48—43	57.086 "	"
France	49—23	57.074 "	Picard & Cassini.
Holland	52—04	57.145 "	De Thury & G. Cassini.
Lapland	66—20	57.405 "	Maupertius.

Frisi, in his calculations, sought to determine, according to Newton's theory, the data for a regular curve from which could be derived the above values. In this he was unsuccessful. The curves were either too large or too small.

The values for the ellipticity of the earth's meridian deduced from the sur-

veys instituted by the French Academy are as follows:

Lapland and French arcs, $\frac{1}{145}$th;
Lapland and Peruvian arcs, $\frac{1}{310}$th;
French and Peruvian arcs, $\frac{1}{334}$th.

There was evidently a serious discrepancy either in the assumption as to the form of the earth, or in the accuracy of the determinations, for if the earth were a spheroid of revolution these results should be identical. Following this discovery numerous measurements of arcs of meridian were made in different parts of the world. The most important of these, however, were executed under the direction of the French Government in the determination of the length of the meter—taken as one ten-millionth part of the quadrant of the earth's meridian. These latter observations when combined with the corresponding values in the Peruvian arc gave for the ellipticity, $\frac{1}{334}$th. The nearest approximation of the calculated curve to an ellipse whose minor axis would be to its major in the ratio of 230 to 231 involved an error of more than 100 toises to the degree. Frisi then determined the mean value for the various depressions resulting from the above data and found, for the mean term, a depression almost identical with that furnished by the observations of the pendulum and the measurements for the determination of the French measures.

The evident impossibility of finding a regular curve to correspond to the different degrees measured gave rise to doubts as to the possibility of measuring a degree of the meridian with accuracy. The instruments then employed in the determinations were liable to errors of three or four seconds for the celestial arc, or 60 toises for a terrestrial degree.*

The attraction of mountains upon the plumb line, causing a deviation of the vertical, was another source of error. Thus, if the direction of the plumb line at the extremities of the arc measured deviated from the normal by 15 sec., it would cause an error of 500 toises or 533 fathoms in the final result, a quantity greater than the presumed difference of the two extreme degrees under the equator and the pole.† Towards the end of the last century various attempts were made to reconcile the accumulating data with the spheroidal hypothesis. Among the most prominent investigations are those of Boscovich in 1760, and Laplace in 1793 and 1799. Laplace took, as the basis of his combinations, nine of the measurements used by Frisi. The curve which he calculated gave for the length of a degree a value too small by 137.7 toises (=nearly 268 meters), or approximately nine seconds of latitude. These errors, says Laplace, are too great to be admitted, and it must be concluded that the earth deviates materially from the elliptical figure.*

When compared with the great size of the earth, this deviation of the figure of the earth from the oblate spheroidal form is very slight. As previously stated, for many practical problems it is sufficiently accurate to consider the earth as a sphere, but where, for the purposes of science, it is necessary to apply the spheroidal hypothesis, mathematicians have deemed it expedient to determine the elements of an ellipse agreeing as nearly as possible with the actual meridian section of the earth, and to base their calculations upon the resulting spheroid.

In 1805 Legendre announced the method of least squares for the adjustment of observations, and during the present century numerous applications of this principle in the determination of the mean ellipse of the earth's meridian have been made, the principal of which are given in the table on page 236.†

Of these, the values of Bessel and Clarke are considered the most reliable, and the spheroid deduced from the elements calculated by these investigators are called respectively the Bessel and Clarke spheroids. The dimensions of the terrestrial spheroid deduced by Bessel are as follows:

Greater or equatorial diameter, 7925.604 miles.
Lesser, or polar diameter, 7899.114 miles.
Difference of diameters, or polar compression, 26.491 miles.
Proportion of diameters as 299.15 to 298.15.

Probably the value for the ellipticity deduced from pendulum experiments is nearer the truth than any deduced from geodetic data. The latter values have

* Bouguer, Fig. de la Terre, sect. 1, § 4.
† Malt-Brun. p. 25.

* Hist. Acad. Paris, 1789.
† Jordan.

Year.	By whom.	Ellipticity.	Quadrant in meters.
1819	Walbeck	1 : 302.8	10.000.268
1830	Schmidt	1 : 297.5	10.000.075
1830	Airy	1 : 299.3	10.000.976
1841	Bessel	1 : 299.2	10.000.856
1856	Clarke	1 : 298.1	10.001.515
1863	Pratt	1 : 295.3	10.001.924
1866	Clarke	1 : 295.	10.001.887
1868	Fischer	1 : 288.5	10.001.714
1872	Listing	1 : 289.	10.000.218
1878	Jordan	1 : 286.5	10.000.681
1880	Clarke	1 : 293.5	10.001.869

been continually approaching those of the former, and we have every reason to believe that when perfection of geodetic operations is more nearly approached, the results will be practically identical. We give below the elements of the earth's figure deduced from pendulum observations:

Ellipticity $=\frac{1}{288.5}$. Eccentricity$=\frac{1}{12}$th.

Quadrant of E's Meridian section=10001 kilometers, or 6214.62 statute miles.

In 1859 Gen. de Schubert, in attempting to find a continuous curve for the meridian which would satisfy all measured geodetic arcs, suggested the hypothesis of an elliptic equator and an ellipsoidal figure. The ellipsoid is not a figure of revolution. The meridian sections, as in the spheroid, are ellipses, but the equator, instead of being a circle, is an ellipse. The curves of latitude, however, except the equator, are not plane curves, and consequently not true parallels.

Thus, we see that the ellipsoid has *three* unequal axes at right angles to each other.

Gen. de Schubert embodied his idea in his "Essai d'une Determination de la veritable Figure de la Terre," and deduced from eight meridian arcs an ellipsoid of the following elements:*

a_1=6,378,566 metres

a_2=6,377,837 "

b =6,356,719 "

$$f_1=\frac{1}{292.1}\quad f_2=\frac{1}{302}.\quad F=\frac{1}{8881}$$

where a_1 a_2 are the semi-equat. axes, b= semi-polar axis, f_1 f_2=the ellipticities of the greatest and least meridian ellipses, F=the ellipticity of the equator.

In 1860 and 1866 similar calculations were made by Capt. A. R. Clarke. Subsequent investigations led Clarke in 1878 to publish the results of a third discussion, giving as the elements of the ellipsoid the following:

a_1=20,926,629 feet.

a_2=20,925,105 "

b =20,854,477 "

$$f_1=\frac{1}{290},\ f_2=\frac{1}{296.3}.\ F=\frac{1}{13706}$$

The present opinion in regard to the ellipsoidal hypothesis is that until data of a more general and accurate kind have been accumulated, the elements of a satisfactory ellipsoid cannot be computed. Arcs of longitude are needed, for the ellipticities of the meridians differ by such small quantities, that measurements in their directions alone, are insufficient to determine with much precision the form of the equator and parallels.

Aside from this, the physical improbabilities of an ellipsoidal figure are so great that it seems more reasonable to attribute any apparent departure from the spheroidal figure to effects of *local attraction*. Again, there are physical reasons for supposing a spheroidal earth, but the existence of a fluid ellipsoid can only be explained by supposing the existence of an ellipsoidal nucleus, which all speculators in cosmogony agree in regarding as highly improbable. Arch. Pratt remarks, concerning the ellipsoidal figure, that "if a very large number of arcs in all parts of the world were measured, and local attraction being taken into account, the result gave an ellipsoid with its two equatorial axes differing by a

* Mem. d l'Acad. Imp. des Sciences de St. Petersbourg, VII. Serie, Tome 1, No. 6.

quantity, important when compared with the residual errors of observation, there might be some argument for an ellipsoidal figure."

During the present century the extensive trigonometrical surveys undertaken by many countries have been the means of furnishing a number of long and accurately measured arcs. Of these the most important are the Anglo-Gallic, the Russian, the Indian, and the U. S. Coast Survey. The first three have been used in most of the later determinations of the mean ellipse mentioned in the table on page 236.

Now, it is highly satisfactory to find that the oblate spheroidal figure, thus practically proved to exist, is what theoretically ought to result from the rotation of the earth on its axis. The form of the earth is owing to the reciprocal attractions of its component particles. When a weight is whirled around, it acquires a tendency to recede from the center of its motion, as a stone whirled around in a sling. This tendency is called *centrifugal* force. Supposing the rotation of the earth, a centrifugal force is generated whose general tendency will be to cause objects to fly off the surface. This force diminishes the gravity of particles, and hence they recede from the axis of the earth until, by their number and attraction, they counterbalance the centrifugal force. This is confirmed by experience. There is an actual difference in the force of gravity or downward tendency of the same body when conveyed successively to points in different latitudes. Delicate experiments, conducted with the greatest care, have fully demonstrated the fact of a regular and progressive increase in the weight of bodies, corresponding to the increase of latitude.

Now, let us suppose a globe of the size of the earth to be uniformly covered with water. So long as the body remained fixed the surface of the water would outline a perfect sphere. Immediately attending the introduction of rotation on its axis, a centrifugal force would be developed which would act upon every particle in such a way as to tend to cause it to recede from the axis of rotation. But now every particle would be subject to the action of two forces—the one just mentioned, and that of gravity. The direction of the former would be perpendicular to the axis of rotation, and that of the latter perpendicular to the surface of the water. Since at no position but the line of the equator are these forces directly opposite, they combine to form a third force which urges every particle not situated in the equator towards it with a force dependent upon the velocity of rotation. This latter force and the figure of the resulting surface of the water are so connected that an increase of centrifugal force is always counterbalanced by a proportionate change in the direction of gravity. Therefore the water would recede from the poles and heap itself on the equator. This would leave the polar regions, in the case of the earth, protuberant masses of land. Now, the sea is constantly washing and grinding away the land, and carrying and depositing pebbles and fragments over its bed. Thus, in the case considered, the water beating the polar continents would gradually wear them down, and, as with the molecules of water, so, in turn, the worn-off particles and fragments of the polar land would would be forced towards the equator, till the earth would assume by degrees the form we have shown it to approximate—the oblate spheroid.

Section II.

It is not our purpose to enter into the mathematical discussion of the fluid theory of the figure of the earth, but simply to place before our readers such principles of the spheroidal hypothesis as will lead to practical results. For a complete investigation of the form assumed by a revolving fluid on the principle of gravitation, we would refer our readers to section 2 of Mr. Airy's essay upon the Figure of the Earth contained in the "Encyclopedia Metropolitana."

We have stated that if the earth be considered a fluid mass, the form of the surface will be an oblate spheroid of small ellipticity. Further, its axis will coincide with the axis of revolution, and the surface will everywhere be perpendicular to the direction of gravity. It follows also upon the assumption that the density of the strata varies according to a certain probable law, that the ellipticity is $\frac{1}{293}$.

Let us assume, then, that the mean

figure of the earth is an oblate spheroid, and endeavor to show by what methods an ellipse can be found cutting the plumb line at right angles and with its minor axis coinciding with the axis of the earth. This end may be reached by four methods, and first we will consider how the figure of the earth may be determined from geodetic operations.

CHAPTER I.

THE FIGURE OF THE EARTH DETERMINED BY GEODETIC OPERATIONS.

The first step in this method is to measure as accurately as possible a base line of any convenient length, not less than 5 or 6 miles, and as near as possible to the meridian upon which we are to base our calculations. From the extremities of this line the angles are measured between the base line and visual lines joining distant points, also taken as near the meridian as convenient. Knowing the length of one side, and two of the angles of a triangle, we can, by trigonometrical formulae deduce the lengths of the other sides. Repeating the operation with the sides already calculated, and selecting new points to suit the emergencies of the case, we establish a connection between the original base line and a second base at the termination of the chain of triangles, and obtain the length of this second base by calculation. It is then measured, and by a comparison of the calculated and measured results the correctness of the operations is tested. This having been satisfactorily performed, the projections of the sides of the triangles upon the meridian are found, and their sum gives the length of the meridian arc between its two extremities. The latitudes of these two extremities are then observed with great care, and from these data the ellipse—of which the arc is part—is found as follows:

Let l and l' be the latitudes of the extremities of the arc.

" m be the mean of l and l', or the middle latitude.

" λ be the amplitude, or $l-l'$.

" a and b be the semi-axes.

" ε be the ellipticity.

" s be the length of the arc.

" r be the radius vector.

Let θ be the angle r makes with the major axis.

Then

$$\frac{1}{r^2}=\frac{\cos.^2\theta}{a^2}+\frac{\sin.^2\theta}{b^2},\ \tan.\ l=\frac{a^2}{b^2}\tan.\ \theta$$

$$\therefore\ \frac{1}{r^2}=\frac{a^2\cos.^2 l+b^2\sin.^2 l}{a^4\cos^2 l+b^4\sin.^2 l},$$

placing $b=a\,(1-\varepsilon)$

$$r=a\,(1-\varepsilon\sin.^2 l)\text{ neglecting }\varepsilon^2,$$

$$\frac{dr}{dl}=-2a\varepsilon\sin.l\cos.l,\ \frac{d\theta}{dl}=1-2\varepsilon+4\varepsilon\sin.^2l$$

$$\therefore\ \frac{ds}{dl}=\sqrt{r^2\frac{d\theta^2}{dl^2}+\frac{dr^2}{dl^2}}=a(1-2\varepsilon+3\varepsilon\sin.^2l)$$

$$=a\,(1-\tfrac{1}{2}\varepsilon-\tfrac{3}{2}\varepsilon\cos.\ 2l)$$

$$\therefore s=a[(1-\tfrac{1}{2}\varepsilon)(l-l')-\tfrac{3}{4}\varepsilon(\sin.2l-\sin.2l')]$$

$$=\tfrac{1}{2}(a+b)\,\lambda-\tfrac{3}{2}(a-b)\sin.\ \lambda\cos.\ 2m.\quad(1)$$

If λ be small, not exceeding 12°, we may place $\sin.\ \lambda=\lambda$, then

$$\frac{s}{\lambda}=\frac{a+b}{2}-3\frac{a-b}{2}\cos.\ 2m$$

$$\because\ \frac{s'}{\lambda'}=\frac{a+b}{2}-3\frac{a-b}{2}\cos.\ 2m'$$

when the lengths, amplitudes and latitudes of two arcs are known.

$$\therefore\ \frac{a-b}{2}=\frac{1}{3}\frac{\frac{s}{\lambda}-\frac{s'}{\lambda'}}{\cos.\ 2m'-\cos.2\,m},\text{ and}\quad(2)$$

$$\frac{a+b}{2}=\frac{\frac{s}{\lambda}\cos.2m'-\frac{s'}{\lambda'}\cos.2\,m}{\cos.2\,m'-\cos.2\,m}\quad(3)$$

From these formulas the semi-axes, a and b, may be computed and the value for ε, the ellipticity follows.

If we were to substitute in these equations observed values resulting from the various measurements of meridian arcs in different parts of the earth, it would be found that the calculated semi-axes and ellipticities in the various cases would be different. If the figure of the earth were truly spheroidal, and there were no errors in the data, *i. e.*, in the observed amplitudes and measured arcs, the results would come out in complete accordance with each other. But this is evidently not the case.

Let us inquire, then, as to the source of these variations in the calculated spheroid, and in attaining this end we will first

state the assumptions we have made in deducing the above formulae. These are (1) that the meridian arc is an ellipse in accordance with the fluid theory. (2) That the plumb line at all stations is normal to this ellipse.

It is plain that the conclusions of the fluid theory are not applicable to the earth in its present state. The irregularities in the external form of the earth are considerable when theoretically considered, and, of necessity, these irregularities of the surface must be accompanied by irregularities in the mathematical figure of the earth. The height of mountains and depth of seas are, in some places, equal to $\frac{1}{4}$ the difference between the equatorial and semi-polar axes, and we are, therefore, prepared to admit that the surface of the earth is not one of revolution. Nevertheless, there must be some spheroid that agrees very closely with the mathematical figure of the earth and having the same axis of rotation.

The errors due to local attraction result from the deflection of the plumb line from the normal to the assumed ellipse. This is sufficient in certain parts to produce material errors in the vertical, and therefore in the difference of latitude determined by the zenith distances of stars.

Thus, suppose A and B to be two stations, and that at A there is a disturbing force drawing the plumb line through an angle δ; then it is evident that the apparent zenith of A will in reality be that of some other place A', whose distance from A is $r\delta$, when $r =$ earth's radius. Similarly, if there is a disturbance at B of the amount δ', the apparent zenith of B will be that of some other place B', whose distance from B is $r\delta'$.

Mountain masses, oceans, and variations in the density of the interior of the earth are the chief causes of *local attraction.*

It is then evident that our assumptions in the preceding formula are at fault, and it is now necessary to discover some means whereby the resulting errors may be eliminated.

Bessel was the first to devise a method whereby the results of all the surveys in the different parts of the earth might be brought to bear simultaneously upon this problem. He was followed by Capt. A. R. Clarke, who discussed the problem at the end of the Ordnance Survey Volume.

Bessel's method is in essence as follows: *Corrections* expressed in algebraic terms are applied to the latitudes of the several stations, dividing the arcs into the subordinate parts, such as to make their measured lengths exactly fit an ellipse. The values of the axes of this ellipse are then determined so as to make the sum of the squares of these corrections a minimum, *i. e.*, an ellipse is deduced which most nearly represents the observations.

We will now endeavor to state briefly how this may be done.

Let us first obtain a formula for correcting the amplitude of an arc so as to make its measured length accord with a given ellipse.

We have equation (1),

$$s=\tfrac{1}{2}(a+b)\,\lambda-\tfrac{3}{2}(a-b)\sin.\lambda\cos.2\,m.$$

Suppose now that x, x' are small corrections which must be applied to the observed latitudes to make the measured arc fit the ellipse of which a and b are the semi-axes; λ and m when substituted in the above formula will not give the measured value of s. Instead of them

$$\lambda+x'-x \text{ and } 2m+x'+x$$

must be substituted. Hence omitting very small quantities

$$2s=(a+b)\,\lambda-3\,(a-b)\sin.\lambda\cos.2m$$
$$+(x'-x)[a+b-3(a-b)\cos.\lambda\cos.2m]$$

$$\therefore x'-x=\left(\frac{2s}{a+b}-\lambda+3\frac{a-b}{a+b}\sin.\lambda\cos.2m\right)$$
$$\left(1+3\frac{a-b}{a+b}\cos.\lambda\cos.2m\right). \quad . \quad (4)$$

Now the mean radius of the earth is known not to differ much from 20890000 feet, and the ellipticity not much from $\frac{1}{300}$. It is therefore convenient to put $a+b$ under the form

$$\frac{a+b}{2}=\left(1-\frac{u}{10000}\right)20890000\ . \quad (5)$$

$$\frac{a-b}{2}=\frac{1}{600}\left(1-\frac{u}{10000}+\frac{v}{50}\right)20890000. \quad (6)$$

$$\therefore\ \varepsilon=\frac{1}{300}\left(1+\frac{v}{50}\right) \quad . \quad . \quad (7)$$

where the squares of u and v may be neglected.

Substituting these in formula (4) and changing the form, we have

$$x'=m+\alpha u+\beta v+x,$$

where m, α, β, are functions of the observed latitudes, the measured lengths and other numerical quantities.*

In every measured arc, not only are the extreme stations determined in latitude, but also a number of intermediate stations; one of them should be taken as a station of reference, and each separate error should be written in terms of the reference station error.

Thus if we have five stations,

$$x_1=x_1$$

$$x_2=m+\alpha u+\beta v+x_1$$

$$\dots\dots\dots\dots\dots\dots$$

$$x_5=m''''+\alpha u+\beta v+x_1.$$

Now, since these equations are affected by errors of observation, it is impossible to find values of u and v which will satisfy all the equations, and therefore it is necessary to find their most probable values. According to the method of least squares, those values of u and v are the most probable, which render the sum of the squares of all the errors a minimum.

In proceeding with this method, we first deduce normal equations for u by multiplying each observation equation by the coefficient of u in that equation, and add the results; likewise, deduce normal equations for v by multiplying each observation equation by the coefficient of v in that equation, and add the results. Thus we will have two normal equations, each containing two unknown quantities, and the solution of these will give us the most probable values for u and v. Substituting these values in equations (5), (6), (7), we obtain values for the two semi-axes and the ellipticity.

In conclusion, let us examine what effect any error in the amplitudes will have upon the resulting axes.

If we differentiate equations (2, 3), there will appear in the denominators of the resulting expressions the quantity

$$\cos 2m-\cos 2m'.$$

The errors in the axes dependent upon the amplitude errors will therefore be least when this quantity is a maximum. If an arc is located in the southern half of the quadrant, cos 2 m is positive, then

$$2m'=180°, \text{ or } m'=90°$$

will give the best result. If another arc is in the northern half, cos $2m$ is negative, and

$$\cos 2m'=0$$

will give the best result.

Therefore the nearer one arc is to the pole, and the other to the equator, the less will be the effect upon the calculations of any errors in the observed amplitudes.

Chapter II.

THE FIGURE OF THE EARTH DEDUCED FROM PENDULUM EXPERIMENTS.

Upon the hypothesis of the earth being a fluid mass, Clairaut deduced an equation showing that the increase of gravity in passing from the equator to the poles varies as the square of the sine of the latitude, and that a certain relation must necessarily exist between the ellipticity and the force of gravity. Therefore, if we desire to use this theorem in the determination of the figure of the earth, we must first obtain some practical method of measuring the force of gravity in any latitude. The necessity of using the pendulum for this purpose will easily be seen if we consider the impossibility of ascertaining the magnitude of the force by an experiment upon the single descent of free bodies. The quantity to be measured is the velocity which gravity creates in any freely descending body by its action continued during a second of time, and in conducting a series of experiments it is usual to observe the number of vibrations made by the same seconds pendulum in the different places at which it is proposed to compare the force of gravity, and likewise the number of vibrations made at London or Paris. The comparative number of vibrations being found, the comparative force of gravity or the comparative length of the seconds pendulum can be deduced; since the length of the seconds pendulum has been very accurately determined at London and Paris, its length in any latitude may be found.

* The values of m, α and β, have been calculated for the principal arcs and may be found in the British Ordnance Survey volume.

We will now endeavor to show by what means we deduce the Figure of the Earth from Pendulum experiments.

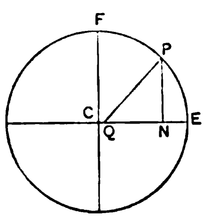
We have for the attraction at any point of a spheriod the following expression:

$$\frac{4\pi}{3}\cdot\frac{\Phi(c)}{c^2}$$

$$\left\{1-(2e-m)-\left(\frac{5m}{2}-e\right)\left(\frac{f^2+g^2}{c^2}\right)\right\}. \quad (8)$$

$$m = \text{in the earth} = \frac{1}{289}$$

In which $m=$ ratio of centrifugal force to gravity, f and $g=$ the co-ordinates of the point, in the directions of CD and CE respectively, $c=$polar axis, $e=$ellipticity and $\frac{4\pi}{3}\cdot\frac{\Phi(c)}{c^2}=$ gravity at the equator.



Let EF represent the earth's surface, and let PQ be the normal at P.

Then P Q N is the latitude of P and

$$QN=PQ, \cos. l.$$

Now as we shall have to substitute only in the small terms of the equation, $PQ=c$ nearly, and $QN=CN$ nearly,

$$=\sqrt{(f^2+g^2)}\ \therefore\ \sqrt{(f^2+g^2)}=c, \cos. l$$

nearly.

Substituting this in equation (8), we have for the force of gravity

$$=\frac{4\pi}{3}\frac{\Phi(c)}{c^2}\left\{1-(2e-m)-\left(\frac{5m}{2}-e\right)\cos.^2 l\right\}$$

$$=\frac{4\pi}{3}\cdot\frac{\Phi(c)}{c^2}\cdot\left\{1-\left(e+\frac{3m}{2}\right)+\left(\frac{5m}{2}-e\right)\sin.^2 l\right\}$$

$$=\frac{4\pi}{3}\cdot\frac{\Phi(c)}{c^2}\cdot\left\{1-\left(e+\frac{3m}{2}\right)\right\}$$

$$\left\{1+\left(\frac{5m}{2}-e\right)\sin.^2 l\right\}$$

Gravity, therefore, may be generally expressed by the formula

$$E(1+n\sin.^2 l), \quad . \quad . \quad . \quad (9)$$

where E = equatorial gravity and

$$n+e=\frac{5m}{2}.$$

Now let p and p' be the lengths of the seconds pendulum in latitudes l and l', P that at the equator, then from equation (9) we have

$$p=P(1+n\sin.^2 l) \quad . \quad . \quad (10)$$
$$p'=P(1+n\sin.^2 l')$$

where

$$n=\tfrac{5}{2}m-e$$

$$\therefore n=\frac{1-\frac{p}{p'}}{\frac{p'}{p}\sin.^2 l'-\sin.^2 l}=\frac{1-\frac{p}{p'}}{\sin.^2 l'-\sin.^2 l}$$

and $$e=\frac{5m}{2}-n$$

In applying the preceding equations to a series of pendulum observations for the determination of the figure of the earth, the principle of Least Squares is applied as in the last chapter to a number of observation equations of the form (10), thereby obtaining the most probable values for P and n.

Chapter III.

THE ELLIPTICITY OF THE EARTH DEDUCED FROM OBSERVED INEQUALITIES OF THE MOON'S MOTION.

In the expression for the tangent of the moon's latitude there is this term

$$-\left(E-\frac{m}{2}\right)\cdot\frac{4.(60)^2}{n^2\pi}\text{-sin.}^2\text{ parallax. }\frac{E}{\mu}$$

$$\text{sin. obliquity, cos. obliquity, sin. }\theta$$

Now $\frac{E}{\mu}=\frac{\text{Earth's Mass}}{\text{Earth}+\text{Moon}}=\frac{70}{71}$, nearly,

$$n=\frac{27.25}{365.25}$$

The mean horizontal parallax $\frac{E(r)}{M(r)}=57'$,

obliquity $= 23°-28'$ nearly,

$$\therefore \text{term}=-\left(\varepsilon-\frac{m}{2}\right).4891''\ \text{sin. }\theta$$

Now this inequality has been found to exist and its magnitude has been inferred rom observation. It has the effect of fncreasing the apparent inclination of the moon's orbit in one position of her nodes and diminishing it as much in the opposite position.

It is found by observation that the coefficient $=-8''$

$$\therefore \left(E-\frac{m}{2}\right)=\frac{8'}{4891}=001635$$

and $\frac{m}{2}=.001730$

$$\therefore \varepsilon=.003365=\tfrac{1}{297}$$

THE EARTH'S ELLIPTICITY DEDUCED FROM THE PRECESSION OF THE EQUINOXES.

The formula expressing the annual Precession is the following:

$$\frac{C-A}{C}\,\frac{3n'}{n}\cdot \cos. I \left(1+\frac{n''^2\, 1-\frac{3}{2}\sin.^2 i}{n'^2\,(1+v)}\right) 180^\circ$$

in which I= the obliquity of the ecliptic $= 23^\circ - 28' - 18''$, i = inclination of moon's orbit to ecliptic $=5^\circ-8'-50''$, n and n' are the mean motions of the earth around its axis and around the sun and their ratio $=365.26$, n'' the mean motion of the moon around the earth $=27.32$ days, $v=$ ratio of the masses of earth and moon $=75$.

Substituting these quantities

$$\text{Annual Precession} = 16225''.6\,\frac{C-A}{C}$$

where A and C are the principal moments of inertia of the mass, the latter about the axis of revolution.

Now $\frac{C-A}{C}=1.98177\ (\varepsilon-\tfrac{1}{2}m)'$*

$\therefore$ Annual Precession $=32155''\ (\varepsilon-\tfrac{1}{2}m)$

But the Precession by observation $=50''.1$

$$\therefore \varepsilon-\tfrac{1}{2}\,m=50.1\div 32155=0.0015581$$

$$\therefore \varepsilon=0.0015581+0.0017271=\tfrac{1}{304}$$

In estimating the reliance to be placed on these results it must be observed that unless the observations extend over a period greater than 20 years they are insufficient. This renders the resulting determinations less reliable than they otherwise would be, as in all probability the observations which are compared have been made by different persons and in different manners. The small lunar inequalities, besides, are involved among a mass of terms greater than themselves. Airy remarks concerning this fact that an error in their determination has less influence on the value of ε than an equal error in the determination of nutation. These facts show clearly that the deduction of the two preceding divisions cannot be compared with those of geodetic measures and pendulum observations. However, the close agreement of the results with those deduced from geodetic measures and pendulum experiments is significant. It shows that in the main the spheroidal hypothesis is correct.

* See Pratt's Fig. of the Earth. Page 151.

ENGINEERING INVENTIONS SINCE 1862.*

By Sir FREDERICK JOSEPH BRAMWELL, F.R.S.

From "Iron."

I.

Born in the year this institution was founded, and by God's mercy permitted to continue an active worker for more than half a century, I now find myself, by your favor, elected to the presidency. In accordance with established usage I have the privilege, but also, I regret to say, the heavy burden, of delivering an address; a privilege, because, by your courtesy, I see around me an audience of most distinguished men; a heavy burden, because of my inability to make that address worthy of my audience. I can imagine that in many societies, each president, as he takes office, must find an increase in the difficulty of selecting a subject for his address. He must feel that the area to which he is restricted has been so well trodden by his predecessors that it is hardly possible for him to strike out a new path on untraversed ground; but it seems to me that many years must elapse (indeed, I doubt if the time will ever come) when the president of the Institution of Civil Engineers need fear such a condition of things.

* Address of Sir Frederick Joseph Bramwell, F. R. S., on his election as President of the Institution of Civil Engineers, January 13th, 1885.

Our profession is so widespread, it ramifies in so many directions, that successive presidents cannot be embarrassed for want of a subject, but rather must find a difficulty in making a selection among many subjects. I have been determined in my choice by the consideration that our member, His Royal Highness the Prince of Wales, has seen fit to honor the institution by appointing its president to the chairmanship of the executive council of the exhibition of inventions, which is to take place this year at South Kensington. I have, therefore, thought it would be fitting and appropriate that, in my double capacity of your president and chairman of that council, I should direct attention this evening to some of the matters which will be, or which ought to be, contributed to the exhibition. The exhibition will consist of two divisions very dissimilar in their nature, the one "inventions" and the other "music." Division I. (inventions) is separated into thirty one groups, and these groups into 165 classes, but to a few of these only will reference be made. In fact, I must restrict myself to such matters as are more particularly connected with civil engineering, which, as defined by our charter, may be summarized as comprising "all engineering other than military." I must, however, make a still further limitation, for engineering, regarded in this sense, is so comprehensive in its scope, that it is difficult to say what there is of invention, in which the engineer has not an interest. Even a purely chemical invention may not be foreign to our consideration; take, as a test of this, an invention relating to the manufacture of gas, or to the purification of water, or to the treatment of metals, or to some kindred subject.

I propose, therefore, to devote the very limited time at my disposal to the consideration of some of the most important of those improvements which are obviously and immediately connected with civil engineering. I am aware of the danger there is of making a serious mistake, when one excludes any matter which at the moment appears to be of but a trivial character. For who knows how speedily some development may show that the judgment which had guided the selection was entirely erroneous, and that that which had been passed over was, in truth, the germ of a great improvement? Nevertheless, in the interests of time, some risk must be run, and a selection must be made; I propose, therefore, to ask your attention while I consider certain of (following the full title of Division I.) "The apparatus, appliances, processes and products invented or brought into use since 1862." In those matters which may be said to involve the principles of engineering construction, there must, of necessity, be but little progress to note. Principles are generally very soon determined, and progress ensues, not by additions to the principles, but by improvement in the methods of giving to those principles a practical shape, or by combining in one structure principles of construction which had been hitherto used apart. Therefore, to avoid the necessity of having a pause, in referring to a work, by finding that one is overstepping the boundary of principle and trenching within the domain of construction, I think it will be well to treat these two heads together. If my record had gone back to just before 1851 (the date of the great exhibition), I might have described much progress in the principles of girder construction; for, shortly prior to that date, the plain cast-iron beam, with the greater part of the metal in the web, and with but little in the top and bottom flange, was in common use, and even in the preparation of the building for that exhibition, it is recorded that one of the engineers connected therewith had great difficulty in understanding how it was that the form of openwork girder, with double diagonals introduced therein (a form which was for years afterwards known as the exhibition girder) was any stronger than a girder with open panels separated by uprights, and without any diagonals. But, long before 1862, the Warren and other truss girders had come into use, and I am inclined to say, that so far as novelty in the principle of girder construction is concerned, I must confine myself to that combination of principles which is represented by the suspended cantilever, of which the Forth Bridge, only now in course of construction, affords the most notable instance. It is difficult to see how a rigid bridge, with 1,700 feet spans, and with the necessity for so much clear headway below,

could have been constructed without the application of this principle.

Pursuing this subject of bridge work: The St. Louis Bridge of Mr. Eads may, I think, be fairly said to embody a principle of construction novel since 1862, that of employing for the arch ribs tubes composed of steel staves hooped together. Further, in suspension bridges there has been introduced that which I think is fairly entitled to rank among principles of construction, the light upper chain, from which are suspended the linked truss-rods, doing the actual work of supporting the load, the rods being maintained in straight lines, and without the flexure at the joints due to their weight. In the East River Bridge, New York, there was also introduced that which I believe was a novelty in the mode of applying the wire cables. These were not made as untwisted cables and then hoisted into place, thereby imposing severe strains upon many of the wires composing the cable through their flexure over the saddles and elsewhere, but the individual wires were led over from side to side, each one having the length appropriate to its position, and all, therefore, when the bridge was erected, having the same initial strain and the same fair play. Within the period we are considering, the employment of testing machines has come into the daily practice of the engineer; by the use of these he is made experimentally acquainted with the various physical properties of the materials he employs, and is also enabled in the largest of these machines to test the strength and usefulness of these materials, when assembled into forms, to resist strains, as columns or as girders. I, of course, do not for one moment mean to say that experimental machines were unknown or unused prior to 1862—chain-cable testing machines are of old date, and were employed by our past-president, Mr. Barlow, and by others, in their early experiments upon steel—but I speak of it as a matter of congratulation that, in lieu of such machines being used by the few, and at rare intervals upon small specimens, for experimental purposes, they are now employed in daily practice, and on a large scale. In harbor work we have had the principle of construction employed by Mr. Stoney at Dublin, where cement masonry is moulded into the form of the wall for its whole height and thickness, and for such a length forward as can be admitted, having regard to the practical limit of the weight of the block, and then, the block being carried to its place, is lowered on to the bottom which has been prepared to receive it, and is secured to the work already executed by groove and tongue.

It would not be right, even in this brief notice of such a mode of construction, to omit mention of the very carefully thought-out apparatus by which the blocks are raised off the seats whereon they have been made and are transported to their destination. It is no simple undertaking (even in these days) to raise (otherwise than hydraulically) a weight of 350 tons, which is the weight of the blocks with which Mr. Stoney deals. But he does this by means of pulley blocks attached to shears built on the vessel which is to transport the block, and he contrives to lift the weight without putting upon his chains the extra strain due to the friction of the numerous pulleys over which they pass. The height of the lift is only the few inches needed to raise the block clear of the quay on which it has been formed, and this is obtained by winding up the chain by steam gear quite taut, so as to take a considerable strain, but not that equal to the weight of the block, and then water is pumped into the opposite end of the vessel to that upon which the shears are carried, this latter end rises, and the block is raised off the seat on which it was formed without the chains being put to work to do the actual lifting at all. The vessel, with the block suspended to the shear legs and over the bows, is then ready to be removed to the place where the block has to be laid. A word must here be said about an extremely ingenious mode of dealing with the slack chain, to prevent its becoming fouled and not paying out properly when the block is being lowered. This is accomplished by reeving the slack of each chain over two fixed sets of multiple sheaves. A donkey engine works a little crab having a large drum, the chain from which is connected with the main chain, and draws it round the multiple sheaves, so as to take up the slack as fast as the main crab gives it out. The steam is always on the donkey, which is of such limited dimensions

that it can do no injury to the chain even when its full power is in vain endeavoring to draw it any further; directly, however, the main crab gives more slack, and the chain between it and the two sets of sheaves falls into a deeper catenary, and one which, therefore, puts less opposition to the motion of the donkey engine, that engine goes to work and makes a further haul upon the slack, and in this way, and automatically, the slack is kept clear.

A noteworthy instance of the use of pneumatic appliances in cylinder-sinking for foundations, is that in progress at the Forth Bridge. The wrought-iron cylinders are 70 feet in diameter at the cutting edge, and have a taper of about 1 in 46. They are, however, at a height of 1 foot above low water (that is, at the commencement of the masonry work at the pier) reduced to 60 feet in diameter; at their bottoms there is a roofed chamber, into which the air is pumped, and in which the men work when excavating, this roof being supported by ample main and cross-lattice girders. Shafts with air-locks and pipes for admitting water and ejecting silt are provided. The air-locks are fitted with sliding doors, worked by hydraulic rams, or by hand, the doors being interlocked in a manner similar to that in which railway points and signals are interlocked, so that one door cannot be opened until the other is closed. The hoisting of the excavated material is done by a steam engine fixed outside the lock, this engine working a shaft on which there is a drum inside the lock, the shaft passing air-tight through a stuffing box. A separate air-lock, with doors, ladders, &c., complete, is provided to give ingress and egress to the workmen. I have already adverted to one Scotch bridge; I now have to mention another, viz., the Tay Bridge, also now in course of construction. Here the cylinders are sunk, while being guided through wrought-iron pontoons, which are floated to their berths and are then secured at the desired spot by the protrusion, hydraulically of four legs, which bear upon the bottom, and thus, until they are withdrawn, convert the pontoon from a floating into a fixed structure.

I regret that time will not admit of my giving any description of the modes of "cut and cover" which have been proposed for the performance of sub-aqueous works; sometimes the proposition has been to do this by means of coffer-dams, and with the work therefore open to the daylight during execution, and sometimes by movable pneumatic appliances. Consideration of sub-aqueous works necessarily leads the mind to appliances for diving, and although its date is considerably anterior to 1862, I feel tempted, as I believe the construction is known to very few of our members, to say a few words about the diving apparatus known as the "Bateau-plongeur," and used at the "barrage" on the Nile. This consists of a barge fitted with an air-tight cabin, provided with an air-lock, and having in the center of its floor a large oval opening, surrounded by a casing standing up above the water-line. In this casing slides another casing telescopically, the upper part of which is connected to the top of the fixed casing by a leather "sleeve." When it is desired to examine the bottom of the river, the telescopic tube is lowered till it touches the bottom, and then air is pumped into the cabin until the pressure is sufficient to drive out the water, and thus to expose the bottom. This appears to be a very convenient arrangement for shallow draughts of water. Reverting for a moment to Mr. Stoney's work, I may mention that he uses for the greater depths he has to deal with, when preparing the bed to receive his blocks, a diving apparatus which (while easily accessible at all times) dispenses with the necessity of raising and lowering needed in an ordinary diving bell, to allow of the entrance and exit of the workmen. Mr. Stoney employs a bell of adequate size, from the summit of which rises a hollow cylinder, furnished at the top with an air-lock, by which access can be obtained to the submerged bell. Beyond the general improvement in detail and in the mode of manufacture, and with the exception of the application of the telephone, there is probably not much to be said in the way of invention or progress in connection with the ordinary dress of the diver. But one great step has been made in the divers' art by the introduction of the chemical system of respiration, the invention of Mr. Fleuss. He has succeeded in devising a

perfectly portable apparatus, containing a chemical filter, by means of which the exhaled breath of the diver is deprived of its carbonic acid; the diver also carries a supply of compressed oxygen, from which to add to the remaining nitrogen oxygen, in substitution for that which has been burnt up in the process of respiration. Armed with this apparatus, a diver is enabled to follow his avocations without any air tube connecting with the surface, indeed, without any connection whatever. A notable instance of a most courageous use of this apparatus was afforded by a diver named Lambert, who, during one of the inundations which occurred in the construction of the Severn tunnel, descended into the heading, and proceeding along it for some 330 yards (with the water standing some 35 feet above him), closed a sluice door, through which the water was entering the excavations, and thus enabled the pumps to unwater the tunnel. Altogether, on this occasion, this man was under the water, and without any communication with those above, for one hour and twenty-five minutes. The apparatus has also proved to be of great utility in cases of explosions in collieries, enabling the wearer to safely penetrate the workings, even when they have been filled with the fatal choke damp, to rescue the injured, or to remove the dead.

With respect to the subject of tunneling, thus incidentally introduced—in subaqueous work of this kind, I have already alluded to that which is done by "cut and cover," but where the influx of water is a source of great difficulty, as it was in the old Thames tunnel (though, in this case, for water one should read silt or mud), I do not know that anything has been devised so ingenious as the Thames tunnel shield; improvement has, however, been made by the application of compressed air. In the instance of the Hudson River tunnel, the work was done in the manner proposed so long ago as the year 1830, by Lord Cochrane (Earl Dundonald) in that specification of his, No. 6,018, wherein he discloses, not merely the crude idea, but the very details needed for compressed-air cylinder-sinking and tunneling, including air-locks and hydraulically-sealed modes, for the introduction and extraction of materials. I may, perhaps, be permitted to mention that some few years ago I devised for a tunnel through the water-bearing chalk a mode of excavation by the use of compressed air to hold back the water, and combined with the employment of a tunneling machine. This work, I regret to say, was not carried out. But there are, happily, cases of sub-aqueous tunneling where the water can be dealt with by ordinary pumping power, more or less extensive, and where the material is capable of being cut by a tunneling machine. This was so in the Mersey tunnel, and would be in the Channel tunnel. In the Mersey tunnel, and in the experimental work of the Channel tunnel, Colonel Beaumont and Major English's tunneling machine has done most admirable work. In the 7-foot 4-inch diameter heading, in the new red sandstone of the Mersey tunnel, a speed of as much as 10 yards forwards in 24 hours has been averaged, while a maximum of 11¾ yards has been attained; while, in the 7-foot heading for the channel tunnel, in the grey chalk, a maximum speed of as much as 24 yards forwards in the 24 hours has been attained on the English side, and with the later machine put to work at the French end, a maximum speed of as much as 27½ yards forwards in the 24 hours has been effected. In ordinary land tunneling, since 1862, there has been great progress, by the substitution of dynamite and preparations of a similar nature for gunpowder, and by the improvements in the rock-drills worked by compressed air, which are used in making the holes into which the explosive is charged. For boring for water, and for many other purposes, the diamond drill has proved of great service, and most certainly its advent should be welcomed by the geologist, as it has enabled specimens of the strata passed through to be taken in the natural unbroken condition, exhibiting not only the material and the very structure of the rock, but the direction and the angle of the dip of the beds.

Closely connected with tunneling machines are the machines for "getting" coal. This "getting," when practiced by manual labor, involves, as we know, the conversion into fragments and dust of a very considerable portion of the underside of the seam of coal, the workman

laboring in a confined position, and in peril of the block of coal breaking away and crushing him beneath it. Coal-getting machines, such as those of the late Mr. Firth, worked by compressed air, reduce to a minimum the waste of coal, relieve the workman of a most fatiguing labor in a constrained position, and save him from the danger to which he is exposed in the hand operation. It is a matter of deep regret on many grounds, but especially as showing how little the true principles of political economy are realized by working men, who are usually well informed on many other points, that the commercial failure of these machines is due to their opposition. In connection with colliery work, and indeed in connection with explosives, in the sense of a substitution for them of sources of expansion acting more slowly, mention should be made of the hydraulic wedges. The employment of these in lieu of gunpowder, to force down the block of coal that had been undercut, is one of the means to be looked to for diminishing the explosions in collieries. Another substitute for gunpowder is found in the utilization of the expansion of lime when wetted. This has given birth to the lime cartridge, the merits of which are now universally recognized, but it is feared that trade prejudices may also prevent its introduction. While on this subject of "accidents in mines," it will be well to call attention to the investigations that have been made into the causes of these disasters, and into the probable part played by the minute dust which prevails to so great an extent in dry collieries. The experiments of our honorary member, Sir Frederick Abel, on this point have been of the most striking and conclusive character, and corroborate investigations of the late Maquorn Rankine into the origin of explosions in flour mills and in rice mills, which had previously been so obscure. The name of Mr. Galloway should also be mentioned as one of the earliest workers in this direction. At first sight, pile-driving appears to have but little connection with explosives, but it will be well to notice an invention which has been brought into practical use, although not largely (in this country, at all events), for driving piles, by allowing the monkey to fall on a cartridge placed in a cavity in the cap on top of the pile; the cartridge is exploded by the fall, and in the act of explosion drives down the pile and raises the monkey; during its ascent and before the completion of its descent time is found for the removal of the empty cartridge and the insertion of a new one.

In the days of Brindley and of Smeaton, and of the other fathers of our profession, whose portraits are on these walls, canals and canalized rivers formed the only mode of internal transit, which was less costly than horse traction, and, thanks to their labors, the country has been very well provided with canals; but the introduction of railways proved, in the first instance, a practical bar to the extension of the canal system, and, eventually, a too successful competitor with the canals already made. Frequently the route that had been selected by the canal engineer was found (as was to be expected) a favorable one for the competing railway, and the result was, the towns that had been served by the canal were served by the railway, which was thus in a position to take away even the local traffic of the canal. For some time it looked as though canal and canalized river navigations must come to an end, for although heavy goods could be carried very cheaply on canals, and with respect to the many works and factories erected on the canal banks, or on bases connected therewith, there was with canal navigation no item of expense corresponding to the cost of cartage to the railway stations, yet the smallness of the railway rates for heavy goods, and the greater speed of transit, were found to be more than countervailing advantages. But when private individuals have embarked their capital in an undertaking, they do not calmly see that capital made unproductive, nor do they refrain from efforts to preserve their dividends, and thus canal companies set themselves to work, to add to their position of mere owners of water highways, entitled to take toll for the use of those highways, the function of common carriers, thus putting themselves on a par with the railway companies, who, as no doubt is within the recollection of our older members, were in the outset legalized only as mere owners of iron highways, and as the receivers of toll from any persons

who might choose to run engines and trains thereon—a condition of things which was altered as soon as it was pointed out that it was utterly incompatible, either with punctuality or with safe working. This addition to the legal powers of the canal companies, made by the Acts of 1845 and 1847, has had a very beneficial effect upon the value of their property, and has assisted to preserve a mode of transport competing with that afforded by the railways. Further, the canal proprietors have from time to time endeavored to improve the rate of transport, and with this object have introduced steam in lieu of horse haulage, and by structural improvements have diminished the number of lockages. Many years before the period we are considering, there was employed, to save time in the lockages, and to economize water, the system of inclined planes, where, either water borne in a traveling caisson, as on the Monklands incline, or supported on a cradle, as in the incline at Newark, in the State of New Jersey, the barges were transferred from one level to another; but an important improvement on either of these modes of overcoming a great difference of level is the application of direct vertically lifting hydraulic power. A notable instance of this system was brought before the institution in a paper read on the "Hydraulic Canal Lift at Anderton, on the River Weaver," by S. Duer,* and another instance exists on the Canal de New Fosse, at Fontinettes, in France, the engineers being Messrs. Clark and Standfield, who have other lifts in progress. The system reduces the consumption of water, and the expenditure of time to a minimum.

With respect to canalized rivers, the difficulty that must always have existed when these rivers (as was mostly the case) were provided with weirs to dam up the water for giving power to mills, has been augmented of late years by the change in the character of floods. It has frequently been suggested that in these days of steam motors in lieu of water power, and of railways in lieu of water carriage, the injury done by obstructing the delivery of floods is by no means compensated by the otherwise all but costless power obtained, or by the preservation of a mode of transport competing with railways. It has thereupon been suggested that it would be in the interests of the community to purchase and extinguish both the manufacturing and the navigating rights, so as to enable the weirs to be removed, and free course to be provided for floods. It need hardly be said, however, that if means could be devised for giving full effect to the river channels for flood purposes, while maintaining them for the provision of motive power and of navigation, it is desirable that this should be done. The great step in this direction appears to be the employment of readily, or, it may be, of automatically movable weirs. Two very interesting papers on this subject, by Messrs. Vernon Harcourt and R. B. Buckley, were read and discussed in the session 1879–1880. These dealt, I fear exclusively, with foreign—notably with French and Indian—examples. I say I fear, not in the way of imputing blame to the authors for not having noticed English weirs, but because the absence of such notice amounts to a confession of backwardness in the adoption of remedial measures on English rivers. An instance, however, of improvement since then has been the construction by Mr. Wiswall, the engineer to the Bridgewater Navigation Company (on the Mersey and Irwell section of that navigation), of the movable Throstle Nest weir at Manchester. It does seem to me that, by the adoption of movable weirs, rivers in ordinary times may be dammed up to retain sufficient water to admit of a paying navigation, and water for the mills on their banks, while in time of flood they shall allow channels as efficient for relief as if every weir had been swept away. But the great feature in late years of canal engineering is not the preservation or improvement of the ordinary internal canal, but the provision of canals, such as the completed Suez Canal, the Panama Canal in course of construction, the contemplated Isthmus of Corinth Canal—all for saving circuitous journeys in passing from one sea to another—or, in the case nearer home, of the Manchester Ship Canal, for taking ocean steamers many miles inland.

But the old fight between the canal engineer and the railway engineer, or,

* "Minutes of Proceedings," Inst. C. E., vol. xlv., p. 107.

more properly speaking, between the engineer when he has his canal "stop" on, and the same individual when he has his railway "stop"—(you will see that I am borrowing a figure, either from *Dombey & Son*, where Mr. Feeder, B. A., is shown to us with his Herodotus "stop" on, or, as is more likely, I am thinking of the organs to be exhibited in the Second Division, "Music," of that exhibition of which I have the honor to be chairman)—I am afraid this is a long parenthesis, breaking the continuity of my observations, which related to the old rivalry between canal and railway engineering—I was about to say that this rivalry was revived, even in the case of the transporting of ocean vessels from sea to sea, for we know that our distinguished member, Mr. Eads, is proposing to connect the Atlantic and the Pacific Oceans by means of a ship railway across the Isthmus of Panama. He suggests that the largest vessels should be raised out of the water, in the manner commonly employed in floating docks, and should then be transferred to a truck-like cradle on wheels, fitted with hydraulic-bearing blocks (this being, however, not a new proposition as applied to graving docks), so as to obtain practical equality of support for the ship, notwithstanding slight irregularities in the roadway, while he proposes to deal with the question of changes of direction by the avoidance of curves and by the substitution of angles, having at the point of junction of the two sides, turntables on which the cradle and ship will be drawn; these can be moved with perfect ease, notwithstanding the heavy load, because the turntable will be floating in water carried in circular tanks. The question of preserving the level of the turntable, whether unloaded, partially loaded or loaded, is happily met by an arrangement of water ballast and of pumping. I cannot pass away from the mention of Mr. Eads' work without just reminding you of the successful manner in which he has dealt with the mouth of the Mississippi, by which he has caused that river to scour and maintain a channel 30 feet deep at low water, instead of that of 8 feet deep, which prevailed there before his skillful treatment. Neither can I refrain from mentioning the successful labors of our friend Sir Charles Hartley, in improving the navigation of that great European river, the Danube. I am sure we are all rejoiced to see that one of the lectures of the forthcoming series, that on "Inland Navigation," is to be delivered by him, and I do earnestly trust he will remember it is his duty to the institution not to leave important and successful works unreferred to because those works happen to be his own. I regret that time does not admit of my noticing the many improved machines for excavating, to be used either below water or on dry land. I also regret, for similar reasons, I must omit all mention of ship construction, whether for the purposes of commerce or of war, a subject that would naturally follow that of rivers and of ship railways and canals, and would have enabled me to speak of the great debt this branch of civil engineering owes to the labors of our late member, William Froude, and would have enabled me also to deal with the question of material for ships, and with the question of armor-plating, in which, and in the construction of ordnance, our past-president, Mr. Barlow and myself, as the two lay members of the ordnance committee, are so specially interested.

The mention of the armor-plates inevitably brings to our minds the consideration of ordnance, but I do not intend to say even a few words on this head of invention and improvement—a topic to which a whole evening might well be devoted—because, only three years ago, my talented predecessor in this chair, Sir William Armstrong, made it the subject of his inaugural address, and dealt with it in so masterly and exhaustive a style as to render it absolutely impossible for me to usefully add anything to his remarks. I cannot, however, leave this branch of the subject without mentioning, not a piece of ordnance, but a small arm, invented since the date of Sir William's address. I mean the Maxim machine gun. This is not only one of the latest, but is certainly one of the most ingenious pieces of mechanism that has been devised. The single-barrel fires the Martini-Henry ammunition; the cartridges are placed in loops upon a belt, and when this belt is introduced to the gun, and some five or six cartridges have been drawn in by as many reciprocations of a handle, the gun is ready to com-

mence firing. After the first shot, which must be fired by the pulling of a trigger in the ordinary way, the gun will automatically continue to send out shot after shot, until the whole of the cartridges on the belt are exhausted; and if care is taken before this happens to link on to the tail of the first belt the head of a second one, and another belt to this, and so on, the firing will be automatically continuous, and at a rate anywhere between one shot per minute and six hundred shots per minute, dependent on the will of the person in charge of the gun, the whole of the operations of loading, firing and ejecting the cartridge being performed by the energy of the recoil. This perfectly automatic action enables the man who works the gun to devote his whole attention to directing it, and as it is carried on a pivot, and can be elevated and depressed, he can, whilst the gun is firing, aim the bullets to any point he may choose.

Since 1862 the power of defending seaports has been added to by the application of submarine mines, arranged to be fired by impact alone, or to be fired on impact when (under electrical control) the firing arrangement is set for the purpose, or to be fired electrically from the shore by two persons stationed on cross-bearings, both of whom must concur in the act of explosion. These mines are charged with gun-cotton, the development of which owes so much to Sir Frederick Abel, while for purposes of attack the same material, not yet in practical use for shells, is taken as the charge for torpedoes, which are either affixed to a spar or are carried in the head of a submerged cigar-shaped body. By a compressed air or by a direct steam impulse arrangement these weapons are started on their course and are directed, and then the running is taken up by their own engines operating on screw propellers, driven by a magazine of compressed air contained in the body of the torpedo. Means are also provided to maintain the designed level below the water surface. The torpedo may either be projected from the war ship itself, or from one of those launches which owe their origin to our member, Mr. John Isaac Thorneycroft, who first demonstrated the feasibility of that which was previously considered to be impossible, viz., the obtaining a speed of twenty miles and over from a vessel not more than 80 feet long. Experiments have been carried on in the United States, by Captain Ericsson, to dispense with the internal machinery of the torpedo, and to rely for its traverse through the water upon the original impulse given to it by a breech-loading gun, carried at the requisite depth below the water level in a torpedo boat. This gun, having a feeble charge of powder at a low gravimetric density, fires the torpedo, and, it is said, succeeds in sending it many yards, and with a sufficient terminal velocity to explode the charge by impact. Also, in the United States, experiments have been made with a compressed air gun of 40 feet in length, and 4 inches in diameter (probably by this time replaced by a gun of 8 inches in diameter), to propel a dart through the air, in the front of which dart there is a metallic chamber containing dynamite. Although, no doubt, the best engineer is the man who does good work with bad materials, yet I presume we should not recommend any member of our profession to select unsuitable materials with the object of showing how skillfully he can employ them. On the contrary, an engineer shows his ability by the choice of those materials which are the very best for his purpose, having regard, however, to the relative facilities of carriage, to the power of supply in sufficiently large quantities, to the ease with which they can be worked up or built in, and to the cost.

Probably few materials have been found more generally useful to the civil engineer, in works which are not of metal, than has been Portland cement. It should be noticed that during the last twenty-two years great improvements have been made in the grinding and in the quality of the cement. These have been largely due to the labors in England of our member Mr. John Grant, to the labors of foreign engineers following in his footsteps, and to the zeal and intelligence with which the manufacturers have followed up the question, from a scientific as well as from a practical point of view, not resting until they were able with certainty to produce a cement such as the engineer needed. I do not know that there is very much to be said in the way of progress (so far as the finished results are concerned) in the materials

which Portland cement and other mortars are intended to unite. Clean gravel and ballast and clean sand are, I presume, very much the same in the year 1884 as they were not only in the year 1862, but as they were in the year 1. The same remark applies to stone and to all other natural building materials; and, indeed, even the artificial material, brick, cannot in these days be said to surpass in quality the bricks used by the Romans in this island nineteen hundred years ago, but as regards the mode of manufacture and the materials employed there is progress to be noted. The brick-making machine and the Hoffmann kiln have economized labor and fuel, while attempts have been made, which I trust may prove successful, for utilizing the clay, which is to be found in the form of slate in those enormous mounds of waste which disfigure the landscape in the neighborhood of slate quarries. Certain artificial stones, moreover, appear at last to be made with a uniformity and a power of endurance, and in respect of these qualities, compare favorably with the best natural stone, and still more favorably having regard to the fact that they can be made of the desired dimensions and shape, thus being ready for use without labor of preparation.

Reverting to natural materials, there remains to be mentioned that great class, timber. In new countries the engineer is commonly glad to avail himself of this material to an extent which among us is unknown. For here, day by day, owing to the ready adaptability of metals to the uses of the engineer, the employment of wood is decreasing. Far, indeed, are we from the practice of not more than a hundred years ago, when it was not thought improper to make the shell of a steam-engine boiler of wooden staves. The engineer of to-day, in a country like England, refrains from using wood. He cannot cast it into form, he cannot weld it. Glue (even if marine) would hardly be looked upon as an efficient substitute for a sound weld; and the fact is that it is practically impossible to lay hold of timber when employed for tensile purposes so as to obtain anything approaching to the full tensile strength. If it be desired to utilize metals for such a purpose, they can be swollen out into appropriate "eyes" to receive the needed connection; but this cannot be done with wood, for the only way of making an enlarged eye in wood is by taking a piece that is big enough to form the eye, and then cutting away the superfluous portion of the body. Moreover, when too much exposed to the weather, and when too much covered up, wood has an evil habit of rotting, compared with the rapidity of which mode of decay the oxidizing of metals is unimportant. Further, one's daily experience of the way in which a housemaid prepares a fire for lighting is suggestive of the undesirability of the introduction of resinous sticks of timber, even although they may be large sticks, into our buildings. Many attempts, as we know, have been made to render timber proof against these two great defects of rapid decay and of ready combustibility, and as it appears to me, it is in these directions alone one can look for progress in connection with timber. With respect to the first, it was only at the last meeting of the institution we presented a Telford medal and a Telford premium to Mr. S. B. Boulton, for his paper "On the Antiseptic Treatment of Timber," to which I desire to refer all those who seek information on this point. With respect to the preservation from fire of inflammable building materials, the processes, more or less successful, that have been tried are so numerous that I cannot even pretend to enumerate them. I will, however, just mention one, the asbestos paint, because it is used to coat the wooden structures of the inventions exhibition. To the employment of this, I think, it is not too much to say those buildings owed their escape, in last year's very dry summer, from being consumed by a fire that broke out in an exhibitor's stand, destroying every object on that stand, but happily not setting the painted woodwork on fire, although it was charred below the surface. I do not pretend to say that a surface application can enable wood to resist the effects of a continued exposure to fire, but it does appear that it can prevent its ready ignition.

Leaving the old world materials of stone and wood, let us come, not only to the bronze age, but to the iron age, and direct our attention during a few minutes to the improvements which, in twenty-two years, have been made—and

first to deal with that form of iron known as steel. I am aware that I am laying myself open to a charge of having committed a most tremendous "bull," but I am prepared to defend my form of speech, on the very strong ground that no one can say, speaking as a metallurgical chemist, where the dividing line is between commercial iron and commercial steel, for it is quite certain there is material which would be currently bought and sold and used as steel, which is more near to pure iron than is other material which would be commercially bought and sold and used as iron. It is now nearly eight years since I delivered, at the Royal Institution, a lecture on "The Future of Steel," and every year that has passed has justified the opinions I then ventured to put forward as to the way in which steel made by fusion would supersede iron made by the puddling process; and I am not afraid to repeat my prophecy that the time will come when the use of iron made by that process will be restricted to the manufacture of the small articles produced by the hand labor of the village blacksmith, for whose art its plastic character and ready power of welding eminently fit it. Probably the first great revelation in steel manufacture was the exhibition of the ingots, with other products shown by Krupp in the '51 exhibition; it soon became known, however, that these exhibits, after all, gave us no further information than this, viz., that it did not follow because the limit of the charge of a crucible might be 50 or 60 lbs., the limit of the size of the ingot must also be 50 or 60 lbs.; in fact, the world was shown that more than one pot of steel might be discharged into the same ingot mould; indeed, that hundreds of pots might be. Do not imagine for one moment I am depreciating this step. It was an enormous one at the time when the production of fused steel involved the employment of the crucible. But, according to my judgment, the making of steel in crucibles is not so satisfactory a mode of obtaining uniformity in large masses, as is either of the other two great systems of manufacture, I mean the Bessemer and the Siemens, the two processes which have changed the whole complexion of the iron industry. For years after 1862 we had papers at this institution upon the question of steel rails, and we had it solemnly stated that the suggested economy in using these was an apparent economy only, for when interest was taken into account, having regard to the extra cost of steel over iron, which must always prevail, it would never pay to employ steel rails, and the true function of steel in the permanent way would be to restrict its use to points and crossings. Now, it would be difficult to induce anyone to believe that an engineer was serious if he specified for wrought-iron rails, as it would be known that he would have to pay more money to obtain this inferior material. Important as the subject is, time compels me to refrain from further allusion to it, and forces me to conclude this head of my address with the physician's (Abernethy) well-known advice to his patients, "Read my book," *i. e.*, my lecture at the Royal Institution, to which, however, I must add one word, and that is, I must here refer to the important improvement made since the date of that address by the process of Messrs. Thomas and Gilchrist, by which it has been rendered possible to employ successfully, in the production of steel, iron derived from ores which, prior to the date of this invention, had been found wholly inapplicable for the purpose.

In the manufacture of pig iron, improvement has been effected by increasing the dimensions of the furnaces, and (thanks to Mr. Cowper in the outset, followed by others) by increasing the temperature of the blast, and by the closer application of chemistry to the industry, by the total closing of the bottom of the furnace, and by the increased use of the waste gases. From these improvements an economy and a certainty of production have resulted, leaving little to be desired; while it is to be hoped that another waste product—that of blast furnace slag—will be converted to various useful purposes.

I have varied the usual order by taking the iron age before the bronze. To revert to the bronze—the mysterious influences that a very small percentage of some material will exercise upon the quality of the great bulk of another material, with which it may be united, are well shown in the case we have been considering—that of steel—where a few tenths of one per cent. of carbon added to the iron suffices to change the iron

into steel. We are not surprised, therefore, when we find that other metals may have their qualities improved, for many useful purposes, by judicious alloy; and in this way the metal, copper, so long used in its alloyed condition of "gun-metal," has within the last few years been still further improved by alloying it with other substances, and thus making it into the now well-known articles "phosphor bronze" and "manganese bronze"—very useful materials to those of our members engaged in the construction of machinery. So closely allied to the consideration of the nature of a material is that of the means of producing it in the desired form, that one naturally passes thereto. As long as small masses had to be dealt with, and as long as those masses were of a plastic character, it was possible to successfully employ the hand-hammer, the sledge-hammer, and, later on, the steam-hammer; but with the increased dimensions of the main shafts of engines, and of the solid forgings for the tubes of cannon, obtaining at the present day, and having regard to the fact that these are composed of steel, the operations of light steam-hammers are absolutely harmful, tending to produce internal flaws, and the blows of even the heaviest class of hammers are not so efficacious as is pressure applied without blow. I think the time is not far distant when (following the lead of Sir Jos. Whitworth) all steel in its molten state will be subjected to pressure, not with the object of making the metal more dense, but with the object of diminishing the size of any cavities containing imprisoned gases; if this is not done, then some other mechanical means will be employed to get rid of the cavities altogether, and thus to produce (without variations in the constituents of the steel) a casting that shall be practically if not absolutely free from blowholes, and so that such casting, when afterwards forged by pressure and not by percussion, may be thoroughly trusted to contain no latent defect. One of the difficulties that was foreseen in the outset of the employment of steel for tires, was the difficulty of welding the ends of a steel tire bar, after it had been bent into the hoop form. I was, I believe, one of the very earliest to suggest the making of tires in the hoop form, and so not only to avoid the cost and risk of welding, but also to avoid the waste on each tire bar, arising from what was known as the "crop end." I read a paper on this subject before section G of the British Association, at the Birmingham meeting in 1865, and I then prophesied that, in a very few years from that time, a welded tire would be unknown—a prophesy which has been amply fulfilled; but I also pointed out that, so far from its being the right way to set about the manufacture of a hoop by beginning with a straight bar, then bending it, and then welding it, the manufacture in the hoop form would be the proper one to adopt, even if the object were eventually to produce a straight bar, such as a rail; for if this were done the relling would be continuous, and there would be no "crop end," no waste therefrom, and no fear that in order to render the waste as little as possible, there would be retained at the ends of the rail—its most vulnerable parts—metal of an inferior character. In this same paper I showed that the right way of making boiler-shells and boiler-flues would be by the hoop system and by endless rolling, thereby avoiding the longitudinal seams, which, after the very best has been done that can be done, reduce the effective strength of the boiler plate by one-fourth or one-third, and commonly reduce it by one-half. I will refer my hearers to Vol. XX. of the *Engineer*, p. 200, where the paper and the accompanying diagrams are given.

The subject of steam boilers brings one naturally to the consideration of that which still remains the great source of motive power—the steam engine. Here since 1862 it is difficult to point out to any great substantive novelty, but these machines have been more and more scientifically investigated, and the results of such investigation have been practically applied, and have been attended with the anticipated advantages. The increase in initial pressure, the greater range of expansion, the steam-jacketing of the vessels in which the expansion takes place, have all led to economy, so that double-cylinder, non-condensing engines are now currently produced, which work with a consumption of only $2\frac{1}{4}$ lbs. of coal per gross indicated horse-power, or 2.7 lbs. per horse-power delivered off the crank shaft, equal to eighty-three millions of

duty on the Cornish engine mode of computation; and when these high results are augmented by the employment of surface condensation, an indicated horse-power has been obtained for as little as 1½ lb. of coal, and it is commonly obtained, in daily work, for from 2 lbs. to 2½ lbs. But the engineer using steam as his vehicle in a heat motor still has to submit to the chagrin of seeing the largest portion of the heat pass away unutilized. This defect has for years attracted the attention of scientific engineers. Indeed, we know that more than thirty years ago our lamented friend, William Siemens, devoted his great powers to the production of a regenerative steam engine by which he hoped to decrease this loss, but at that time he was not successful in producing a practical machine. The labors of those who, following Stirling, have endeavored, by employing air as the vehicle of heat, to obtain better results, have succeeded in producing very economic machines, and machines of practical utility, but hitherto only applicable where small power is required. There is, however, another form of heat motor which, while vainly essayed during fifty years, has within the last eight years come into common use, and the application of which in cases requiring anything up to 30 indicated horse-power is daily increasing. I need hardly say that I allude to the gas engine. By a happy change in the mode of burning the mixture, and of utilizing the heat thereby generated, the injurious shock of the early forms of gas engine, and the large consumption of gas which caused these earlier forms of engine to be discarded after trial, were obviated, a notable instance being in the engine propelling the fan that ventilates this room, which, after a short time, was pulled out and replaced by an hydraulic engine. According to the *Mechanics' Magazine* of August 10, 1866, page 87, the French engineer who tried a Hugon gas engine, found that 74 cubic feet of gas per indicated horse-power per hour were required; this is now replaced by the 20 to 23 feet per indicated horse-power consumed in the engines of the present day. With the low price of gas commonly prevalent in England, this consumption does not cost more than some seven-eighths of a penny per horse per hour. I am aware it may be said that with coal, even at the London prices of £1 per ton, I might use a steam engine having the low economy of 8½ lbs. of coal per indicated horse-power per hour before I should be called on to spend seven-eighths of a penny per indicated horse-power per hour for fuel; you would be astonished to hear, however, that in an investigation instituted last year by the corporation of Birmingham, when considering whether they should approve of a proposal to lay down power-distributing mains throughout their streets, it was found on indicating some six non-condensing steam engines taken indiscriminately from among users of power, and ranging from five nominal horse-power up to thirty nominal horse-power, that the consumption in one instance was as high as 27.5 lbs., while it never fell below 9.6 lbs., and the average of the whole was as much as 18.1 lbs. This heavy consumption largely arose from a prevalent defect, one I have frequently pointed out, that of too great cylinder capacity; for unless a non-condensing engine is admirably designed, and made with the object of using very high expansion, there is nothing so wasteful as the employment of that which the buyer of an engine looks upon as an advantage—very great cylinder capacity. The result of such a construction being that the initial pressure of the steam, in the cylinder of the ordinary small-power non-condensing engine, is not more than 20 to 30 lbs. above atmosphere, a condition of things wholly incompatible with economy. But even assuming that the user of a gas engine were entitled to compare it with a non-condensing steam engine consuming only some 5 lbs. of coal per indicated horse-power per hour, and demanding, therefore, at 1s. per cwt., only one half-penny for the purchase of coal, this difference in cost is well repaid by the saving of boiler space, of the wear and tear, and of the renewal of the boiler, of the consumption of coal while getting up steam, and during meal times, and the saving in the engineer's or stoker's wages; and on public grounds there are the advantages of freedom from boiler explosion, and of cessation of smoke production.

I have spoken of gas engines hitherto, as though, like hot-air engines, they were

necessarily restricted in their dimensions, but this is not so; engines are now being made to develop 50 horse-power; and further, be it remembered, that when used on a large scale, so that it would pay to have an attendant devoting his whole time, there is no need to work them with illuminating gas from the street mains, they can be driven by producer-made gas on Dowson's system, and when worked in this way a pound and a half of "culm" will give one horse-power, and one lad is sufficient to manage a gas-producing apparatus of a size adequate to provide for engines developing 300 indicated horse-power. I ventured to say, at the meeting of the British Association at York, in 1881, when giving a partial review of that which had happened in engineering in the fifty years from the foundation of the association, that unless some wholly unexpected improvement were made in the steam engine, those who lived to see the celebration of the centenary of the association in 1931, would find the steam engine had become a curiosity, and was relegated to museums, for I could not believe steam would continue to be the vehicle for transmitting heat into work.

A motor has been recently tried where no fuel is employed directly, but where a boiler, being filled with water and steam under pressure, has its heat maintained by exposing caustic soda, contained in a vessel surrounding the boiler, to the action of the waste steam from the engine, the result being that, as the moisture combines with the caustic soda, a sufficient heat is developed to generate steam and keep the engine working for some time. The trials have been made with the motor for propelling a launch, and I believe with one for working a tram-car. It may be we have here a source of power in a portable form, useful for the purposes I have mentioned, and for others analogous thereto. It hardly needs to be said fuel has eventually to be employed to drive off the moisture from the soda, and thus to bring it back to its caustic condition.

I cannot pass away from this brief allusion to heat motors without expressing my gratitude to those lecturers who addressed us on "The Mechanical Applications of Heat" last session, and especially to our member, Mr. William Anderson, for his lecture in that course on "The Generation of Steam and the Thermo-dynamic Principles involved." Let me tell those of you who do not already know it that Mr. Anderson has still three lectures to deliver, out of a course of six lectures which he is giving at the Society of Arts, on "The Conversion of Heat into Useful Work," and permit me to advise all those members of this institution who can possibly do so to attend (as I hope to be able to do) the remainder of those most clear and instructive lectures.

There is one indisputable heat motor I have omitted, viz., that wherein power is obtained directly from the sun's rays. Attempts have been made during the last twenty-two years in this direction, but we enjoy so little powerful sunshine in England, and the question is still in such a thoroughly experimental stage, that I think I must not take up your time by any consideration of it. With respect to other motors, viz., those driven by wind or by water, not commonly looked upon as heat motors (although, in truth, there would be no such agencies without heat), but on these there is not time to say much, I will merely call your attention to the improvement in water-wheels in France, an improvement by which it is asserted that as much as 85 per cent. of all the energy residing in a low fall of water has been converted into power; a result due to the decreasing of the speed of the periphery of the wheel, and to the making of the buckets very narrow and of great depth. In turbines, also, there has been considerable development in these twenty-two years, and they now take their place as very efficient motors, possessing many advantages, where, on the one hand, a very high fall of water has to be utilized, or where, in the case of a low fall, great difference in the working head, and in the level of the tail water, have to be provided for.

With respect to the power of the tide, I, for one, have been very much fascinated with the scope there appeared to be for engineering in utilizing tidal power, especially where there was a great ebb and flow, and I have on former occasions expressed some sanguine opinions as to the practical use that could be made of this source of power; but, being called upon to look into the question, I found that

as in these days of competition, very few businesses needing motive power can allow their plant to remain idle for nearly half the working day, and that as there is an objection to remedying this condition of things by working when possible, both during the night tide and during the day tide, this was an obstacle in most cases to the use of tidal power. Further, when it was sought to preserve continuity of action by providing a series of reservoirs, the outlay needed was so large that the mere interest on it would pay for the fuel for the steam engine. I am afraid, therefore, that, except for certain cases—such as pumping of water into a reservoir, or charging of so-called storage batteries, or matters of that kind not connected with ordinary manufacture—this source of power is not likely to compete commercially with heat motors until coal is very much dearer. The periodic intermittency being a sufficient bar to the employment of tidal motors, it is not to be wondered at that the (proverbial) uncertainty of the wind causes motors which have to be driven by it to be disregarded as substitutes for the steam engine. I have, however, said elsewhere I think it is well worth considering whether wind motors could not be employed as adjuncts to steam engines, diminishing the load upon them or laying them idle altogether, according to whether there was a light or a strong breeze blowing.

The uncertainty as regards the obtaining a sufficient breeze, which prevents the wind from being a trustworthy source of power, aggravated by the further uncertainty as to the direction in which the breeze would blow when it did come, has rendered the air, as a medium for navigation, even more untrustworthy. During the last few years, however, a new locomotive agent has been prominently brought forward—I mean a balloon capable of being propelled and steered, or, as it has been termed, a "dirigible" balloon. Many persons have fancied that it is impossible to propel a balloon through the air, but this, as I need hardly tell those who understand mechanics, is entirely a fallacy. The reasons why the early attempts to steer balloons failed, were practical and not theoretical, and they have been removed by recent mechanical improvements. The first really successful effort was made by M. Henri Giffard, the ingenious inventor of the injector, all reference to which I have omitted, however, because of its being slightly anterior to 1862. In 1852 this gentleman ascended in an elongated balloon propelled by a steam engine working a screw propeller; and he was followed twenty years later by M. Dupuy de Lome (the Government naval architect of France), who, however, used hand power. The speed through the air in these trials was about six miles an hour, and the steering power was fully obtained. Taking these and other experiments as data, my friend and fellow member, Dr. Pole, to whom I am indebted for the information on this subject, published in our "Proceedings" in 1882 a full investigation of the problem, which led him to believe that a velocity of 25 to 30 miles an hour might be attained; and since that time further trials have been made in France by Messrs. Tissandier, Renard, and Krebs, who, using electric power, have already accomplished half the predicted speed, with a promise of much further development, when more experience has been gained with the practical details. I fear that the rapid and changeable motion of the medium in which balloons have to move will prevent this mode of locomotion from ever having a wide application, but there may undoubtedly be particular circumstances in which it would be useful, such, for example, as the exploration of new countries, or as the present Egyptian campaign. I strongly suspect that if our lively neighbors, instead of ourselves, had been invading the Soudan, they would long before this have had a "dirigible" balloon looking down into Khartoum. Let me refer all those who take an interest in this question to an earlier article by Dr. Pole, which was published in the *Quarterly Review* of July, 1875. This article is still considered a "classic" on the subject.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The following is the list of officers for 1885:

President: FREDERICK GRAFF.

Vice-Presidents: GEORGE S. GREENE, JR.; THOMAS J. WHITMAN.

Secretary and Librarian: JOHN BOGART.

Treasurer: J. JAMES R. CROES.

Directors: THEODORE COOPER, WILLIAM R.

Hutton, Walter Katte, O. Chanute, F. W. Vaughan.

February 4th. 1885.—President Frederick Graff in the chair. The death, on February 3d, of Theophilus Sickels, M. Am. Soc. C. E., was announced, and the appointment of a committee to prepare a memoir was authorized.

The paper by E. Sweet, M. Am. Soc. C. E., State Engineer of New York, on the Radical Enlargement of the Artificial Waterway between the Lakes and the Hudson River, was discussed.

Mr. Sweet's paper refers to the fact that for twenty years after the enlargement of the Erie Canal to its present capacity it held a prominent place in our internal commerce. Contemporaneous with its enlargement began the wonderful development of the railway system which has since absorbed much of our best engineering, executive and administrative ability, and much of the available capital of our country, to the exclusion of water routes. The Erie Canal, unchanged for thirty years, except in its gradual deterioration, with unimproved equipment and mode of operation, is gradually losing its capacity for usefulness and its influence upon the problem of transportation. Thirty years ago the Erie Canal carried nine-tenths of the freight between Buffalo and New York; now it carries less than one-fifth of it. Minor improvements have been urged, but its inadequacy to meet the present and prospective wants of northwestern commerce has become so manifest as to demand a far more radical improvement.

To become the permanent highway for this commerce, the canal must have capacity to carry from Lake Erie to the deep waters of the Hudson the largest vessels now navigating the lakes, or as those vessels may probably be enlarged in the future. The canal should be at least 18 feet deep, 100 feet wide at bottom, and with locks 450 feet long and 60 feet wide. It must receive its water from the lake and discharge it into the Hudson. While the changes required are radical, they are entirely practicable and involve no very serious difficulties. The essential change of profile would be in extending the Rome level westward to Lock 57, near Newark, in Wayne County. The only difficulty of importance would be at and near the crossing of the Seneca River, and this would be reduced by a change of location at that point. Probably the best results would be obtained by an entirely new route from Syracuse eastward. From the vicinity of Utica, the Mohawk river should be canalized to the Hudson.

The plan may be summarized as the widening, deepening and necessary rectification of the worst curvatures of the present canal from Buffalo to Newark, about 130 miles: the construction of a new canal from Newark to Utica, about 115 miles; the canalization of the Mohawk river, from Utica to Troy, about 100 miles, and the improvement of the Hudson river from Troy to Four Mile Point, in Coxsackie, a distance of about 30 miles.

The elevation of the western level of the canal, being governed by the surface of Lake Erie, must secure the required depth wholly by deepening, while the profiles of the levels from Lockport east can be adjusted to meet the economical requirements that will be disclosed by detailed surveys.

The first level, from Buffalo to Lockport, will be 32 miles long. Descending from this level at Lockport, by two locks, each of about 25 feet lift, the second level of the canal will be reached. This level, 64 miles in length, will extend to Brighton, where, descending by two locks of about 24 feet lift, we reach the third level of the canal, extending from Brighton to Macedon, 20 miles; there, descending by a lock of about 20 feet lift, we reach the fourth level, extending from Macedon to Newark, 12 miles, where, by a lock of about 20 feet lift, is reached the level of the proposed new canal, to extend from Newark to Utica, about 115 miles, which will be the fifth and longest level of the new canal. From that point the Mohawk river (except at Little Falls and Cohoes, where combined locks will be required) can best be canalized through locks of 10 or 12 feet lift, making pools having an average length of about 5 miles each.

The construction of this great artificial river, more than 300 miles long, is a vast enterprise. Its cost may be roughly assumed at \$125,000,000 to \$150,000,000, and its probable tonnage at 20,000,000 to 25,000,000 of tons per annum.

The first requisite to the possible inauguration of this enterprise is a careful system of surveys. The canal should be under the control of the State of New York, and that State should without delay cause the necessary surveys and estimates to be made.

By means of the enlarged canal, cargoes would be carried from Chicago to New York in less time and at less cost than can now be done by canal from Buffalo to New York. With our widespread territory, cheap transportation is a necessity; and the Erie Canal should be given the necessary capacity to effectually secure that result.

A discussion followed which was to be continued on February 18th.

Engineers' Club of Philadelphia.—Record of Seventh Annual Meeting, January 10th, 1885.

President Ludlow delivered the Annual Address.

He drew a rapid picture of the universality of modern activity and means of communication, and briefly depicted the astonishment of an ancient philosopher, could he be recalled to participation in the life of to-day. The restless activity of mankind in every corner of the globe is directed by careful study and observance of natural laws. The specialization of knowledge naturally follows from its vast accumulations, but the engineer should be careful to thoroughly ground himself in fundamental principles, and maintain his acquaintance with kindred lines of thought. The progress of civilization has been effected by the discovery and utilization of natural laws, by means of which man has emerged from a

savage state and become the ruler of the earth, pressing into service all the powers of nature and using them to his advantage.

The engineering history of the past year was briefly gleaned over and the principal features mentioned, including the improvement of the Mississippi River and the construction of tidal harbors. The prospect of a revival of military engineering in America was touched upon, with the accompanying development of the metallurgical arts in the construction of guns and armor.

Under the general title of Sanitary Engineering in the broadest sense are included nearly all those branches which affect the existence and prosperity of cities; water supply, sewerage, bridges, street cleaning and paving, etc.

Speaking of Philadelphia engineering, he said: "While serious attention has been drawn to the improvement of its water supply, the prospect of actually entering upon the construction of the necessary works is vague and distant. The streets are covered with a pavement which, in all well-regulated cities, has long since been abandoned as costly to traffic, impossible to clean, permeable to street fluids, noisy and hopelessly disreputable. But one sound bridge across the Schuylkill exists. The Fire Department labors under the most serious disadvantages from lack of water. The city sewers are without means of flushing or cleansing, and many are so constructed as to waste their contents. In consequence the foul matters are either to a great extent stored up under the pavements to give out poisonous vapors, or saturate the soil in which the dwellings are constructed. . . . I look forward with the eye of hope to the future of the city; with smooth, noiseless, water-tight pavements, ample facilities for connecting the population of West Philadelphia with the old city; rapid transit of the population from point to point in decency and comfort, clean street., healthful homes, pure air and pure water I anticipate the reconstruction of the sewers upon a well considered and scientific system."

A project for the useful disposal of Philadelphia sewage upon the sandy and thirsty farms of New Jersey by means of intercepting sewers, a conduit beneath the Delaware and a distributing aqueduct eastward, was briefly sketched. The several scientific assemblies in Philadelphia during 1884 are mentioned, and in conclusion the Club is congratulated upon its great prosperity during the year, and a continuance for the future anticipated.

President, J. J. de Kinder; Vice-President, Joseph N. DuBarry; Secretary and Treasurer, Howard Murphy; Directors, T. M. Cleemann, Frederic Graff, Rudolph Hering, William Ludlow and Henry G. Morris.

ENGINEERING NOTES.

On the Relative Cost of Retaining-Walls built in Brick and in Columnar Basalt —By P. H. Kemper.—Mr. Kemper collected data from different public works, executed in brick and in basalt, of late years, and compared their actual cost prices. Basalt being the heavier material, a saving can be effected in the thickness of walls by employing this material. On the other hand, the adhesive strength of mortar on basalt is only about half of that on brick; but the superior weight restores in friction on the joints what may be lost in adhesion.

The author instances works executed, amongst others, the basalt wall of the lock at Flushing, where 27 per cent. was saved, and the quay-wall at Maasluis, where 11 per cent. was saved by building in basalt instead of in brick. For these works the actual cost of 1 cubic meter of basalt masonry was 16.40 florins; 1 meter cube of klinker brickwork, 18.88 florins; of stock-bricks, 17.67 florins. At the lock at Vreeswijek the price paid for basalt masonry was 15.90 florins, for brickwork 16.07 florins per cubic meter. The mortar used for basalt is sand concrete, at 12 florins per cubic meter; for brickwork, mortar of equal parts of shell-lime and Rhenish trass.

In walls up to 4 meters high the saving is not appreciable, but it rapidly increases with the height.

According to experience, columnar basalt is preferable to brick:—in walls exposed to damp, such as retaining-walls for quays and locks, where the water line is oscillating and the backing wet, also because basalt is of greater specific gravity; it is not liable to crumble away in frosty weather; its cost of maintenance is considerably less; and, in cases of settling and subsequent repairs, the material is not lost by the breaking out, but can be used again, which is not the case with brickwork. For large works and walls of great height basalt gives a better appearance than brick.

Proposed Railway Bridge across the Straits of Messina.—This design is put forward by the directors of the Novara-Pino and the Genoa-Acqui-Asti Railways. The bridge is to have three steel arches of 3,280 feet from center to center of piers and two half arches of 1,640 feet. The depth of the straits where the bridge crosses is 360 feet. The piers are to be built of granite in cement, founded on masses of granite thrown down into the sea, and brought up to within 65 feet of the surface. The piers are to be 33 feet high from water to springing level, and above this they are to be built of immense blocks of granite to a further height of 62 feet for the arches to abut against. The width of the piers is to be 236 feet, the clear span of the arches 3,083 feet, and their versed sine 328 feet. The thickness of the arch at the springing is 65 feet, which is reduced towards the crown. The road is carried on a longitudinal girder 10 feet in depth, connected with the arch by lattice bracing.

The width of the bridge is 65 feet at the center, widened out to 197 feet at the springing, to give lateral resistance to the structure and to enable it to withstand wind-pressure. The arch is to be erected without scaffolding or centers. Above each pier a temporary structure is to be erected similar to the lattice work between the horizontal girder and the arch but in an inverted position, and forming with the latter a pair of cantilevers. The operation of erecting the arch is

to be carried out thus: First, the arrangement above described is to be put up, consisting of the permanent lattice-work and the temporary inverted lattice, forming a cantilever on each side of the pier 690 feet in length. Next a length of 525 feet of the main arch is to be erected on each side of the pier. The erection is then to be extended a further length of 260 feet on each side. An additional length of 180 feet is then added, being as before supported by the longitudinal girder and lattice-work, which are kept in advance of the arch. Similar processes are repeated till a length of 1,230 feet on each side of the pier is in position, leaving a length of 410 feet on each side to complete the two half arches, or 820 feet in each arch; but this, being the lightest part of the structure, can be put together on the part already erected, and then rolled out and lowered into position.—*Abstracts of Inst. of Civil Engineers.*

IRON AND STEEL NOTES.

SEPARATION OF THE PHOSPHATES FROM SLAG AFTER THE THOMAS-GILCHRIST PROCESS.—The increasing adoption of the Thomas-Gilchrist process in Germany during the past few years had turned the attention of chemists in that country to the nature of the slag or refuse products of this new development of the iron manufacture. It was ascertained that amongst other things fully 20,000 tons of phosphoric acid were every year being thrown away with this refuse because no method had been discovered of separating it at a profit. Numerous experiments have been made during the last five years with a view to hitting upon a sufficiently cheap process, but hitherto these attempts were all unsuccessful. A few months ago, however, Professor Scheibler, of Berlin, succeeded in solving the problem. An analysis of the slag from the Thomas-Gilchrist process at one of the chief ironworks in Germany, showed its constitution to be as follows: Silicic acid, 6.23 per cent.; carbonic acid, 1.70 per cent.; sulphur, 0.56 per cent.; phosphoric acid, 19.33 per cent.; iron, 9.70 per cent.; manganese, 9.50 per cent; lime, 47.60 per cent.; and oxide alumina, 2.58 per cent. Other analyses did not materially differ from this; the quantity of phosphoric acid only varied between 15½ and 20 parts in the 100, while the silicic acid varied from 6 to 11 per cent., the proportion of lime being always nearly 50 per cent. According to the Scheibler process, only the earth phosphates and the silicates are brought into solution. The proportion of metallic oxides found in the solution is of no practical consequence, and thus the quantity of acid employed in the operation is reduced to a minimum. The phosphoric acid can be precipitated directly from the solution in the form of double basic phosphate of lime. It comes out in the shape of a powder in the finest state of division, and owing to the readiness with which in this form it is taken up by the roots of plants, these phosphates furnish at once a very valuable manure without any further treatment. On the other hand, the Scheibler process leaves the metallic substances and part of the earthy bases undissolved in the slag, and since the silicic acid is nearly all taken out with the phosphates, the refuse that remains after the operation furnishes a useful material for blast furnaces and other purposes.

THE KRUPP WORKS AT ESSEN.—The great iron and cannon founding establishment of Herr Krupp at Essen is constantly enlarging its space and *personnel.* In 1860 it contained but 1,764 workmen, and this number had increased by 1870 to 7,084, while at the present time it is over 20,000; if also the women and children dependent on the establishment are included, a population of no less than 65,381 is gathered together, of which 29,-000 persons are actually living in houses belonging to the works. The various departments of the Krupp undertaking are eight in number, and embrace the workshops at Essen, three collieries at Essen and Bochum, 547 iron mines in Germany, mines in the north of Spain, in the neighborhood of Bilbao; the smelting furnaces, a trial ground of 17 kilos. at Meppen for proving cannon, together with others at different places with an area of 7½ kilos. There are 11 smelting furnaces, 1,542 puddling and heating furnaces, 439 steam boilers, and 450 steam engines of 185,000 horse-power. At Essen alone the works connected with rolling stock comprise 59 kilos of rails 28 locomotives, 883 wagons, 69 horses, 191 trolleys, 65 kilos. of telegraph line, 35 telegraphic stations, and 55 Morse apparatus.

REMOVING RED-SHORTNESS FROM IRON.—The iron ore of Cornwall, Pennsylvania, is rich in copper and sulphur. The greater portion of the latter is removed by roasting; but the copper remains, and, in smelting, alloys the iron with from 0.75 to 1.25 per cent., according to analyses made by a prominent steel-works chemist. It is very red-short or brittle at a red heat—a fact so well established that the trademark of the North Cornwall furnaces is C. R. S., standing for Cornwall red-short. This metal is used in the Bessemer process, mixed with good hematite pig-iron to some extent; but until recently it has never been used in the open-hearth process, as the product has been too red-short for hammering or rolling. The iron being in large and cheap supply, it became desirable to remove the red-short property in a simple and economical manner; and with that end in view, some was sent during the last summer to Bellefonte, Pennsylvania, to be experimented with in the open-hearth furnace there. The first trial, conducted in accordance with the plan of Mr. James Henderson, was made with one-half weight of Cornwall No. 3 pig-iron, half rail crop-ends, and 5 per cent. of iron ore, charged upon silica brick at the rate of 60 pounds of brick per ton of metal. The bricks were burnt and were composed of ninety-five parts of sand and five parts of lime by weight. The bricks were charged on the hearth, which was highly heated and ready for charging the pig-iron. The latter was placed on them, and the crop-ends on the iron. By the time the pig melted, the brick became partly fused and stuck fast to the

bottom and remained there, and gradually melted away during the decarbonization of the metal on the top of them, and passed up through the metal, removing the red-shortness. The second trial was without the silica brick, and the metal was too red-short to be of use. The trials with the silica brick were continued over several weeks, using several car-loads of this metal with uniformly satisfactory results, whether all pig and ore without scrap was used, or pig, ore and scrap. When sand alone was used, it removed the red-shortness, but a portion adhered to the hearth, and gradually raised it, so that it became necessary to cut it out afterward with lime. The better way is, therefore, to mix the lime with the sand at the start, and keep the hearth from rising by accumulation. A brick made of ninety-five parts of sand and five parts by weight of lime, mixed with water containing glucose, in the proportion of twenty parts to one by bulk and air-dried, is the most suitable for the use, as the brick insures the hearth being kept at its normal state. The ingots made from the metal rolled directly into shapes equally well, and tested as well as others that had been previously bloomed. The Allentown Rolling-Mill Company rolled some of these ingots, and reports the elastic limit 42,867 pounds and the tensile strength 66,289 pounds per square inch, with 25 per cent. elongation in 8 inches. This metal, when rolled into plates, punches cold without cracking, and doubles over upon itself at all temperatures without any crack at the bends, and for boiler plates is in every way equal to those made from the most costly materials. From this, it appears that, while silica in excess prevents removal of phosphorus, it acts in a contrary way when used with iron containing copper, and removes the copper.—*Engineering and Mining Journal.*

RAILWAY NOTES.

Railway Brakes in Germany.—Some time since we announced that the Baden State Railways had adopted the Westinghouse brake. It is true this step was not taken until they had experienced the terrible calamity at Hugstetten, where 72 people were killed and over 100 injured, though in this they were no worse than most people, who are proverbially desirous of shutting the stable door when the horse is gone. It would seem, however, that the Wurtemburg State has decided to follow the example of Baden, in adopting the Westinghouse brake, and without waiting for the occurrence of a disastrous accident. The work is to be carried on as rapidly as possible, and all the Continental railways will no doubt before many years be equipped with automatic air pressure brakes. It is one of the many curious features in the brake question that no railway company appears to consider that the experience of other companies should affect their own action. So long as they have themselves escaped without any disastrous consequences, they maintain the hope that the experience of others will never be their own, and this, whether lives have been lost by the want of efficient brakes, or, on the other hand, saved by the special qualities of a particular appliance. So far as this country is concerned, there is just as little provision upon many lines for meeting a calamity like that of Penistone, as there was previous to the disastrous day in July, when that fearful accident took place.

Fireless Tramway Engines.—The system of tramway haulage by fireless locomotives has been tried on a very considerable scale in Batavia, and has given so much satisfaction that it is contemplated to extend it. The Batavia Steam Tramway Company owns a line divided into two portions; the first, from Batavia to Kramat, having a length of 8 kilometers (5 miles) laid with a double track of Demerbe grooved rails, and the second from Kramat to Muster Cornelis, having a length of 4½ kilometers of single track of Vignoles rails. The first piece is almost level, with the exception of short inclines of 1 in 32 over bridges; there are two long curves, and a number of short ones of 30 meters radius. The second section has a continuous gradient of 1 in 450. The haulage is affected by 21 fireless Lamm Francq locomotives, and five stationary boilers, the whole of which were manufactured by the Hohenzollern Locomotive Works, Dusseldorf. Two of the boilers are situated at Batavia, and three at Kramat, but one only is in work at each station at a time, the remainder being in reserve. They are worked 12 hours a day, and fill an engine every 1½ minutes during about 3 hours in the day, and every ten minutes at other times. An engine charged to a pressure of 12 atmospheres will draw two or three passenger cars from Batavia to Kramat, and from Kramat to Cornelis, up and down again to Kramat. Part of the line was opened in July, 1883, and from the last annual report it appears that the cost of haulage amounted last year to 23 cents per kilometer (7.4d. per mile), composed of the following items:

	Cents.
Driving engines	4 7
Heating boilers	2.3
Coals	14.0
Packing, lubricating, &c	2.0
Total	23.0

(5 cents = 1 penny).

More recently the cost of haulage has been only 17 cents per kilo. (5.24d. per mile), the price of coals being £2 per ton. The consumption of fuel was at first 6 kilogrammes per kilometer (21.3 lbs. per mile), but recently it has fallen to two-thirds of that amount. Repairs of boilers and engines have cost 2 cents per kilometer, and have consisted chiefly in re-turning the wheel tyres and renewing the felt on the boilers. Since the road has been completed, the receipts per month have amounted to 22,800 florins, and the total expenditure to 12,800 florins, leaving a net monthly profit of 10,000 florins (£800). The fare is 2½d. for four miles' run, or any part of it. The engines give every satisfaction. They are in native hands, and run constantly, with little attention and no breakdowns. Two more have been ordered, and will be shipped from Amsterdam this month. It is believed that with

a better road the expenses might be reduced to 50 per cent.

Electric Tram-Car.—A tram-car, fitted by Mr. A. Reckenzaun with secondary batteries and electro-motor, has now been running experimentally for some weeks at the works of the Electrical Power Storage Co. at Millwall.

The car is an old one, procured from one of the Metropolitan lines, and it has been drawn by horses between Greenwich and Westminster for many years. The body of this vehicle weighs 2¼ tons, and it accommodates 46 passengers. The accumulators are of a special type manufactured by the Storage Company, from the designs of Mr. Reckenzaun. Placed under the seats on long trays, which run on rollers for their speedy removal, they are out of sight, and the whole car internally and externally has the ordinary appearance. The motor and gearing—Reckenzaun's patents—are placed underneath the car, and occupy so little space that to an ordinary observer they are invisible. The speed may be varied from three miles to ten miles per hour.

The accumulators weigh 1¼ ton, the motor, gearing and accessories about ½ ton, bringing the total weight of motive power to about 1¾ ton for a car which, with its full complement of passengers, weighs itself 5½ tons; whilst the batteries, motor and gearing are capable of furnishing, at any desired moment, a power of sixteen horses, if required. This weight of motive power is compared with steam and compressed air locomotives weighing eight to ten tons, to do the same amount of useful work.

The line—4 ft. 8½ in. gauge—is 400 ft. long, forming a right angle of nearly equal sides, so that about half way a curve of 35 ft. radius has to be passed. From one end, as far as the commencement of the curve, the road is tolerably level; but with this curve commences an incline of 1 in 40, which rises gradually until it reaches a maximum of 1 in 17 nearly at the end of the up journey; thus, it is impossible to make a rush for the hill on account of the sharp curve intervening.

The running cost, including 15 per cent. depreciation on machinery and 50 per cent. on accumulators, is stated to be 3.5d. per car mile, or about one-half of the cost of horsing on tram lines. The car on the line at Millwall runs for two hours with one charge, starting, stopping and reversing every sixty seconds; and the accumulators can be replaced, it is said, almost as quickly as changing a pair of horses, by means of a trolly, which brings and removes the tray of cells, running on rollers. There are sixty of these accumulators, or thirty on each side. The load is distributed upon two small bogies, so that no objection may be raised on the part of tramway companies using light rails laid for horse-car traffic, and the old rolling stock can be utilized by putting the bogies which carry the motor under the car, and fitting the space under the seats for the reception of the accumulators. The car is lighted by four 20-candle power Swan lamps, and bell-pushes inside the vehicle enable the passengers to communicate with the conductor or driver by the ringing of electric bells.—*Journal of Society of Arts.*

ORDNANCE AND NAVAL.

The Imperial Brazilian Navy.—In a recent number of *Revista Maritima Brazileira* is given a general list of the ships of the Brazilian Navy, from which we extract the following particulars:

The largest ironclad is the Riachuelo, launched in 1883, and built by Messrs. Samuda. She has a tonnage of 5,800, is built of steel, and has steel armor 10 inches on the turrets and 11 inches on the side. Her indicated horse-power is 6,000, speed 16 knots, and she is armed with 4 Armstrong guns of 20 tons each, 6 of 5½ tons, and 15 Nordenfelt machine guns.

There are two ironclads, launched in 1876, of 3,600 tons each, named Solimoes and Javary. They are built of iron, and have iron armor 18 inches on the turrets and 12 inches on the side. Their speed is 12 knots, and they are each armed with 4 Whitworth guns of 25 tons each and 4 Nordenfelts.

Two smaller ironclads are also of iron hulls—the first, the Bahia, built in 1865, is 928 tons, has 4-inch armor on the turret, 3-inch on the side, and is armed with 2 Whitworth 7 inch guns; the Mariz a Barros is 1,196 tons, and has 4-inch armor, and carries 2 Whitworth 7-inch guns and 2 smooth bore 68-pounders. Both these vessels have a speed of 9 knots, and they are 18 years old.

A wooden ship, the Sete de Setembro, of 2,179 tons is plated with 4-inch armor, carries 4 Whitworth 9-inch guns and 4 Nordenfelts, and has a speed of 11 knots. She was launched in 1874.

There are, in addition to these, four small monitors for river service, built of wood plated with 4-inch armor, each carrying a 7-inch Whitworth gun, and having a speed of 7 knots.

As regards unarmored cruisers, the Brazilian Government has one of 4,000 tons building, of steel, to steam 15½ knots, to carry 4 Armstrong 12-ton guns, 10 small guns of 6-inch bore, and 12 Nordenfelts. They have also seven cruisers built of wood, of which four are between 1,400 and 2,000 tons, and are armed with Armstrong and Whitworth rifled cannon and Nordenfelts. Three vessels of about 750 tons each are armed with Whitworth guns. The speed of these wooden vessels ranges from 9 to 12½ knots, and most of them are of recent build.

Of vessels of smaller size, Brazil has seven wooden and five iron gunboats; and also five composite gunboats in course of construction. She has in course of construction at Jarrow, five torpedo boats built of steel, to steam 18 knots, and over 100 feet in length. Three torpedo boats are building by Messrs. Thornycroft, but of these the dimensions are not given.

Brazil provides also for the training of her seamen, a wooden brig, the Appendiz Marinhein, which completes the list.

BOOK NOTICES.

Publications Received.

Report of New York State Survey for 1884. James T. Gardner, Director. Albany: Weed & Parsons.

The Sanitary Engineer, Vol. X.

Professional Papers of the Signal Service No. XV.; Researches on Solar Heat.

Report of Meeting of Mississippi Levee Commissioners.

Ninth Report of the Committee on the Metric System of Weights and Measures.

Annals of Mathematics, Vol. I., No. 3.

REPORT ON THE MANUFACTURE OF COKE. By JOS. D. WEEKS. New York: David Williams.

This is a statistical report prepared for the Superintendent of Census. The descriptions of coking processes in European countries are given with exceptional fullness and with abundance of diagrams.

Much space is devoted to the history of coke manufacture as a special industry, and this is substantially a history of the manufacture of pig iron, as four-fifths of all the coke made is for iron manufacture only.

An interesting book for coke or iron producers.

STEAM MAKING, OR BOILER PRACTICE. By CHAS. A. SMITH, C. E. Chicago: The *American Engineer*.

The merits of this work are its eminently practical character, and the careful presentation of those fundamental principles upon which successful practice depends.

The Nature of Heat and Properties of Steam are treated in the first chapter; then follow in order—Combustion, Externally-Fired Boilers, Internally-Fired Boilers, Locomotive and Marine Boilers, Construction and Strength of Boilers, Heating Surface, Boiler Fittings.

The illustrations are of notable examples in use.

THE SELF INSTRUCTOR IN NAVIGATION. By W. H. ROSSER. London: James Imray & Son.

The preface of this book distinctly states that the Self Instructor is essentially practical, and not theoretical.

Problems involving all useful methods in determining a ship's place are propounded and solved, with a view to aiding the student in preparing to pass the "Board of Trade" examinations.

The work would well supplement the ordinary academic course as a work for practice.

ELEMENTARY TEXT-BOOK ON PHYSICS. By PROF. WM. A. ANTHONY and PROF. CYRUS F. BRACKETT. Part I. New York: John Wiley & Sons.

This is neither rudimentary physics nor physics for advanced classes. A knowledge of trigonometry is required in the student who begins it.

The scientific accuracy of the work is unquestionable, but to be a widely useful book it needs either considerable expansion in the treatment of the topics presented, or else a more rudimentary presentation of them.

A learner who has just completed the ordinary high school course of Natural Philosophy may study this book to advantage, but he will still need a text-book on Mechanics to obtain a fair knowledge of the practical bearings of Physical Science.

CURVE-TRACING IN CARTESIAN CO-ORDINATES. By WILLIAM WOOLSEY JOHNSON. New York: John Wiley & Sons.

This little book will prove a source of delight to students who have just completed the ordinary academic course of analytical geometry.

It will doubtless serve in many instances to stimulate the reader to a further investigation of the comprehensiveness of this delightful branch of study.

The treatise will be found easy reading to all who appreciate co-ordinate geometry, and both teachers and pupils will, we doubt not, consider it a profitable supplement to the ordinary course.

THE DESIGNING OF ORDINARY HIGHWAY BRIDGES. By J. A. L. WADDELL, C. E., B. A., &c. New York: John Wiley & Sons.

The author of this work has performed a good service for the engineers who design and construct bridges for common roads.

As bridges may be bought of responsible firms, just as mowing machines are, the class of engineers upon whom the responsibility of designing a bridge would fall is probably a small one. But there is no doubt that at times such a treatise as the present one is of great value.

Prof. Waddell has with great pains tabulated the dimensions down to the smallest details. The designing is restricted to the Pratt and Whipple systems, but it is urged that the great majority of American bridges are built on these systems.

The young engineer will find the study of the book a valuable aid to designing generally.

The typography and plates are exceedingly good.

METEOROLOGICAL AND PHYSICAL TABLES. By PROF. ARNOLD GUYOT, Ph. D., LL. D. (Price $3.00. For sale by D. Van Nostrand.)

The fourth edition of this useful work, revised and enlarged, has just been published by the Smithsonian Institution. The preceding, or third, edition was published in 1859, and though stereotyped, it was thought advisable to have this new edition entirely reconstructed. It now forms an octavo volume of 763 pages (including the introductory 25 pp).

Tables that have stood successfully the test of long use by a large number of scientific workers, are well established in public favor.

References to Guyot's Tables are familiar to all readers of physical science.

The tables are arranged in series, as follows:

First series (15 in number)—Thermometrical comparisons and conversions.

Second series (of 33 tables)—Hygrometrical computations.

Third series (of 27)—Barometrical tables.

Fourth series (of 26)—Hypsometrical tables.

Fifth series—Geographical tables, including 40 of measures of length (for heights, &c.), 10 of itinerary measures, and 10 of square measures of geographical surface.

Sixth series (of 99)—Tables for correction of variations of temperature, &c., at different parts of the earth.

Seventh, and last, series—9 miscellaneous tables.

Prof. Henry, in his report for 1851 on the first publication of these tables, says:

"These tables supply a desideratum in the English language, and will doubtless be highly prized by all engaged in physical research."

In his report for 1855, in referring to the preparation of a new edition of these tables, remarked:

"No publications of the Institution have been called for more frequently than these Tables. They have been introduced into Great Britain, and have supplied a want which has long been felt by the practical cultivator of physical science in that country as well as our own."

Architectural Perspective for Beginners. By F. A. Wright. New York: Wm. T. Comstock.

This is substantially Perspective without a master, although a beginner would need some preparatory instruction to enable him to solve successfully any one of the examples. The plates are good for students who are capable of beginning with the author's instructions, but then it would be an open question whether they should begin with Plate I. or Plate V. We should think the latter, unless they have acquired some previous knowledge of perspective.

Architectural draughtsmen will find valuable suggestions in this work, and as it is for such that the book was prepared we have no doubt that it will fulfill its mission.

A Treatise on the Adjustment of Observations, with Applications to Geodetic Work and other Measures of Precision. By T. W. Wright, B. A., C. E., late Assistant Engineer United States Lake Survey. New York: D. Van Nostrand. 1884. Price, $400.

This treatise will be found a valuable addition to the literature of geodetic operations; the title is, however, misleading—it implies a discussion of the various corrections required to allow for the effects of temperature, refraction, &c. Such corrections, however, are either omitted or only superficially dealt with, and the principal subject matter is the adjustment of unavoidable errors by the method of least squares.

The work commences by a discussion of the various causes of error, and several practical hints are given as to how to diminish them. A remark in connection with personal error is worth quoting: "A good observer, having taken all possible precautions with the adjustments of his instruments and knowing no reason for not doing good work, will feel a certain amount of indifference towards the results obtained. The man with a theory to substantiate is rarely a good observer, unless, indeed, he regards his theory as an enemy, and not as a thing to be fondled and petted."

In the second chapter the usual law of error is stated, and the method of least squares is deduced therefrom, together with formulæ for calculating the mean square error, the probable error, and the average error. The author points out that the name "probable error" is unfortunate, and so we think; he is also of opinion that the average error might with advantage be more used than it is at present as a measure of the precision of a set of observations. This chapter is concluded by a most instructive discussion on the laws of error, based on various assumptions as regards the number of sources of unavoidable error. It is first supposed that there is only one source of error, and that all errors between certain limits are equally probable; the curve of error then becomes a finite straight line. The next case considers two independent sources of error, the curve then becomes two straight lines intersecting on the axis of y at an angle of 45°. In the third case three sources of error are assumed, and the curve of error is shown to consist of three parts, which together form a close approximation to the usual curve of error. The method of least squares is further developed in the succeeding three chapters, and applied to the adjustment of the direct observations of one unknown, to indirect and to condition observations. Various methods of solving the numerous resulting equations are given, both rigorous and approximate; amongst the latter the method of solution by successive approximations as used in reducing the primary triangulation of the Ordnance Survey of Great Britain is strongly recommended. The author also recommends the use of a calculating machine, or of Crelle's Tables, in order to diminish the arithmetical labor.

The remainder of the work is devoted to applying the foregoing to triangulation, to base-line measurements, to spirit leveling, to trigonometrical leveling, to the graduation of line measures, to the calibration of thermometers, and to the discovery of empirical formulæ. The application to triangulation is treated very fully, and several methods of solving the necessary equations are given and illustrated by means of examples. One of these examples is the adjustment of the angles of a quadrilateral taken from the Survey of the Great Lakes of North America, executed by the United States engineers; three methods of solution are given, one of them being that adopted by the United States engineers.

The author remarks very truly that it is a waste of time applying the rigid methods of adjustment to tertiary or even to secondary triangulation, and he proposes a method of successive approximations by first adjusting the angles at each station for the local conditions, and then using these adjusted values for the further adjustment in connection with the side and angle equations of the net. It may be mentioned that the reduction of the secondary triangulation of Great Britain, now being carried out, is effected by a graphic method applied after the angles have been locally adjusted; this method is found to give excellent results with far less labor than even an approximate method of calculation. The criticism on the title of the work is well exemplified in the chapters on base-line measurements and on the graduation of line measurements. For instance, there is no mention of the corrections required to be made to a base-line measure-

ment to allow for errors in alignment or of level, for the effects of temperature and for reduction to sea level. We think that, at any rate, a sketch of these and other sources of error and their methods of adjustment would not have been amiss.

The adjustment of the errors of trigonometrical leveling is very fully considered, and one of the examples proposed for solution is the adjustment of the levels taken trigonometrically during the triangulation executed to determine the axis of the St. Gothard tunnel.

The following remark is, we think, worth quoting: "Closely allied to the preceding (elimination of accidental errors) is the common idea that if we have a poor set of observations good results can be derived from them according to the method of least squares, or that if work has been coarsely done such an adjustment will bring out results of a higher grade. A seeming accuracy is obtained in this way, but it is a very misleading one. The method of least squares is no philosopher's stone; it has no power to evolve reliable results from inferior work."

An excellent feature in the work is the illustration of the text by means of examples, embracing almost every possible case that occurs in practice. Some of these examples are fully worked out, others are proposed as exercises. Most of them are derived from geodetic work carried out in the United States. In conclusion we can strongly recommend this book.—*Nature.*

MISCELLANEOUS.

Descriptions of some waterproof varnishes for paper are given by the *Journal of the Society of Chemical Industry* from the *Papier Zeitung* as follows:—(1) One part Damar resin; four, five, to six parts acetone are digested in a closed flask for two weeks and the clear solution poured off. To this, four parts of collodion are added, and the whole allowed to clear by standing. (2) Thirty parts white shellac are digested with 500 parts of ether, and to the solution fifteen parts of lead carbonate are added, then shaken for some time and repeatedly filtered. (3) Five parts of glue are dissolved in 100 parts of warm water, and this solution spread on paper. After drying, the paper is soaked for an hour in 10 per cent. solution of acetate of alumina and again dried, in order to give it a final glaze. (4) 120 parts of linseed oil are heated and poured into a mixture of thirty-three parts of quicklime and twenty-two parts of water, to which fifty-five parts of melted caoutchouc have been added, stirring all the time. The varnish is strained and used hot. (5) One part of gutta-percha is carefully digested in forty parts of benzine on the water bath, and the paper covered with it. This varnish can be drawn or written on.

At the Montreal Meeting, Prof. Frankland communicated the results of a study of the phenomena attending the discharge of accumulator cells containing alternate plates of lead peroxide and spongy lead: (1) The energy of a charged storage-cell is delivered in two separate portions, one having an E.M.F. of 2 volts and upwards, the other an E.M.F. of 0.5 volt and under. One of these may be conventionally termed *useful*, and the other *useless*, electricity. (2) The proportion of useful electricity obtainable is greatest when the cell is discharged intermittently, and least when the discharge is continuous. (3) Neither in the intermittent nor continuous discharge at high E.M.F. is the current, through uniform resistance, augmented by rest. At low E.M.F., however, the current, after continuous discharge of the high E.M.F. portion, is greatly augmented, but only for a few minutes. This augmentation of current at low E.M.F. after rest is hardly perceptible when the high E.M.F. discharge has been taken intermittently. (4) The suddenness of fall in potential indicates two entirely distinct chemical changes, the one resulting in an E.M.F. of about 2.5 volts, the other in one of about 0.3 volt. (5) The chemical change producing low electromotive force is the first to occur in charging, and the last to take place in discharging the cell. It is the change which occurs during what is called the "formation" of a cell, and for economy's sake, a reversal of this change should never be allowed to take place. (6) Currents of enormous strength can be readily obtained from storage batteries coupled up in parallel, viz., a current of 55,000 amperes from only 100 cells. Such a current reduces to insignificance the output of the largest dynamo ever built. It is to be hoped that currents of this magnitude will open up new probabilities of research into the construction of matter. —*Engineer.*

Regenerative Accumulator.—M. Zenger's "regenerative accumulator" is formed by surrounding the electrode forming the positive pole, with a halogen, such as bromine, chlorine, or iodine. These halogens serve to depolarize the electrode in combining, when the circuit is closed, with the hydrogen upon the cathode. M. Zenger uses bromine because of its comparative cheapness and fluidity, rendering it preferable to gaseous chlorine and hydrate of chlorine, which are very unstable, or to iodine, which is costly and of a solid consistency. The bromine is placed at the bottom of a porous vase filled with fragments of retort carbon and closed by a covering of paraffin furnished with a cork hole to insert the liquid bromine. A layer of chloride of iron is placed over the carbon; and the zinc plate is surrounded with a solution of dilute chlorhydric acid (1 to 10) mixed with 5 per cent. of glycerine to reduce the resistance of the zinc-carbon bromine cell. A very constant battery for telegraphic work, where the line has a considerable resistance, is obtained from a concentrated solution of chloride of iron, more or less diluted. In this way a hydro-electric battery of 1.95 volts electromotive force and 0.5 to 5.2 ohms internal resistance is obtained, which gives 3.9 to 0.38 amperes according to its resistance. The constancy of the pile is considerable; and the cell can be regenerated in a short time by the current from a dynamo, which effects the reduction of the bromine.

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HYDRAULIC PROPULSION.

By SYDNEY WALKER BARNABY, Assoc. M. Inst. C. E.

Proceedings of the Institution of Civil Engineers.

I.

So much has been said and written on the subject of hydraulic propulsion, and the principles underlying it are so well understood, that the author proposes, notwithstanding the title of the paper, to give little more than a description of the latest boat propelled by this system, and a comparison of it with a sister-boat driven by a screw, and with some hydraulic vessels built previously.

The first mention of this system of propulsion which the author is able to find is in an old patent taken out in the year 1661 by Toogood and Hayes, for "a particuler way of Forceing Water through the Bottome or Sides of Shipps belowe the Surface or Toppe of the Water, which may bee of singuler Vse and ease in navigacon." Many patents were subsequently obtained, and Mr. Ruthven, whose patent is dated 1839, built two vessels, one vessel 9 feet long, which was tried in Edinburgh, and the other 40 feet long tried on the Forth in 1844. A vessel for commercial purposes, fitted with Ruthven's propeller was built in Prussia in 1853; and, besides others, a floating fire-engine was constructed on the Thames, in which, by the advice of the late I. K. Brunel, the pumping power was utilized for propelling the vessel, an adaptation of the turbine propeller for which it is specially suited.

Public attention was first prominently directed to the claims of this propeller by the trial of the "Nautilus" in 1866. This vessel, which was 115 feet long, and had engines of 127 indicated H. P., attained a speed of 8.32 knots per hour. She was propelled by a turbine 7 feet in diameter, which drew water from an opening in the bottom of the boat near the fore part, and discharged it through two nozzles in the sides, just, above the water. The area of each nozzle was 78.54 square inches.

In 1866 also the "Waterwitch," an armored gunboat, 162 feet long, 32 feet beam, and having 1,161 tons displacement, was built for the Admiralty at the Thames Ironworks, the machinery being designed by Ruthven, and constructed by Messrs. Dudgeon at Millwall. She was driven by two water-jets, discharged from nozzles at the sides level with the water, the mean diameter of each of which was 24 inches. The speed attained by the vessel was 9.3 knots per hour. This ship was sister to the "Viper," a twin screw-ship, but the latter suffered from the disadvantage of a double keel aft and a slightly fuller run. A detailed comparison between the two will be found in Table 1.

In 1878, a hydraulic torpedo vessel was built by the Swedish Government for

TABLE 1.

	Date.	Length.	Beam.	Maximum Draught.	Displacement.	I. H. P.	Speed per Hour	Midship Section.	$\frac{V^3 \times D^{\frac{2}{3}}}{\text{I H. P.}}$	Number of Propellers.	Revolutions per Minute.
		Ft. Ins.	Ft. Ins.	Ft. Ins.	Tons.		Knots.	Sq. ft.			
H. M. S. Viper..........	1867	162 0	32 0	11 10	1,180.00	696	9.58	337.0	141.4	2 screws.	110
H. M. S. Waterwitch.......	1867	162 0	32 0	11 2	1,161.00	760	9.30	336.0	116.9	1 turbine.	40
Swedish screw...........	1878	58 0	10 9	4 3	20.00	90	10.00	25.0	82.0	2 screws.	250
Swedish hydraulic........	1878	58 0	10 9	4 2	21.00	78	8.12	25.0	52.5	2 turbines.	384
Thornycroft screw.........	1883	63 0	7 6	3 8½	12.80	170	17.30	11.9	169.0	1 screw.	636
Thornycroft hydraulic.....	1883	66 4	7 6	2 6	14.40	167	12.60	13.4	72.0	1 turbine.	428

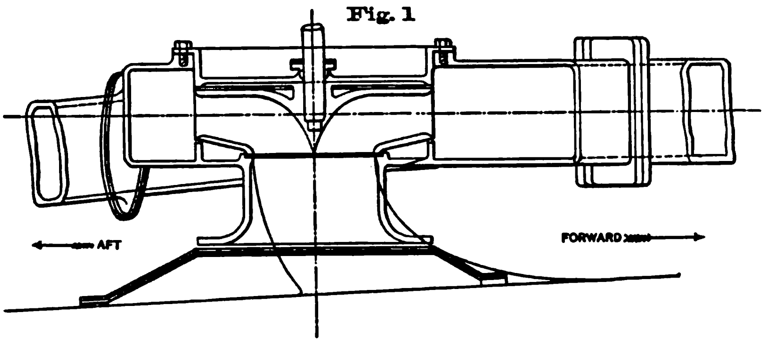
TABLE 2.

	Diameter of Turbine.	Area of Inlet.	Combined Area of Discharge.	$\frac{\text{Midship section}}{\text{Area of Discharge.}}$	Velocity of Discharge.	Water Discharged.	Efficiency of Pumps and Jet.	Total Efficiency.
	Feet Ins.	Square feet.	Square feet.		Feet per sec.	Lbs. per sec.		
H. M. S. Waterwitch.........	14 0	28.25	6.280	53 5	29.0	11,650	0.234	0.180
Swedish hydraulic...........	1 11½	1.62	0.864	30.5	28.0	1,510	0.277	0.214
Thornycroft hydraulic.......	2 6	1.52	0.951	14.1	37.2	2,210	0.330	0.254

competition with a similar vessel with twin screws; and by the kindness of her designer, Mr. Lilliehook, the author is able to give drawings of her arrangements. Figs. 1 and 2 show the position of the pumps, and of the inlets and outlets. In this vessel two pumps were used, not because the designer thought it an advantage, but in order that a passage might be maintained from the forward to the after part of the ship below the deck. The performance of this vessel will likewise be found in Table 1.

the cylinder, works up and down in it. The cylinder being now full of water, and the float consequently at the top, steam is admitted by a valve above the float and driving it down, ejects the water through the nozzle. On reaching the bottom of its stroke, the float opens the exhaust, and the steam passes into the condenser. The vacuum thus created in the cylinder causes the water to rise partly through the nozzle, but principally through a suction-valve in the bottom of the condenser. The cylinder is thus filled with water,

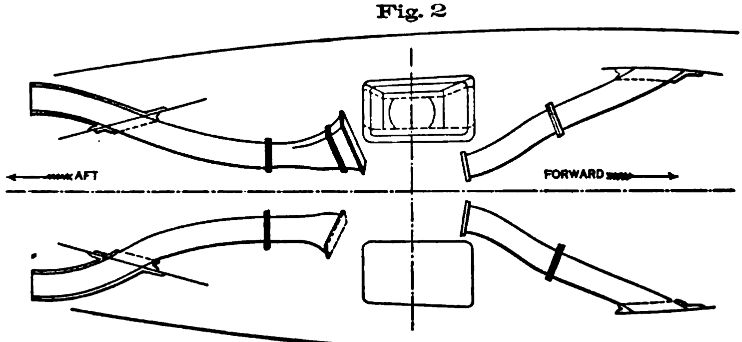
Fig. 1



7.87 knots. 8.12 knots.

LONGITUDINAL SECTION THROUGH INLET OF SWEDISH HYDRAULIC VESSEL.

Fig. 2



PLAN OF DISCHARGE-PIPES OF SWEDISH HYDRAULIC VESSEL.

In 1879, the "Fleischer Hydrometer," a ship of a different design, but still propelled by the reaction of jets of water, was built in Germany. In this vessel the water is acted upon directly by steam without the intervention of a pump. The arrangement is as follows:—There is a cylinder lined inside with wood, at the bottom of which is a large pipe leading to a nozzle at the bottom of the vessel. A float, of nearly the same diameter as

and the float rises to the top, in doing which it closes the exhaust and opens the steam-valve, when the operation is repeated. The loss by condensation appears, from the indicator cards, to have been much less than might have been expected in a cylinder filled alternately with steam and with water; but as the cylinder is not entirely emptied at each stroke, a layer of boiling water always remains at the top and adheres to the wooden lining

as the float descends. The vibration was enormous, and the jerky motion was very unpleasant at the earlier trials, but was somewhat lessened afterwards. The data obtainable are unreliable, as the speed is variously given as 9 and as 6 knots per hour, and there is no means of ascertaining the loss between the boiler and the indicator, which is probably large. As the down stroke is made by the pressure of the steam and the upstroke by the pressure of the atmosphere, it is probable that the upstroke occupies the longer time. Water is ejected and propelling takes place during the down stroke; therefore, in order to eject a given volume of water per second at a given mean velocity of discharge, it is necessary either to have two or more pumps where one would be sufficient if the action were continuous, or the actual velocity of discharge must be at least double, and probably, more than double the mean velocity. Either plan produces loss of efficiency.

The advantages claimed by the advocates of the hydraulic system of propulsion may be enumerated as follow:—No impediment to speed under sail; no racing due to pitching; no vibration; power of reversing-motion in the hands of the officer on deck; full engine-power for manœuvring; vessel capable of being made double-ended, and power of ramming much increased. The propeller is not liable to damage from running aground, and cannot be fouled by floating obstructions; it is favorable for light draught; and the large pumping power is available for keeping down leaks. The disadvantages of the hydraulic propeller are mainly these: First and foremost is the difficulty of utilizing the full energy of the feed-water, or the velocity of the water entering the propeller. Secondly, every particle of water acted on must be carried in the ship. Thirdly, the loss by friction of the water in the passages. Fourthly, the loss by bends in the pipe, which can only be reduced by increasing the losses from the second and third causes, namely, the weight of water carried, and the friction of the passages.

The advocates of the system of hydraulic propulsion were not satisfied with the Waterwitch as an embodiment of their ideas. They were supported in their demands for a further trial by Lord Dufferin's Committee on Designs in 1871. This Committee, which numbered amongst its members, Sir William Thomson, Professor Rankine, and Mr. Froude, inserted the following clause in their report:

"Our attention has been directed to the hydraulic method of propelling steam ships, with reference especially to vessels of very light draught, and intended for service in waters which are so shallow as scarcely to afford sufficient immersion even for twin screws, or in which there is reason to apprehend that the screws are likely to be fouled by obstacles placed there for that purpose. Regarded in this light, we are of opinion that the system is deserving of a more thorough trial than it has yet received, and we beg leave to recommend the subject to their Lordships' consideration accordingly." The Admiralty hesitated long before doing so, but were finally induced in 1881 to order one of a number of second-class torpedo boats, then being built by Messrs. Thornycroft, to be fitted with a turbine propeller. It was intended that the boat should be as much as possible like one of the screw-boats, in order that the performance of the two might be compared. It was, however, soon found desirable to make certain modifications in the form of the hydraulic-boat, the reasons for which will be explained. The conditions laid down by the Admiralty were that a similar boiler was to be used to that in the screw-boat, and about the same H.P. developed, and that the builders were to obtain as much speed as they could.

The number of revolutions made by the engines of the screw-boats are about six hundred and thirty per minute. This was considered much too high a velocity for the turbine, and its revolutions were fixed at four hundred per minute. This involved larger cylinders. Those of the screw-engines were 8¼ inches and 13½ inches in diameter and 8 inches length of stroke; while those of the hydraulic engines were increased to 8¼ inches and 14½ inches in diameter and 12 inches length of stroke; and, although the latter were relieved of the weight of the reversing gear, they were still considerably the heavier, the former being 1.9 ton, and the latter, including pumps and water in pipes 2.58 tons. The total weight of machinery, including water in

the boiler, &c., is, for the screw, 4.87 tons; for the turbine 5.56 tons.

It was felt that a vessel of the same dimensions as the screw-boat would be much handicapped by this extra weight, as the whole displacement of the latter is only 12.9 tons. The length was therefore increased from 63 feet to 66 feet 4 inches, and this, together with a modification of the bottom, which gave some extra displacement, restored the boat to about the same draught amidships as the screw type.

The difference observable in the disposition of the machinery was dictated by the necessities of trim. This explains why the boat, intended for comparison with a similar vessel driven by a screw, was not made of the same dimensions. The designers consider that the alterations render the conditions much more fair for the hydraulic boat than if no modification had been made. The difference in length tells slightly in favor of the longer boat.

It will be convenient here to glance at the causes of loss of work in propellers of different kinds, and the methods adopted for reducing them to a minimum in the boat under consideration. These causes of loss, irrespective of friction, may be thus summed up:

1st. Suddenness of change from velocity of feed to velocity of discharge.

2d. Transverse motion impressed on the water.

3d. Waste of energy of the feed water.

Propellers which suffer from the first cause of loss are, the ordinary uniform-pitch screw and the ordinary paddle-wheel; while those which, in varying degrees, avoid it are the gaining-pitch screw, certain forms of feathering paddles, Ruthven's form of centrifugal pump, and, probably best of all, the oar. Propellers which undergo loss from the second cause, namely, transverse motion imparted to the water, are ordinary screw propellers, radial paddle wheels and oars. This loss is greatly reduced in the guide blade, screw propeller, and is entirely avoided in the turbine propeller. The third cause of loss, that is, the waste of energy of the feed-water, is experienced only by the jet propeller, as it has been previously used, and it is from this cause principally that its inefficiency results.

In the boat under consideration, however, careful provision has been made to utilize as much as possible the velocity of the feed-water; and it is in this respect chiefly that she differs from the Waterwitch and from the Swedish boat, where nearly the whole of that velocity was lost.

In the Thornycroft hydraulic boat there is a sudden break in the bottom, just forward of the pump. The bottom at that point has been formed into a great scoop (Fig. 3), rising by a gentle incline to the inlet of the pump, which is placed at an angle, so as to reduce the change of direction imparted to the entering water as much as possible. The velocity of the water causes it to rise in the scoop, and the vanes of the pump are adjusted to pick up the water without shock, and gradually to accelerate it to the speed of discharge. Having now put the full energy into the water in the form of velocity, it is simpler to keep it as such, and to get rid of the water overboard as quickly as possible, instead of converting it, by means of a vortex chamber, into pressure, in order to reduce the friction through the pipes, as is generally done when the water is carried any distance. In the Swedish boat (Fig. 2) the outlets are carried to some distance ahead and astern. This was done, the author believes, to reduce the loss by sudden bends, and to increase the leverage for steering, as valves were put at the ends of the passage which were actuated by the steering wheel, and which diverted the water into an athwart-ship direction. This involved a large increase in the weight of water carried and in the friction.

The nozzles in the Thornycroft boat are 9 inches in diameter, and are formed of pieces of copper pipe bent to a radius of 18 inches. They are shown in Fig. 4. Each of these bent pipes is pivoted in such a manner at the point P that either end can be presented to the hole in the side of the boat. No valve is therefore required. The movement is effected by handles in the conning-tower.

There seems to be a considerable difference of opinion as to the best position for the inlet to the pump. Some say that it is immaterial where the water is taken in, while one distinguished engineer thinks that the inlet should be at the

stern. His idea, which at first sight seems reasonable, is, that by taking in at the after end, water which is at rest, as regards still water, then by a gradually narrowing channel and a gradually imparted acceleration, giving it a velocity *v*, equal to the speed of the ship relative to still water, and then turning it round and discharging it at the stern with a velocity *v* relative to the ship, and no velocity relative to still water, there is no slip. The efficiency then comes out to that extremely deceptive figure unity, and he states in a paper, "On the Efficiency of Jet Propellers," which appears in the Transactions of the Institution of Naval Architects, that this is a theoretically perfect propeller. This will be examined more particularly in the appendix, but looked at in a simple manner, suggested by Mr. Thornycroft, the result seems clear. As the propelling apparatus takes up water at rest at the stern and discharges it at the same place and in the same condition, and as the quantity discharged is equal to the quantity received, it seems evident that the inlet and the outlet might be connected together by a pipe, and the same water be passed round and round. The result is plainly no propelling power.

Fig. 3

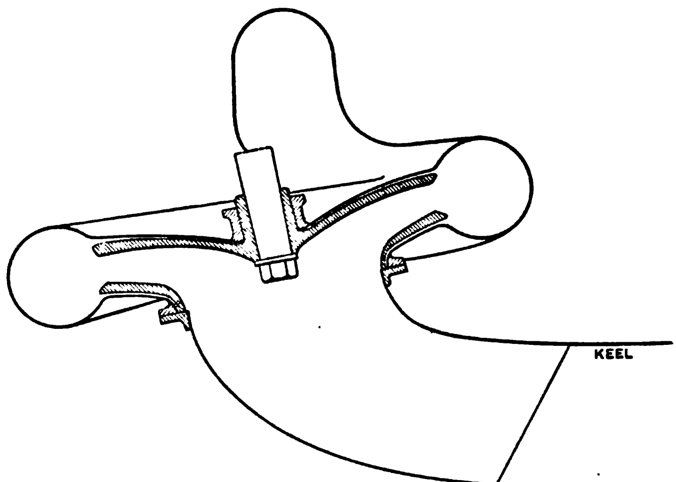
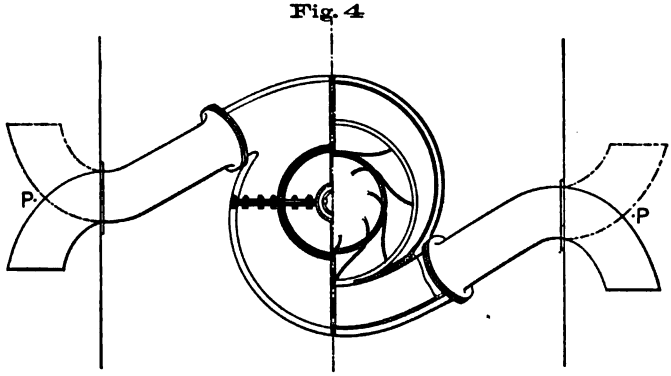


Fig. 4



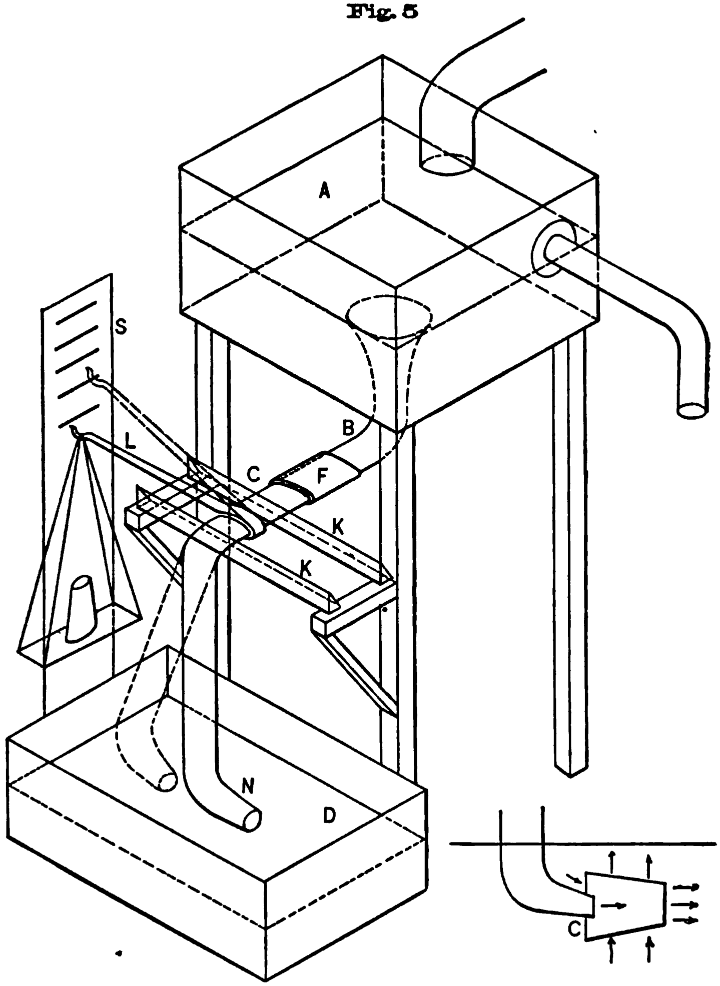
THORNYCROFT DISCHARGE.

Another point to be considered was whether the outlets should be above or below water. The amount of water discharged, and the resultant reaction, are absolutely the same from a given orifice

and a given head, whether that orifice be above or below water. But work is lost in raising the water above sea level. Another reason for keeping the orifices below the surface was that it was hoped some advantage might be taken of induced currents set up by the discharged water. Experiments were made upon this point independently by Mr. Thornycroft and by the designer of the Swedish hydraulic boat.

In Fig. 5, A is a tank containing water which flows through the pipe B C, and is discharged by the nozzle N below the surface of the water in the tank D. The pipe B C is supported upon knife edges K K. The portion B of this pipe is connected with the part C by a flexible india-rubber pipe F, which permits C to swing back to balance the reaction of the water issuing from the nozzle N. A lever L is attached to the movable pipe C, upon the end of which weights can be hung. The nozzle N being stopped by a cork and the tank A and pipe B C filled with water, the position of the end of the lever L is noted upon the scale S. The nozzle is then uncorked and the water level kept constant in the tanks A and D. The pipe C swings into the dotted position. Weights are then placed on the lever L until it is brought into the old position,

Fig. 5



EXPERIMENTAL APPARATUS.

and the moment of the weight balances the reaction of the jet.

If now a hollow truncated cone be placed over the nozzle and the water discharged through it, outside water is drawn through the annular opening *a*, and mixing with the jet has velocity imparted to it. There is a loss of pressure along the inside of the cone, and a corresponding increase along the outside which impels it forward, on account of its conical form, and increases the reaction of the apparatus. By placing two or three of these conical tubes one outside the other, as shown in Fig. 6, and thus reducing the speed of the discharged water, and increasing the volume, the reaction of the several cones considerably augments that of the original jet.

In one experiment five cones were used, and a jet driven by a small propeller was passed through them. They were so arranged that they could be slipped over the jet while the apparatus was working. A weight was lifted by the action of the jet, and the pull of the propeller alone was measured first. The cones were then slipped over it, when the pull was immediately increased in the proportion of 1 to 1½. The experiment gave such unmistakable results that much was hoped from it. When, however, the apparatus was drawn through the water, the friction against the larger surfaces counterbalanced any advantage in reaction, and it had to be abandoned.

Experiments with nozzles towed through the water and discharging below the surface, and then discharging above it, showed very clearly that the resistance of the nozzle far outweighed the loss by raising the water the extra height necessary to keep them above the surface.

Fig. 7 represents the curves of areas of the immersed portions of the hulls of the screw and turbine boats. The length of the former is represented by the base line B C, that of the latter by A B. Ordinates at any point measure the area of the section at that point. The area of the curves thus obtained gives the volume of the displaced water. B is the bow, and A and C are the sterns. It will be seen that there is a sudden increase in the sectional area amidships in the hydraulic boat. The height of the ordinate F D represents the area of the section just forward of the inlet to the pump. D E is the area of the inlet. Instead of the area abaft the inlet falling back again, as in all previous hydraulic boats, it is

Fig. 6

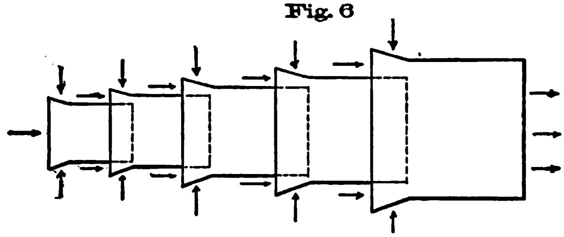
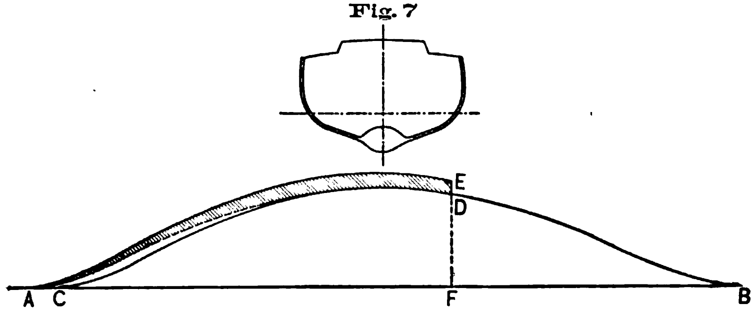


Fig. 7



Curves of Areas and Sections of Inlet.

kept at the full amount FD+DE, and continues to the stern at the higher level. An amount of extra displacement, represented by the shaded portion, is thus obtained without the water being conscious of any discontinuity in the form, as the amount which has to be displaced to make room for the increased section is removed by the pump. When the section of the hull is kept the same before and abaft the inlet, fresh water has to flow in from the sides to supply the place of that passing into the pump, and probably disturbs the smooth action of the stream-lines.

It has not been found practicable to utilize the full pumping power of the engines for keeping down leaks. A centrifugal pump will not work unless fully charged; and the supply of water necessary in this case is so enormous in proportion to the size of the boat that large and complicated valves become needful for closing the inlet and throwing the pump into communication with the bilge, and then to supplement the bilge-water with water from outside as the former becomes insufficient to feed the pump. The difficulty is seen directly when it is stated that the pump delivers an amount of water equal to the whole displacement of the boat in fifteen seconds. Thus, although not impossible, it would have involved the addition of such a large weight, and the consequent loss of such an amount of efficiency as regards speed, that it was reluctantly abandoned, on the ground that nothing should be allowed to interfere with the highest possible speed being attained.

Although the double-ended form of hull, and the consequent advantage in power of ramming, would no doubt be adopted in an iron-clad, it is unnecessary in a torpedo boat, and some additional speed was anticipated from the retention of the ordinary form.

It has always been considered a feature in the hydraulic system that an equal speed is attainable ahead and astern. It is obvious, however, that the form of inlet adopted is unfavorable for going astern. As a general rule, anything designed for locomotion in more than one direction may always be adapted to give a better result when moving in one particular direction than in any other; and when there is an equal power of moving either forwards or backwards, it is possible to effect an improvement in one direction at the expense of the other. Vessels intended to navigate crowded rivers require some power of quickly reversing their motion; but for torpedo boats, which have to run rapidly up to an enemy and then escape from him, the case is different. The qualities required in torpedo boats are high speed and quick steering power. Several of the more recent inventions in screw propellers have had for their object the utilizing of the whole power of the engines for steering; and it is not improbable that, in vessels intended for this service, the complications necessary for reversing the engines may be done away with.

The performance of the boat is shown in Table 2. The quantity of water discharged was about 1 ton per second; the velocity of discharge 37.25 feet per second, and the speed of the boat 21.4 feet per second, or 12.65 knots per hour. The efficiency of the pump and jet combined, *i. e.*, useful work in jet divided by effective HP., was 0.33, and the total efficiency, *i. e.*, useful work in jet divided by indicated HP., 0.25.

The method adopted for measuring the volume of water pumped, and the velocity of discharge, was considerably more accurate than any hitherto employed. In the case of the Waterwitch, the only measurements taken were with a patent log hung in the jet, and the speed of discharge was not exactly known. The author has taken the efficiency as calculated by Mr. Brin, which gives the quantity and speed of discharged water shown in Table 2. This velocity, 29 feet per second, approaches very closely to the peripheral velocity of the wheel, 29½ feet per second, and it is probable that the result is somewhat over-estimated.

In the Swedish boat a pressure-gauge was placed in the nozzle, and the calculations were made from the pressures found there.

In the new boat, a thin plate, $1\frac{5}{16}$ inch square, was attached to the end of a lever, and placed in the jet just where it left the nozzle. The pressure on this plate was recorded by a dynamometer attached to the other end of the lever. The apparatus was so arranged that the pressure could be measured at every point of the jet, and not in the center of the

stream only. The mean pressure in the jet was found to be nine-tenths of the pressure in the center. From this the velocity was estimated, and from the velocity, the quantity discharged. Fig. 8, curve A, shows the pressure in lbs. upon the $1\frac{5}{16}$-inch plate and lever carrying it, in the center of the jet, at different revolutions of the pump. Curve B shows the pressure upon the lever carrying the plate, plus a constant weight, and forms the zero. This curve was obtained by finding the pressure upon a similar lever without the plate. The difference between A and B is the actual pressure on the plate. A curve of the efficiency of the pump and jet at different revolutions is also shown.

Curves D and E correspond to A and B, but were measured when the boat was moored. As a test of the accuracy of the method of measurement, a dynamometer was attached to the stern of the boat, and the actual pull found at a given number of revolutions. At two hundred and ninety-five revolutions, the reaction estimated from the curve was $9\frac{1}{4}$ cwt. The pull recorded by the dynamometer

Fig. 8

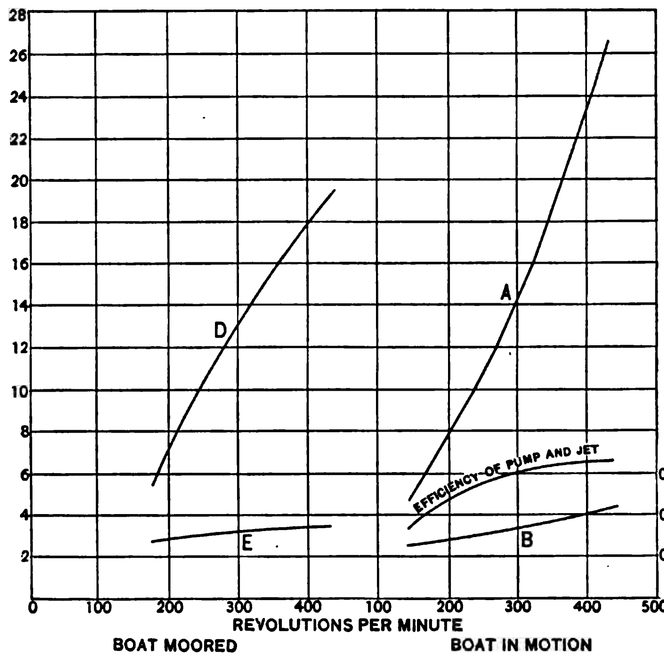
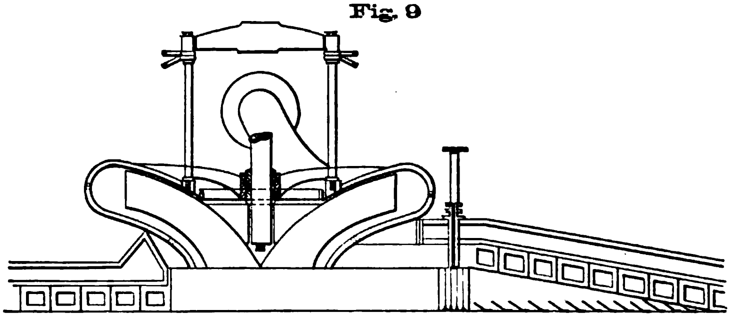


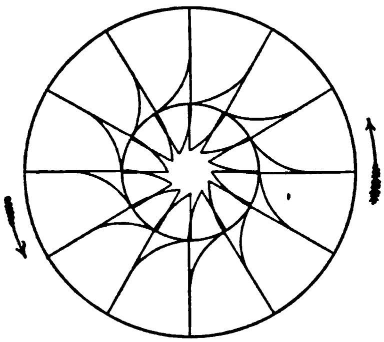
Fig. 9



INLET OF WATERWITCH.

was $9\frac{3}{8}$ cwt. This was considered very satisfactory, and shows that the efficiency is, at any rate, not over-estimated.

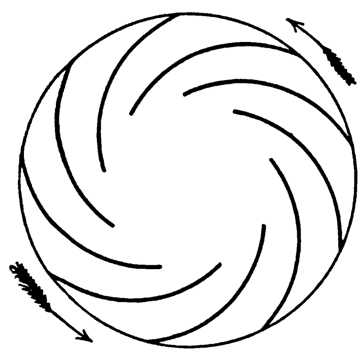
Fig. 10



Peripheral velocity 29.6 feet per second.

WATERWITCH PUMP.

Fig. 11



Peripheral velocity 39.36 feet per second.

SWEDISH PUMP.

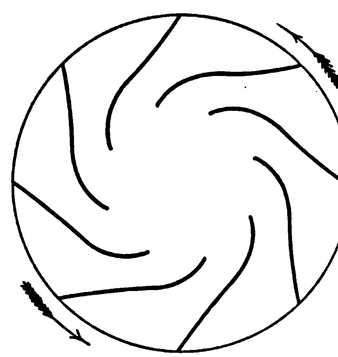
It is possible from the two curves A and D to estimate the effect of the form of inlet upon the efficiency of the jet.

The indicated HP. for a given number of revolutions was found to be the same in both cases. At four hundred and twenty-eight revolutions, the mean pressure when moving was considerably more than the pressure at the same number of revolutions and indicated HP. when moored. This excess of pressure was due to the energy of the feed-water, and corresponded to a velocity of 20.6 feet per second. This shows that only 0.8 foot per second of the full velocity of the boat was lost.

The efficiency of the jet thus appears to be 0.71, and of the pump, 0.46. The calculations are given in Appendix I. In the case of the Waterwitch, where the whole of the velocity of the feed was lost, the efficiency of the jet was 0.5, and of the pump, 0.47. In the Swedish boat, under similar conditions, the jet gives 50 per cent., and the pump 55 per cent. In this boat an alteration was afterwards made in the inlet, as shown in black lines in Fig. 1. and the speed was thereby raised from 7.87 knots to 8.12 knots.

The measurements were all taken, and the efficiency calculated under the original conditions when the speed was 7.87 knots. The actual HP. expended in driving the vessel at this speed was $78 \times 0.214 = 16.7$. Suppose this to be increased as the cube of the speed, the actual HP. for 8.12 knots would then be 18.4. This raises the total efficiency to $(18.4 \div 78) = 0.236$.

Fig. 12



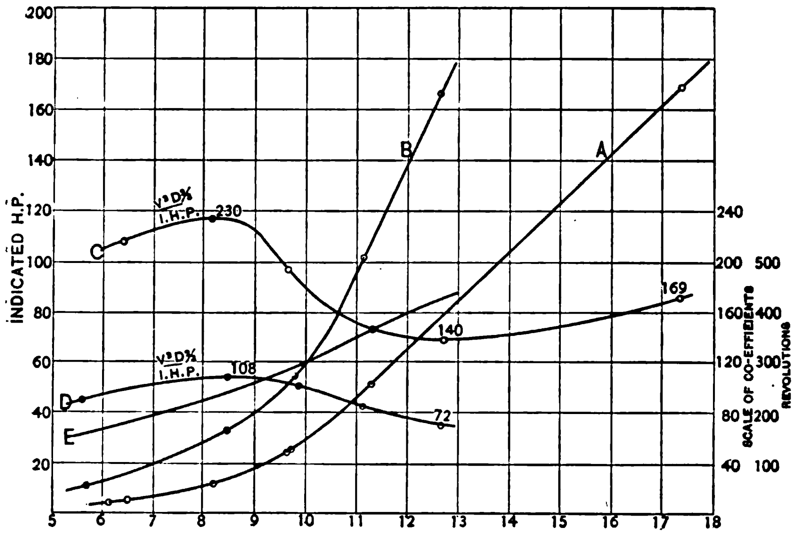
Peripheral velocity 56.0 feet per second.

THORNYCROFT PUMP.

It can be deduced from this that the speed of discharge was raised from 28 to 29 feet per second, corresponding to an increase of head of nearly 1 foot. The velocity of the stream entering the turbine would appear to be only 7.48 feet per second, or a little more than one-half of the speed of the boat. The efficiency of the jet is increased from 0.5 to 0.567. Figs. 10, 11 and 12 show the three forms of centrifugal pump, all reduced to the same diameter.

It should be remarked that the comparison between the performances of the Waterwitch and of the two smaller boats, on the basis of the displacement

Fig. 13



Speed in knots.

E. Curve of revolutions, and speed of Hydraulic boat.

Curves of Horse-power and Speed.

formula given in Table 1, is not a fair one. The coefficient is taken at the maximum speed in each case, and the Waterwitch gives a very good coefficient. But 9.3 knots per hour is an easy feed for a vessel of her length, while 12.6 knots is a very difficult one for a boat only 66 feet long.

It is alike unfair to compare the coefficients of the two Thornycroft boats at 12.6 and at 17.3 knots.

Fig. 13 shows the HP. required for different speeds, and the value of the coefficient $\frac{V^3 D^{\frac{2}{3}}}{\text{I.HP.}}$. A is the curve of power and speed of the screw-boat; B, of the hydraulic-boat; C and D show the respective coefficients at different speeds. It will be seen that at 12½ knots the coefficient is at its worst, and that afterwards it steadily improves. The comparison between these two should be taken at 12.6 knots for both boats. The hydraulic is then 72 and the screw 140. This ratio is almost the same as the ratio of the efficiency, 0.254 to 0.5. The maximum performance of the Thornycroft was 108; that of the Waterwitch, 116. It will be asked, how is it that, although the efficiency of the new hydraulic machine is higher than that of the Waterwitch, yet the latter gives a better coefficient even at corresponding speeds? Looking at the curve of coefficients Fig. 13, it will be seen that the maximum occurs at 8½ knots. This corresponds to two hundred and thirty-five revolutions of the pump. In Fig. 8 it is shown that the efficiency of the pump and jet is only 0.26 at two hundred and thirty-five revolutions. If the pump had been designed to run at its maximum efficiency at the speed which best suited the boat, a much better coefficient would have been obtained. The indicated HP. at 8½ knots was 33. This multiplied by the maximum efficiency of pump and jet, 0.33, and divided by the efficiency at 8½ knots, 0.26, gives 42 indicated HP.; this would admit a speed of 9.2 knots. This speed of 9.2 knots, which would have been obtained with 33 indicated HP. if the efficiency of the hydraulic machine had been 0.33 instead of 0.26, gives a coefficient of 140 against 116 in the Waterwitch.

In conclusion, it is worthy of note that one of the greatest obstacles to the success of the jet-propeller, namely, the loss of energy of the feed-water has been overcome. It was clearly foreseen by

Mr. Thornycroft, and by adapting the bottom of his boat to meet it in the manner described, he has raised the efficiency of the jet from 0.50 to 0.71.

Unfortunately this obstacle did not stand alone. What efficiency it is possible to get with a centrifugal pump delivering a ton of water per second, with a lift of 21½ feet and of limited weight and dimensions, the author cannot say. Forty-six per cent. seems very low. Had it reached 70 per cent. the total efficiency would have been 0.385, and the speed upwards of 15 knots. Perhaps this amount of success may yet be achieved for the hydraulic propeller, but it is not likely to be exceeded.

The case at present stands somewhat thus:

In the screw-boat the efficiencies are—Engine, 0.77; screw propeller, 0.65; total, 0.5.

In the hydraulic-boat—Engine, 0.77; jet propeller, 0.71; pump, 0.46; total, 0.254.

The jet as a propeller may be taken as a little better than a screw, but the loss in the pump is a dead loss, and represents about half of the power. In other words, before a hydraulic-propelled boat can be made to compare favorably with one driven by a screw, the pump producing the jet must work without loss.

NOTES ON THE INSPECTION OF METALLIC STRUCTURES.

By JAMES SANDERSON, C. E.

Papers of the Engineering Society of the University of Michigan.

Midway between the engineer and the mechanic, in the chain of those whose industry builds up the great structures of the day, stands a class of persons who are supposed to be possessed, to a certain extent, of the qualifications of both the other classes. These are called inspectors, and their duty is to follow closely the labor of the workman who embodies in material form the ideas of the engineer, to see that the work is well done, and that the results are in accordance with the guiding plans and specifications.

It is quite possible that certain parts of the inspector's experience may be of value to the engineer; for the former comes more closely in contact with actual materials and workmanship than does the latter, and may be expected to observe some things which his employer has no opportunity of seeing. Therefore I make no apology for bringing to your notice a few items from such an experience in connection with the building of iron and steel structures, hoping that, although meager and loosely thrown together, and hardly in the line of engineering studies, they may yet be of some interest, and, possibly, of some value.

One of the things with which the inspector is supposed to deal is the testing of pieces of metal, and of entire members of different kinds. In this, however, my own experience, though perhaps extensive enough in amount, is limited in kind, being confined to the testing of tension pieces, and particularly of eyebars.

It is customary in some shops, and required in the specifications of many structures, to test each eyebar used in a bridge or a roof, to an amount exceeding by one-third to one-half, the working load of the member. In steel bars the elongation produced by this test is often measured, as an indication of the uniformity, or otherwise, of the material, with respect to its modulus of elasticity. For measuring this elongation, various apparatus may be used. I shall describe briefly the one I employed:

It consisted mainly of two flat wooden rods, each of them half as long as the part of the eyebar to be measured. The rods lay along the upper edge of the bar, but were prevented from touching it by small, smooth pieces of hard wood, about an eighth-inch in thickness, tacked to the under side, one on each end of each rod. The rods were clamped down to the bar at their outer ends, by ordinary joiners' clamps, placed directly over the small

wooden blocks. Thus the movement of the rods, as they slipped to and fro on the blocks at the inner ends, indicated the extension and the contraction of the bar between the two points at which the rods were clamped. To assist in measuring this movement, a small steel plate was secured to the top of each rod, at its inner ends, and the distance between the inner edges of these plates, before, during, and after the test, was taken by means of verniered calipers, which read to 1-1000 of an inch.

As to the results of such tests, there is nothing in them worthy of special remark here. The elongations obtained would, of course have been different in steel of a higher or lower grade, and would, therefore, be no standard for other cases.

When it was desired to make tests to rupture, in which the space under measurement was usually less than in routine tests of eyebars (the bars themselves being, as a rule, smaller) a somewhat different apparatus, was used. There was but one bar, instead of two, and this was clamped at one end and free at the other. To the free end was fastened the measuring apparatus, consisting, essentially, of a small steel roller, bearing a long, light, radial arm of steel. The roller, which rested on the top edge of the test-piece, was roughened, so as to roll upon it without slipping, and as it revolved, the end of the steel arm moved around a brass arc, upon which were marked, in a greatly exaggerated scale, the distances passed over by the periphery of the roller.

In making a test to rupture, the particulars noted were usually the following, viz.: Original size and length of bar; thickness of head and size of pin-holes (if an eye-bar); distance between witness-marks before test; stretch (in the portion of the bar under the measuring rod) at tensions of 10,000, 15,000 and 20,000 lbs. per square inch, respectively, the bar being released from tension after each reading; stretch at 21,000, 22,000 lbs, per square inch, the bar being relaxed after each pull as before, until it showed permanent set; amount of permanent set, and the tension at which it occurs; total pull at the instant of rupture; size of bar, after test, at point of rupture; distance between witness-marks after test. From the last two items are deduced the reduction of section and the elongation, which indicate the comparative ductility or brittleness of the material tested. The tension at which the first *perceptible* permanent set occurs is taken as the elastic limit (proof strength) of the piece. I am aware that Rankine objects to this method of measuring the proof strength, as unscientific; but it gives results which are satisfactory for practical purposes, and for shop-work, where dispatch is needed, rather than scientific nicety, it is far preferable to the cumbrous process described by Rankine himself. (See *Civil Engineering* § 144.)

Much is sometimes made of the appearance of a fracture on a piece of iron, as indicating its quality; and a practiced eye may, indeed, discriminate between coarse and fine metal by examining the broken ends of a bar. But I am of the opinion that little dependence is to be placed upon the supposed difference between iron which shows a "fibrous," and that which shows a "crystalline" fracture. I have frequently seen the same break show patches of each kind of fracture, and that, too, where I was practically sure that the texture was uniform. The difference seemed to lie not so much in the quality of the iron, as in the manner in which it had been broken. Where, for example, the fracture was in the nature of a cross-break, it had a crystalline appearance, but where it ran along the grain, like a tear, it looked fibrous. In other words, the so-called "crystals" seemed to be merely the squarely broken ends of fibers, which, when drawn out into points, would have brought a "fibrous" fracture. And, in general, the causes producing one sort of break or the other, seemed rather external than structural. For instance, when a piece was broken suddenly, as by a blow, or was nicked, and then bent cold, the resulting fracture was almost invariably crystalline; but when it was pulled steadily until rupture took place, or was bent without nicking, the break generally looked fibrous. In this respect, iron seems to behave much like pitch, and other ductile substances, which, when drawn out slowly, will run to a point, or break like a bunch of fibers, but, if struck or jerked suddenly, will crack with a smooth conchoidal fracture corresponding to one of the crystalline surfaces or facets of the broken metal.

Aside from the matter of tests, there are many points requiring the inspector's attention, but few, perhaps, that can be verbally explained. As with the "knacks" and turns of any trade, an acquaintance with them presupposes personal contact with materials and work. There is, however, one subject of considerable importance, though seemingly a small one, about which a few things may, I think, profitably be said. It is the subject of looseness in rivets.

The rivet, more than any or all other forms of fastening, is used in connecting the parts of iron or steel structures; and it is doubtful if anything will ever be devised to take its place. Nevertheless, as at present used, it is subject to certain disadvantages and accidents of workmanship, which it is the business of the inspector to detect, and of the engineer to remedy or to prevent.

Perhaps the most important of these drawbacks is the liability of rivets to looseness. The object of almost all the rivets in an engineering structure, is to carry transverse shear. To be convinced of this fact, if conviction be needed, one has but to consider the relations which the parts of a built member bear to one another, and the fact that the rivets joining them are driven at right angles to the principal lines of stress.

Now, if a rivet be loose, *i.e.*, if its shank be separated, by any appreciable space whatever, from the sides of the hole which it is supposed to fill, it is evident that it can take up, from the pieces which it connects, none of the shear intended for it, and that its hole might as well be empty. To detect this looseness in rivets, so as to have the matter corrected, is part of the inspector's duty. But as in other cases, so in this, prevention is far better than cure; and if means can be found to insure the tightness of rivets in the first place, the results will be far superior to those produced by any amount of cutting out and replacing. If, therefore, the experience of any number of inspectors or observers shall aid in discovering the causes or conditions of looseness, so that these may be removed, much will have been effected.

It is impossible, however, to lay down any infallible rules as to the parts of iron or steel structures where most loose rivets are to be found. I do not know of many other cases where it may be more truly said that "exceptions are the rule;" and therefore, while I think that the few general statements I shall make below will prove true in the long run, they may very possibly be false guides in any given case, and are only to be taken as expressions of the average of results.

1st. Hand-driven rivets are more apt to be loose than those driven by power, and are more irregularly loose. That is to say, supposing an equal number of rivets to be loose in each of two members, one power, and the other hand-riveted, the loose rivets in the former are likely (if numerous) to occur in groups and series; while, in the latter they will probably be scattered, without any regularity, over the member.

2d. Counter-sunk rivets are more apt to be loose than those with ordinary round heads. Whether rivets counter-sunk on both sides are more frequently loose than those with one full head, I am unable to say, as it is generally impossible to test double-counter-sunk rivets for lateral looseness. However, such rivets will frequently move longitudinally in the hole, which is not true of the most full-headed rivets.

3d. Rivets connecting thick plates, or pieces of metal, are more likely to be loose than those in lighter work.

4th. Rivets holding lattice-bars or light cover-plates to flanges of channels, &c., are very seldom loose. If, however, a rivet passes through two or more thicknesses of metal besides lattice-bar or bars, or a cover-plate, its liability to looseness is increased in proportion to the extra thickness of metal passed through.

5th. Pin-plates, and other reinforcing plates, especially when fastened by staggered rivets close together, are likely to have some loose ones. In general the closer the rivets together, the more apt are some of them to be loose. In long, regularly spaced rows, the rivets are less apt to be loose than when crowded or bunched.

6th. In rows of rivets, those at or near the ends are most apt to be loose. When the end rivet of the row is tight, a loose one may frequently be found second or third from the end.

7th. Other circumstances increasing the liability to looseness, will suggest themselves. For example, when rivets

have to be cut out, and others driven to replace them, the shock of the cutting and redriving is very apt to loosen rivets, previously tight, in their neighborhood. This makes a loose rivet a spreading evil, and one which is peculiarly difficult to remedy. Again, it will readily be seen that, in hand-riveting, any rivet, either of whose heads is hard to get at for the purpose of holding, or heading up, is likely to prove loose. Also, that it is difficult to make a tight rivet at or very near the angle of a bent piece, or at any point where it is not easy to bring the parts together. This, too, is evident, that a rivet, in order to be tight, should fit the hole as closely as possible before driving, and should be long enough to admit of being well headed up when in the hole. A small, half-formed head is a frequent accompaniment of a loose shank.

8th. I have not found that rivets holding together a large number of moderately thick plates, are especially apt to seem loose under the hammer. I doubt, however, if such rivets actually fill the holes. I suspect that a section of such a rivet would show it to be bent in the hole, and so held apparently tight, while not actually so.

9th. It stands to reason that a fairly punched and reamed hole is more apt to contain a tight rivet than is an irregular one; but I do not think, from my experience so far, that drilled holes (like those generally used in steel-work) will prevent loose rivets, or even greatly diminish their number. This, of course, is not the primary object of drilling the holes in steel, but I supposed, at one time, that a great diminution in the proportion of loose rivets to tight ones would be one of the results of the process; and in this expectation I have been somewhat disappointed. This much may be said, however,—that, in a rough hole, a slight looseness may not be detected, as a rivet may be jammed, so to speak, between projecting points; while, in a smooth, drilled hole, the slightest space between the rivet and the surrounding metal will cause motion under the hammer.

10th. After all endeavors to account, in some systematic manner, for the looseness of rivets, there is left a large margin of irregularity and absolutely nothing can be said with certainty as to the rivets in a particular piece of work, until they have been tested. Out of ten similar pieces, similarly riveted to all appearance, nine may show only one or two loose rivets each, while the tenth may look, after testing, and marking, like a constellation of chalk-circles. And even then, it cannot be said that all the loose rivets have been found, but only that none more are *known* to exist.

11th. For the detection of loose rivets, the only method I know of is to strike each rivet with a hammer. In my experience, the motion indicating looseness is more reliably ascertained by touch than by sight or sound. As to the precise manner of testing, it will, of course vary with individuals. My own method has been somewhat as follows: I strike a couple of blows with the hammer on each side of the rivet-head, placing the finger, at the same time, on the opposite side of the head, and in contact with the surface of the plate or piece riveted, so as to feel the motion, if any, between the rivet-head and the solid metal. The motion is much more readily felt by touching the head which is struck, than by holding the opposite head of the rivet, as is sometimes done. Looseness is also more readily detected by striking the original head of the rivet than by striking the head which is made in riveting.

In inspecting rivets counter-sunk on both sides, the only possible test is obtained by striking directly on the head of the rivet, at the same time placing the fingers on the opposite head, to detect longitudinal motion. This should be done, if possible, from both sides of the part riveted.

A light hammer (about one or one and a half pounds) is preferable to a heavy one. The blows struck should not be too strong, but such as may be struck freely from the wrist.

The mean spotted area of the sun was slightly greater in 1883 than during preceding year, although the faculæ showed a small falling off. For 1883 Greenwich Observatory photographs are available on 215 days, and Indian photographs filling up gaps in the series on 125 days, making a total of 340 days out of 365 on photographs measured. In 1882 the total number of days was 343, viz., Greenwich series 201 days, supplemented by Indian photographs on 142 days.

PHYSICAL TESTS OF MALLEABLE CAST IRON.

By Prof. PALMER C. RICKETTS.

Written for Van Nostrand's Engineering Magazine.

The physical tests of malleable cast iron here recorded were made on the 50,000 pounds Olsen testing machine of the Rensselaer Polytechnic Institute. The metal was procured from two different manufactories in different States, and the iron from which it was converted was cast from patterns made specially for test specimens. Results were also obtained for some of this cast iron in tension and as beams in order to compare it with the malleableized material.

Strains for given increments of stress for a few of the bars in tension are given in Table VI., *but the "elastic limit" given in the tables is the stress at which the metal refused to hold up the scale beam*, and does not give the point below which the ratio of stress over strain is essentially constant. So determined, being a function of the time, it may be stated that except in cases where the strain was measured, no longer time was allowed, within the "elastic limit," than that necessary for the beam to come to rest. After this point was reached the piece was immediately broken in a length of time, counted from the first application of stress, varying from 9 to 15 minutes. When strains were measured the time varied from 20 to 30 minutes.

The dimensions of the specimens though recorded to hundredths of an inch were measured to thousands, and where increased accuracy would result, were so used in the calculations. Generally, however, the measurements taken showed that on account of the variation in cross-section of the material no such increase would be so obtained.

The bars whose tensile strength was determined were gripped by wedges resting in ball and socket joints, and with the exception of those recorded in Table V. were tested as they came from the furnace with the "skin" on. In these bars the length in which the elongation was measured always contained the fractured section, and in the tables the reduction of area at this point is given as a per cent. of that of the original section. The intensity of stress is also given in terms of the original section.

In Table I. is given the results of the tests of square bars of malleable cast and cast iron. The pattern from which Nos. 1 to 10 were cast was 14 inches long, and one inch square near the ends. About 4 inches from the ends it was beveled down to the section given in the third column, the length of the middle part being 5.5 inches. The elongation was measured in 5 inches in all of the malleable specimens. Nos. 9 and 10 were fractured at the bevel, as was always the case with the cast iron when not of uniform cross-section throughout. The malleable iron never broke at this point.

Nos. 11 to 17 were bars of nearly uniform size, Nos. 11 and 12 being the two ends of the same 24-inch piece, which was sawed in two at the middle before testing. The variation in ultimate resistance of these two is seen to be about two per cent. of the greater. No. 13 was 24 inches in length, and after it had been pulled apart one of the pieces, 18 inches long, was again subjected to tension, and the result is shown as No. 14. The ultimate stress was greater in the latter case than in the former, and it is believed that this may happen with any specimen, as the variation in the material of the same bar may be more than great enough to cover any diminution of resistance caused by the bar having been previously broken. In No. 14 the elongation in 10 inches was 0.19 inch, giving 1.9 per cent., whilst in 5 inches it was 0.11 inch, giving 2.2 per cent. as shown.

The rectangular bars in Table II. were 20 inches long, the middle 10 inches of each being of the size there given. The ends clutched were 1.20×0.45 inch in cross-section, and were beveled down to the smaller central part. The elongation was measured in 7.5 inches. No. 2 broke at an imperfection. The increments of strain for 1,000 lbs. increments of stress of Nos. 6 and 7 are given under Nos. 7 and 8 of Table VI. Nos. 8 and 9 of the cast iron broke at the bevel and the cen-

Table I.

Tenacity, Elongation, and Reduction of Area of Square Bars of Malleable Cast Iron and of Cast Iron.

No.	Metal.	Dimensions of Specimen in Inches.	Stress in Lbs. per Sq. In.		Final Elongation per cent.	Reduction of Area per cent.
			Elastic Limit	Ultimate.		
1	Malleable cast iron..	0.78×0.78	1150	39400	4.6	7.2
2	" " ..	0.79×0.78	1140	39500	5.0	7.0
3	" " ..	0.79×0.79	800	40480	6.0	7.2
4	" " ..	0.79×0.78	1130	36130	6.6	6.4
5	" " ..	0.79×0.78	980	37190	4.4	6.4
6	" " ..	0.79×0.78	980	39480	5.0	6.9
7	" " ..	0.81×0.80	1080	44290	7.6	8.0
8	" " ..	0.80×0.78	810	42100	5.8	6.4
9	Cast iron...........	0.77×0.77	500	19360		
10	"	0.77×0.76	510	20450		
11	Malleable cast iron..	0.74×0.73	3680	34070	2.2	6.8
12	" " ..	0.73×0.72	2390	33400	1.8	3.5
13	" " ..	0.75×0.75	1770	30970	2.0	4.1
14	" " ..	0.75×0.74	1260	31980	2.2	4.4
15	Cast iron...........	0.76×0.74	540	22320		
16	"	0.74×0.73	550	25780		
17	"	0.75×0.74	630	20650		

Table II.

Tenacity, Elongation, and Reduction of Area of Rectangular Bars of Malleable Cast Iron and of Cast Iron.

No.	Metal.	Dimensions of Specimen in Inches.	Stress in Lbs. per Sq. In.		Final Elongation per cent.	Reduction of Area per cent.
			Elastic Limit	Ultimate.		
1	Malleable cast iron..	1.02×0.38	3850	33800	2.3	5.6
2	" " ..	1.02×0.39	3770	32750	2.0	6.1
3	" " ..	1.02×0.40	3680	34250	1.6	6.9
4	" " ..	1.01×0.39	2300	36990	3.2	7.7
5	" " ..	1.00×0.38	1320	34870	2.3	7.4
6	" " ..	1.02×0.39		35000	2.7	5.5
7	" " ..	1.01×0.39		33600	2.8	7.2
8	Cast iron...........	0.99×0.38		14630		
9	"	0.99×0.38		19410		
10	"	0.99×0.38		27660		
11	"	0.99×0.38		29440		

tral part of the specimens when tested gave the results shown by Nos. 10 and 11.

All the pieces in the following table, except Nos. 7, 8 and 9, were larger at the ends, and the smaller middle portion was 10 inches long. The three exceptions noted were 24 inches long and of approximately uniform cross-section. In No. 7 the elongation in 7.5 inches was 0.34 inch, or 4.5 per cent., as shown, and in 9.5 inches it was 0.38 inch, or 4 per cent., both lengths containing the fractured section. The cast iron broke at the bevel between the lesser and greater cross-section, without perceptible elongation or contraction.

Table IV. contains results of tests of bars

Table III.

Tenacity, Elongation, and Reduction of Area of Circular Bars of Malleable Cast Iron and of Cast Iron.

No.	Metal.	Diameter of Specimen in Inches.	Stress in Lbs. per Sq. In.		Final Elongation per cent.	Reduction of Area per cent.
			Elastic Limit	Ultimate.		
1	Malleable cast iron..	0.59	2600	36600	2.7	14.7
2	" " ..	0.60	2300	37800	3.1	9.9
3	" " ..	0.57	2400	38400	3.2	10.3
4	" " ..	0.57	2200	39200	3.3	10.3
5	" " ..	0.55	1700	44680	5.1	10.6
6	" " ..	0.54	2190	43640	4.8	10.3
7	" " ..	0.51	1970	39160	4.5	10.3
8	" " ..	0.51	2900	36230	2.5	5.7
9	" " ..	0.53	2260	36200	2.0	8 1
10	Cast iron...........	0.56	1440	23050		
11	"	0.55	1920	22650		

Table IV.

Tenacity, Elongation, and Reduction of Area of Circular Bars of Malleable Cast Iron and of Cast Iron.

No.	Metal.	Diameter of Specimen in Inches.	Stress in Lbs. per Sq. In.		Final Elongation per cent.	Reduction of Area per cent.
			Elastic Limit	Ultimate.		
1	Malleable cast iron..	0.79	2040	29390	0.4	7.4
2	" " ..	0.81	1370	28570	0.8	3.9
3	" " ..	0.79	1120	33670	2.5	5.0
4	" " ..	0.79	1330	39430	3.7	4.4
5	" " ..	0.79	1440	40860	4.1	9.0
6	" " ..	0 80		32300	0.8	3.2
7	" " ..	0.79		32900	1.2	0.0
8	" " ..	0.79		36700	1.4	5.4
9	" " ..	0.79		36900	2.0	7.9
10	" " ..	0.79		36000	1.5	6.7
11	" " ..	0.81		32400		2.7
12	" " ..	0.81		29900	0.0	0.0
13	" " ..	0.78		34500	1.6	1.1
14	Cast iron...........	0.78	1150	21350		
15	"	0.78	940	27820		
16	"	0.78	930	24840		

of uniform cross-section 20 inches long. In Nos. 6 to 10 inclusive the elongation was measured in 10 inches, and in the others in 7.5 inches. It was not measured in No. 11, which broke outside the marks. In Nos. 4 and 5 it was in 9.5 inches, 3.7 and 4.5 per cent. respectively, and in 11.5 inches 3.6 and 4.6 per cent. No. 2 broke at a fault. In Nos. 7 and 12 there was apparently no contraction at the fractured section. In the cast iron No. 14 broke at a fault and No. 15 in the wedges. In these last three pieces the elongation and contraction were too small to be measurable.

In Table V. the specimens were turned from bars of diameter, shown in column headed "original diameter." The smaller tensile resistance of this metal with the skin off is rendered evident. An average of 15 tests of bars like those in Table IV. with the skin on, gives a resistance of

TABLE V.

TENACITY, ELONGATION, AND REDUCTION OF AREA OF CIRCULAR BARS OF MALLEABLE CAST IRON WITH THE SKIN TURNED OFF.

No.	Original Diameter in Inches.	Diameter of Specimen in Inches.	Stress in Lbs. per Sq. In.		Final Elongation per cent.	Reduction of Area per cent.
			Elastic Limit	Ultimate.		
1	0.79	0.74	1180	29950	1.2	3.7
2	0.80	0.74	1160	28840	0.1	4.1
3	0.79	0.76		33300	1.3	5.1
4	0.79	0.74		34600	0.8	2.4
5	0.78	0.75		31700	1.1	3.8
6	0.79	0.73		30190	0.7	1.6
7	0.79	0.60	1430	26430	0.5	3.5
8	0.80	0.63	960	28110	1.1	3.7

34,600 lbs. per square inch, whilst the 6 tests below, of specimens of the same original diameter as those above, from which about one-fiftieth of an inch had been turned, give an average of 31,430 lbs. When the bars were turned still more until their diameter was reduced about one-fifth of an inch the average of Nos. 7 and 8 in the table gives 27,300 lbs.

The elongations corresponding to increments of stress, as given in the table below, were measured with an instrument constructed for Prof. Burr. Through each of its two-ring shaped parts, which encircle the bar to be tested, pass three converging screws with milled heads whose sharp-pointed ends penetrate the bars at sections whose initial distance from each other is known. Extending vertically upward from the lower ring and diametrically opposite to each other are two steel rods with polished tops. These are clamped tightly to the ring near one of their ends, and their polished tops are vertically under the pointed ends of two micrometer screws which pass through projections in the upper ring. The parts being fastened as described, and the micrometers being screwed down until their pointed ends are in contact with the tops of the rods, the readings are taken. This contact may be detected by the minute shadows on the polished surfaces as the points approach. When for a given increment of stress the bar stretches the points separate from the surfaces, and when again brought in contact the mean of the differences of the readings of the micrometers before and after the increment of stress is applied gives the corresponding strain. This instrument reads to ten-thousandths of an inch. Like other similar ones its object is to determine the point at which the ratio of stress over strain ceases to be essentially constant.

In Table VI., Nos. 3 to 8 give the strain, in 7.5 inches, for 1,000 lbs. increments, starting at 1,000 lbs.

In Nos. 1 and 2, for which the strain in 7.5 inches was also measured, the first contact was made at 300 lbs., but at these low stresses the results were not generally satisfactory, probably on account of initial bends. These strains are seen to be quite variable. Part of this is, of course, due to inaccuracies in setting and reading, but in the use of the same instrument on high steel, for instance, greater uniformity is obtained. The increase in the strains as the bars approach their ultimate resistances is evident. The micrometer was always removed when the breaking point was supposed to be near. No. 2 broke at 14,400 lbs., and No. 4 at 12,650 lbs.

In the lowest horizontal line is given the mean of the strains for the 1,000 lb. increments between 1,000 and 7,000 lbs. Taking the mean of the three corresponding to the circular sections with the skin on, we obtain for the ratio of stress over strain between the above limits 24,350,000 lbs. In like manner for the "rounds" with skin off, and the rectangular sections we get respectively 23,148,000 and 24,038,000 lbs. The bars in this table taken in their order from No. 1 are the same as Nos. 5, 1 and 13, Table IV., Nos. 6, 4

TABLE VI.

STRAINS IN BARS OF MALLEABLE CAST IRON.

No.	1	2	3	4	5	6	7	8
Stress in Pounds.	Strains in Ten Thousandths of an Inch.							
	"Rounds," Skin on Mean Diameter, 0.79 Inch.			"Rounds," Skin off Mean Diameter, 0.74 Inch.			Rectangular, Skin on Mean Section, 1.02 x 0.39 In.	
300								
500	2.62	4.87						
750	1.75	0.87						
1000	1.25	4.87						
2000	5.00	2.50	7.37	6.75	5.25	5.62	5.00	7.00
3000	5.75	4.25	5.87	5.50	4.37	5.62	8.12	6.37
4000	5.00	7.12	8.00	8.75	7.12	10.62	9.75	7.62
5000	6.62	5.50	6.00	7.50	9.00	13.37	8.00	7.87
6000	7.62	6.12	8.00	7.75	5.50	8.25	9.12	7.50
7000	6.87	6.12	9.50	8.62	8.37	7.75	8.75	9.00
8000	5.62	8.25	7.12	11.25	10.75	7.87	11.00	11.12
9000	4.62	5.75	11.00	12.25	13.37	10.62	10.62	11.62
10000	5.12	6.87	14.87	19.50	14 25	14.62		
11000	8.00	9.12	15.75	36.12	22.50			
12000		9.87						
13000		18.87						
	6.14	5.27	7.46	7.48	6.61	8.54	8.12	7.56

TABLE VII.

COMPRESSIVE RESISTANCE OF SHORT BLOCKS OF MALLEABLE CAST IRON AND OF CAST IRON.

No.	Metal.	Dimensions of Sections in Inches.	Length in Inches.	Stress in Lbs. per Sq. In.		Angle of Shear.
				Elastic Limit	Ultimate.	
		Rectangular.				
1	Malleable cast iron..	0.514×0.518	1.460	17000	128800	56°
2	" " ..	0.514×0.515	1.460	18900	109400	
3	" " ..	0.513×0.515	1.460	18900	108900	
		Circular diam.				
4	" " ..	0.510	1.248	15660	140200	61°
5	" " ..	0.508	1.252	9360	121230	57°
6	" " ..	0.509	1.249	15270	123150	60°
7	" " ..	0.592	1.246	10900	148400	47°
8	" " ..	0.591	1.246	9120	160950	47°
9	" " ..	0.590	1.247	10620	159340	42°
10	Cast iron...........	0.513	1.248	16900	144900	
11	"	0.506	1.248	16410	151700	62°
12	"	0.512	1.247	17560	158500	67°

and 5, Table V., and Nos. 6 and 7, Table II.

The compressive resistance of short blocks of square and circular section of this metal was found, and also that of three blocks of round cast iron. The dimensions of the pieces and the results of the tests are given in Table VII. All had the skin on with the exception of Nos. 7, 8 and 9, which had been turned

down from a bar 0.79 inch in diameter, the same bar given as No. 7 in Table V. With the exception of Nos. 2, 3 and 10, the specimens failed by shearing, the angle made by the plane of shear with a normal section being shown by the numbers in the last column. Nos. 2 and 3 failed without shearing, and were unable to raise the beam when their lengths were reduced to 0.98 and 0.99 inch. No. 9 sheared in two planes nearly at right angles to each other. No. 10 crushed down and broke in pieces with no well defined plane of shear. The angle of shear is seen to be greater in the cast than in the malleable cast specimens. A comparison of the specimens of the same diameter and length show that the compressive resistance of the malleable iron is about 0.85 of that of the cast.

Table VIII. gives the reduction in the length of the short blocks numbered from 4 to 9 in the preceding table. The cast iron specimens were reduced but slightly, at 20,000 pounds about 0.04 inch, compared with 0.25 inch for malleable specimens of the same cross-section.

Table IX. contains the results of experiments on beams of malleable cast iron to determine the center weight at the elastic limit and at rupture, and therefore the value of K, the intensity of stress in the extreme fiber at these points.

TABLE VIII.

REDUCTION IN LENGTH OF SHORT BLOCKS OF MALLEABLE CAST IRON UNDER COMPRESSION.

No.	4	5	6	7	8	9
Stress on Specimen in Pounds	Reduction in Length of Specimen in Inches.					
8000	0.04	0.04	0.04	0.04	0.03	0.03
12000	0.10	0.11	0.09	0.05	0.06	0.06
16000	0.19	0.19	0.19	0.11	0.10	0.10
20000	0.25	0.25	0.25	0.16	0.16	0.16
24000	0.29	0.47	0.47	0.23	0.23	0.23
28000	0.38			0.34	0.32	0.32
32000				0.39	0.39	0.39
36000				0.49	0.46	0.46
40000				0.59	0.54	0.54
44000					0.63	0.63

Two sets were made; in one the bars were, as shown, about 1.5 inch square, and in the other about half of that, the spans varying from 15 to 4 inches.

The larger bars used for the 6 and 4 inch spans had previously been broken as 15 and 12 inch beams. This made the second ruptured area 4 or 5 inches from the first, and it is believed that, from the nature of the material, the fact that the parts had been previously broken did not materially decrease their center-breaking weight in the shorter spans. This breaking weight in all the spans varied decidedly with the appearance of the interior of the bar. When this was homogeneous and dark, until the center was nearly reached, a greater resistance was obtained than when the center lighter part extended over a greater portion of the section. The results given for the smaller beams were obtained from two 24-inch bars, which were first broken at their centers as 15-inch beams, and their ends then used for the 9 and 6 inch spans.

In both sets it will be noticed that the results for any length of span are not very accordant and, on account of the fewness of the bars of smaller cross-section tested no deductions with regard to the value of ultimate K can be drawn, but from the larger ones the increase in its value with the decrease of span is clearly shown, though this could not always be inferred from the successive pairs. The means of these values for the successive spans from 15 inches down are 64,600, 54,000, 73,000, 78,600 and

TABLE IX.

CENTER WEIGHT AT THE ELASTIC LIMIT, CENTER BREAKING WEIGHT, AND VALUES OF K AT THESE POINTS FOR BEAMS OF MALLEABLE CAST IRON.

No.	Span in Inches.	Breadth in Inches.	Depth in Inches.	Center Weight at Elastic Limit. Pounds.	Center Breaking Weight. Pounds.	K. El. Limit. Pounds.	K. Ultimate. Pounds.	Deflection at Breaking in Inches.
1	15	1.47	1.51	2800	7750	18800	52080	0.29
2	15	1.48	1.50	5100	10250	34460	69260	0.96
3	15	1.51	1.53	1500	10700	9550	68000	0.99
4	15	1.52	1.52	1100	10800	7050	69230	0.99
5	12	1.50	1.52	2900	8750	15060	45450	0.29
6	12	1.51	1.49	3000	12900	16110	69290	0.65
7	12	1.51	1.52	2000	9100	10320	46960	0.11
8	9	1.49	1.49	2600	15500	10630	63410	0.32
9	9	1.51	1.53	2400	20200	9170	77180	0.54
10	9	1.53	1.53	1500	20700	5660	78110	0.50
11	6	1.46	1.50	2800	31000	7670	84930	0.35
12	6	1.51	1.52	1500	22200	3870	57280	0.15
13	6	1.49	1.44	1100	30800	3210	89870	0.41
14	6	1.50	1.51	700	31300	1840	82370	0.29
15	4	1.48	1.50	2100	37000	3780	66660	0.15
16	4	1.49	1.49	1500	47400	2720	85980	0.14
17	4	1.48	1.50	2400	37500	4320	67570	0.31
18	4	1.52	1.52	1500	50000	2560	85470	0.18
19	4	1.52	1.54	1500	50000	2500	84000	0.15
20	15	0.74	0.75	800	1300	43300	70310	2.25
21	15	0.73	0.73	750	1350	43380	78080	2.00
22	9	0.74	0.74	900	1950	29710	64360	0.52
23	9	0.72	0 73	1050	2500	36490	86870	1.50
24	6	0 72	0.74	900	3500	20756	80720	0.47
25	6	0.73	0.74	1200	3750	26960	84270	0.61

77,900. The small value for the 12-inch span is accounted for when it is stated that Nos. 5 and 7 were defective pieces, No. 5 breaking 0.25 inch from the center at a fault, and No. 7 one inch from the center at a section, a large portion of the area of which was light in color and non-homogeneous. No. 12 broke at a fault 0.5 inch from the center. In the 4-inch spans, although No. 15 did not break at a fault, the interior, lighter, weaker portion of the metal, formed a much larger part of the area of the section of fracture than usual, about 40 per cent. In No. 18 the stress was kept at 50,000 lbs. for 10 minutes, at the end of which time the bar broke, whilst in No. 19 the specimen was not broken, as it refused to yield after 50,000 lbs. had been kept on for half an hour. Hence the K for this bar is not ultimate K, and its mean value for this length of span is lower than it ought to be. The table also shows that the center weight at the elastic limit, and hence K at that point, though exceedingly variable for each span decreases decidedly with the length of the span.

The results for the smaller bars however, as well as those for the larger, serve to compare the resistance of malleable cast iron with that of the cast iron from which it was made. Experiments on the latter are given below, the spans being made the same so that the results are directly comparable. In the beams of smaller cross-section the center breaking weight of the cast iron in the 15, 9, and 6-inch spans was 69.0, 63.6, and

Table X.

Center Weight at the Elastic Limit, Center Breaking Weight, and Values of K at these Points for Beams of Cast Iron.

No.	Span in Inches.	Breadth in Inches.	Depth in Inches.	Center Weight at Elastic Limit. Pounds.	Center Breaking Weight. Pounds.	K. El. Limit. Pounds.	K. Ultimate. Pounds.	Deflection at Breaking in Inches.
1	15	1.49	1.49	2400	4800	16330	32660	0.08
2	15	1.46	1.46	1000	5800	7240	41970	
3	9	1.48	1.48	1600	10100	6620	41740	0.05
4	9	1.47	1.50	1200	10400	4880	42260	
5	6	1.48	1.49	1200	13700	3290	37530	0.00
6	6	1.45	1.45	800	14000	2360	41380	
7	6	1.49	1.50	600	16600	1610	44560	0.02
8	4	1.46	1.50	1500	23200	2740	42370	
9	4	1.48	1.48	2000	19000	3680	34970	
10	4	1.48	1.49	1500	19500	2750	35820	
11	15	0.74	0.77		950		48720	0.18
12	15	0.73	0.74		850		47810	
13	9	0.72	0.73		1400		49250	
14	9	0.73	0.75		1400		45980	0.10
15	6	0.74	0.75	1900	2250	41050	48610	
16	6	0.73	0.74	1200	1700	27000	38250	
17	6	0.74	0.77	1000	2300	21040	48390	

55.5 per cent. respectively, of the malleable and in the larger beams of 15, 9, 6, and 4-inch spans, the per cents. were 53.5, 54.2, 51.4, and 46.4. Though the bars compared are not of exactly the same cross-section, these figures show that the difference in resistance between the malleable and the cast iron beams is greater the shorter the span, and also greater the larger the cross-section. A comparison of the last columns of both tables shows to what extent the flexibility of the material has been altered by the conversion. The deflection of the shorter spans of cast iron were generally not appreciable.

Nos. 5 to 10 inclusive and 16 and 17 had previously been broken as beams and Nos. 11 to 17 inclusive had been in tension. In Nos. 11 to 14 the elastic limit, as determined by the action of the scale beam, did not seem to be reached before rupture, and its general variability is evident from an inspection of the table.

The appearance of the fractured section of this metal varies materially from that of cast iron. Around the outside invariably is found a thin layer of a light gray color from 0.02 to 0.03 inch in thickness. At the center there is generally a core varying in color and size; in color from bluish gray to light gray and in size from nothing to a large proportion of the section, in one case 54 per cent. This core not only varies in size in different bars but changes materially in different parts of the same bar. In one of the larger specimens broken as a 12-inch beam it was about 3 per cent. of the section, and when one of the pieces was again used for a 4-inch span it reached 50 per cent., of the fractured area. It is liable to form a larger proportion of the area of large bars than of small ones, rarely reaching in the smaller pieces tested 20 per cent., and in some cases appearing only as a few light specks on the darker portion of the surrounding material. The larger it is the weaker the iron. Uniformly when a high resistance was obtained the dark gray part between the outer thin layer and the center was large and the core small. That the material near the center of the bar has less tensile resist-

ance is shown conclusively in Table V. and in the beams, as has been remarked in connection with Table IX., a high breaking weight and small core were always found together. It would naturally be assumed, from the method of conversion and from the known effect of the size of the central lighter portion on the physical properties of the malleable iron, that this part consisted of the original cast iron either partly or wholly unconverted. Mr. J. M. Sherrerd, chemist to the Albany and Rensselaer Iron and Steel Co., kindly made some color tests, for combined carbon, of borings made at different distances from the center of one of the inch and a-half square bars. The amount was found to be about the same in the outer thin light layer and in the darker part of the bar between this and the core. In each case it was less than 0.08 of one per cent., practically none. It might seem that there would be a gradual increase in the quantity found going inward from the surface, but the comparisons made showed that, in this case at least, the amount did not grow larger until the core itself was reached, metal taken from its edge giving the same results as that from the exterior of the bar. Tests of material taken from the center gave from 0.44 to 0.31 of one per cent., the first from the finer portions of the borings and the other from the coarser, the differance being probably due to the fact that the parts higher in carbon being harder would be more finely divided in the lathe. Mixtures of the two gave results varying between these limits.

No analyses were made to determine whether the amount of graphitic carbon remained unchanged during the process of conversion.

MEASUREMENT AND FLOW OF WATER IN DITCHES.

By CHAS. E. EMERY, Ph.D.

Written for Van Nostrand's Engineering Magazine.

In the valuable paper by Mr. Aug. J. Bowie, Jr., on the above subject, published in the January number of the Magazine, there is given on page 34 the accepted formula:

$$Q=ac\sqrt{rs},$$

and the notation is described in the following language:

"Q—is the quantity of water which the ditch is capable of carrying in cubic feet per second.

"a—the effective area of cross-section of ditch as constructed originally, in square feet.

"r—the hydraulic mean depth in feet.

"s—the fall of surface in a unit of length.

"c—a coefficient covering all common losses."

An examination of the examples given shows that Mr. Bowie has used in the calculation the *average depth* of the stream, not the "hydraulic mean depth," as stated in the description of the notation. The latter term is a technical one, referring to the area divided by the wetted perimeter. (The words "in feet" are therefore unnecessary.)

Full dimensions are not given from which to calculate r on the correct basis, but, assuming probable proportions for the ditches, it will be found that for the "Texas Creek Branch Ditch," c should equal 109, instead of 59 as stated, and that, for the flume in connection with that ditch, c should equal 180, instead of 59 as stated. For the La Grange main ditch, c should be 116.5, instead of 52 as stated. The context showing plainly the error in the original paper.

The final formula, instead of being

$$Q=31 \text{ to } 45a\sqrt{rs},$$

should be for the ditches

$$Q=109 \text{ to } 116.5a\sqrt{rs},$$

and the coefficient be increased to 180 for board flumes.

All the examples are not reworked for want of data, but we trust that Mr. Bowie will supplement his valuable paper with another, giving all the data and the constants on the corrected basis.

ENGINEERING INVENTIONS SINCE 1862.*

By Sir FREDERICK JOSEPH BRAMWELL, F.R.S.

From "Iron."

II.

Next to the subject of motors should have come (had I not been led captive by a balloon) that which I am now about to mention, *i. e.*, the transmission of power. Taking this in the restricted sense of the transmission from a part of the machine to another, commonly with the object of varying velocity, one may point to the increasing use of multiple rope-driving gear, in lieu of belts to inclined spur gear for diminishing noise, and to that kind of frictional gearing to which the name of "nest gearing" has been given. Here the frictional driver being acted on at the two opposing sides, strain is removed from the bearings, and the liability of one of the frictional wheels to stand still, and to be fatally injured by having a flat rubbed upon it is avoided. In that very important branch of transmission, wherein power is taken to long distances, however, we have the development of hydraulic transmission, as is evidenced by the fact of pipes being now laid down through our towns for supplying water under the 700 lbs. on the square inch pressure for motive power; we have companies authorized, if not at work, for laying down pipes to distribute compressed air; we have now, by reason of the improvement in gas engines, the ability to lay on power in every town illuminated by gas, which practically means every town and large village; and we have in New York, and in some other cities of the United States, high-pressure steam, conveyed in mains below the streets, to be used both for power and for heating, for which second purpose, however, it should be remembered, the contents of a gas main are equally available. I will not touch upon other modes, except just to mention the rope system at Shaffhausen; but I think we may take it as clearly established that we are, day by day, becoming more alive to the benefit, where little power is required, or where considerable power is required, but only intermittently, of deriving that power from a central source.

Under the heads of motors and of transmission of power (both of them, it is true, eminently subjects for the civil engineer) I have spoken of water, but there is another way in which water is used, the way with which engineers and the public are more familiar, viz., its employment for the supply of our towns, which I have not as yet mentioned. Except in the magnitude of the work and the excellence of the design, of which the new Liverpool waterworks now in progress may well stand as a typical example, there is not much to say as regards progress in those waterworks which are dependent upon storage. Indeed there is nothing very marked to point to in these twenty-two years in the way of progress in pumping machinery. Having visited the United States and Canada twice within the last two years, and having seen the waste of water that takes place in both those countries (a waste which not only causes the mains to be incapable of keeping up the pressure under the excessive draught, but renders sources of supply insufficient which otherwise would be ample for years to come), I cannot but rejoice at the progress that has been made here in the matter of house fittings, by which waste has been greatly checked, and the risk of contamination that formerly existed with certain closet fittings is ended. This question of house fittings has always been a difficult one, and it becomes impossible to be grappled with by water authorities, such as those in the United States and in Canada, *i. e.*, municipal authorities afraid of offending the voter. We owe it, however, to Mr. Deacon, the engineer of an English municipal authority that it is now possible to deal with the correction of household fittings at a minimum of cost, and, what is equally important with a minimum of annoyance to the householder. By the employment

* Address of Sir Frederick Joseph Bramwell, F. R. S., on his election as President of the Institution of Civil Engineers, January 13th, 1885.

of the waste-water meter, situated under the flagstones of the footway, and controlling a group of houses, it is possible to find out the total waste in the whole of those houses and on the mains supplying them; then to localize that waste so as to attribute its true proportion to the houses that are the offenders, and to attribute the proportion, if any, to the pipes of the suppliers of water. Having ascertained these facts, not only can the suppliers of water cure the defects in their pipe system, but they are enabled to cure the household waste, not by the expensive and annoying process of an inspection of the fittings throughout the whole district, involving the annoyance of, say, ninety householders whose fittings are in perfect order, to detect the ten householders whose fittings are in a reprehensible condition; but by the mere visitation of those ten who are in default, and who cannot, therefore, complain of the visitation. With respect to the purity of the water supply, this, although it relates to water, is so "burning" a question that I fear to touch upon it. I believe that in most of our towns the supply is satisfactory, but I do believe, in spite of the alarm raised by the suggestion of double mains, we might do well in many cases where there is a pure but limited supply, to have a dual system of mains and thus to distribute the pure water separately and for potable purposes. I am not about to hold up the water supply of Paris as an example for us to follow on all points, but the Parisians at least have recognized the expediency of thus sorting their supply when that supply is of varying quality, and when the best of it is limited in quantity. In cases where there appears to be no thoroughly satisfactory source of water the experience of the efficacy of iron purification, as practised at Antwerp, does hold out very considerable promise.

Gas, likewise, has been alluded to under the heads of motors and of transmission of power and of heat, but I now desire to say a few words in connection with it under its more ordinary aspect, that of a distributed illuminant. In the year 1862 the price of ordinary coal gas in London was from four to five shillings per thousand cubic feet; the illuminating power was such that 5 cubic feet of the gas burnt in a specified burner in one hour should give a light equal to twelve sperm candles, each burning 120 grains in the hour. At that time the consumer was, as it was facetiously called, "protected" by restricting the company to a maximum statutory dividend. Obviously, so soon as this dividend was earned, all incentive to improvement was removed. One of the few cases in which recent legislation relating to private companies supplying public wants can meet with the approval of the political economist, was that which a few years ago first recognized it would be well for the private company and for the public that the ordinary incentive of increased profit for increased exertion should remain, and that introduced in certain gas undertakings the "sliding scale." This provided for a normal price, and for a maximum dividend, but allowed the company to ratably increase this dividend in accordance with a decrease in price below the normal. Under this wise legislation, sixteen-candle gas is sold in London for as little as two shillings and tenpence per thousand cubic feet. But illuminating gas has to be considered by the engineer under two distinct heads: one, its manufacture and distribution; the other, its utilization. This last, it is true, is but to a small extent in the hands of those engineers who have the charge of the first. Considerable progress has, however, been made of late in illumination, largely, it is true, due to a greater liberality on the part of lighting authorities, and the use thereunder of multiple burners in street lanterns, but to a considerable extent due to that much more to be desired improvement, whereby a greater amount of light is obtained from the same volume of gas. The regenerative gas-burners, and other modes of burning, into which time will not permit me to enter, promise to largely increase (it is said, even to more than double) the candle-power per cubic foot of gas burnt. Such improvement as this is undoubtedly of great moment, not only on the score of economy, but on the sanitary ground of diminishing the amount of products of combustion poured into a room in relation to the light therein afforded. It need hardly be mentioned that the decrease in cost and the increase in profits are largely due to the application of chemistry to this manufacture, by

which application the former nuisance-creating by-products have been turned into sources of revenue and into fertilizers for our fields. I have also, in the most cursory manner, mentioned gas as a means of distributing heat; but a word should be said about those valuable improvements in gas-furnaces—I do not mean the Siemens furnace—which have enabled coal-gas to be applied to the melting of even very refractory metals, by means of a most inexpensive plant. Nor have I spoken of those other applications, where, either burnt with coke (it may be of the very coal from which the gas itself was derived), or caused to raise incombustible bodies to incandescence, it forms the cheerful and smokeless substitute for a smoky coal fire, or is utilized for the purpose of domestic cookery. In this latter case, however, if absolute cleanliness and ventilation are not preserved, there will (as the unhappy traveler, compelled to temporarily sojourn in the "limited" hotels of the present day finds to his cost) be one universal dirty gas-oven flavor impressed upon all his food, be it the homely leg of mutton or the lordly haunch of venison.

Although it is quite certain that the first suggestion for using liquid fuel (notably tar, to aid in heating gas-retorts) must date long before 1862, yet the great development of the mineral oil industries since that date has led (and especially in Russia, in whose territory such enormous yields of oil are afforded) to the employment of this material as a fuel in furnaces and in steam boilers. Next to the infinitely divisible forms of gaseous and of liquid fuel comes, as I have said elsewhere, the dust fuel introduced by Mr. Crampton. In the use of any of these three forms, regularity of mechanical supply is a condition involved; and any one of these three, therefore, irrespective of all other considerations, is desirable because it is a means of dispensing with that most unsatisfactory form of labor—"stoking," dispensing also with the production of smoke, and with the diminution of maximum effect attendant on the hand-feeding of coals, where the condition of the fuel in the grate and its temperature must be ever varying. Having regard to these advantages which are to be obtained in using oil, and to the cheapness of the material in Russia, one is not surprised to find that there are lines of steamers on the Caspian worked entirely by liquid fuel, and that the same kind of fuel is used to fire the locomotives in many districts.

I have mentioned the improvement in small furnaces worked by illuminating gas; but I am not entitled to bring within my period the regenerative gas furnace, that great invention made by our lamented friend Sir William Siemens, with whose name in this matter should be coupled that of his brother, Mr. Frederick Siemens. This latter gentleman, by a course of study, has recently discovered, and it is an interesting scientific fact, that so far from the heating power of the flame being increased by its confinement within narrow chambers, and by its being brought into contact with the material to be operated upon, such arrangements only diminish that power; and he has further found that this discovery can be usefully applied in practice by keeping the roof of a regenerative gas furnace at such a height above the hearth on which the materials to be heated lie, that the flame can traverse from one side of the furnace to the other, free of contact with the roof above, or with the materials below. Very excellent economic effects and a high heat, it is stated, have been obtained by causing the outgoing products of combustion to give up their heat to the incoming cold fuel. I have seen such furnaces in operation making steel by the hearth process, and it is the fact that the chimney has been without a trace of red glow within it.

The natural oils which are used as fuel, and to which I have referred, are rarely employed in their crude state as obtained from the wells; but they all undergo more or less refining before use. There is another natural fuel, however, which has been discovered in America, and within the last few years largely utilized—this is the gas obtained from wells in a manner similar to that in which the oil is obtained. It is a marsh gas of high calorific power, and is in certain parts of the United States being used very largely for domestic heating, for the heating of furnaces of every description, including those for the manufacture of plate-glass and of steel; it is also being employed for the manufacture of lamp or carbon black, and for the carbon points for electric lighting. It is stated that within a radius of 20 miles from the town of Pittsburgh, taken

as a center, there are twenty-five wells, each producing 3,000,000 cubic feet per twenty-four hours, and that the produce of the whole of the wells at present opened up is 100,000,000 cubic feet of gas per day. To my mind this is one of the most perfect fuels which can be imagined. It requires no preparation, but can be and is, used in the same state as that in which it issues under a high pressure from the wells; it can be mechanically controlled with the greatest nicety, and when properly burnt is entirely free from smoke or similar defects. When employed in the Siemens regenerative furnace, the producer, which is necessary where coal is used, is entirely dispensed with.

Probably there is no function of the engineer in which the public feel their interest to be so immediate as when he is engaged in supplying to them their food. Prior to 1862 it is true that steam ploughing and various cultivating and reaping machines were in existence; but they have been much developed since, and if the English farmer is to be saved while growing grain, it will be by reason of his availing himself of the labors of the engineer. Unhappily for the farmer, he has not the monopoly of the engineer's services, the products of whose skill are as fully appreciated for the cultivation of the enormous corn districts of the far west by the farmers there as they are in England. Again, unhappily for our farmers, the engineer, by his railways and by his improved steamships, renders it possible for the grain grown in the United States and in Canada to find its way to our markets at a cost for freight so trifling as not to equal that which, a few years ago, would have been paid for transit from one part of England to another. It would not be right to pass away from improvements in agricultural engineering without referring to that which is a distinctive novelty since 1862. I mean the fast-becoming general combination with the reaping machine of string sheaf-binding apparatus. I am afraid I cannot claim for the engineer that recent introduction the "silo." He is rapidly turning his attention to the improvement of the details, and is showing how mechanical appliances can be advantageously used in connection with them. By the aid of silos our grass crops may be saved in the green form, notwithstanding wet and unpropitious seasons; but those who still prefer sweet and sound hay may hope that the engineer will devise some practical mode of artificial drying, and thus enable them to obtain it even in the absence of the sun, and may also hope that the adoption of similar means will save our grain crops, although the harvest may be followed by steady and continued rain. But a question may arise, whether, except for horse feed, we need trouble ourselves about silos or hay, having regard to the fact of the great development since 1862 in refrigerating machinery, which renders possible the importation of frozen meat from Australia, and from other countries. I hope for the sake of the English farmer that there will still be many who will be prepared to pay for English grown beef and mutton, and for real milk and real butter, and that they will not be tempted by cheapness to substitute milk of condensation and butter of oleo-margarine. But I hear the poor farmers are now threatened by a flood of steamboat-transported milk from Holland. While on the question of food the temptation is great upon me to refer to the wonderful improvements that have been made in "milling' since the year 1862; but I must refrain from this and from all other remarks upon the question of food, except to remind you that if the providing of food is one of the great social problems of the present day, another is how to get rid of sewage. This latter problem, however, has been so fully dealt with by my immediate predecessor, Sir Joseph Bazalgette, as to leave me nothing to say.

There are two other most important subjects involving large commercial interests, and in one of the subjects at all events, great modern invention, upon which, fortunately, I need not say one word, as in respect of the first of these—electricity—I can refer you to the volume of lectures delivered here in 1883; and in respect of the second—tramways—I can refer you to the papers which, with the discussion upon them, have already occupied three evenings of this session. I see that our allotted time is already exceeded, and I am thus compelled to leave unsaid much which I should have liked to have told you, touching many things which almost every one of you must remember (each in his own special line of engineering) as being of general interest,

and novel since 1862—railway brakes and signals, for instance; but the subject upon which I have undertaken to speak is so vast, that even with the severe limitation which, as I have stated in the earlier portion of my address, I had imposed upon myself, I find omission is inevitable. Just a few words (and they shall be very few) about our institution. You have done me the honor to elect me your president; and I trust it is unnecessary to assure you that during my term of office I will do everything in my power to uphold the dignity, the honor, the usefulness, and the prestige of the institution; but my efforts alone will not be sufficient; I must ask you—each one of you—to help me, as failing this help the president is powerless. If each one of you, in his own way, works to advance our general interests by attending at our meetings, by bringing his quota of information on the subject which is under discussion to enrich our proceedings, by taking care that in speaking to give this information, the time of the institution shall not be wasted, either by bald repetition of platitudes or by fine oratory, and by remembering that his endeavor should be to add to the general knowledge in the simplest and most concise way possible to him, then I hope we shall be able to say at the termination of my period of office that the institution has not retrograded, but that the ends and aim I am sure we all have in view have been materially enhanced. As I have already said, I will do all that lies in my power in the future as I have done in the past to arrive at such a consummation, and I must ask you—all of you—to assist me, feeling sure as I do that such assistance will be cheerfully and gladly rendered.

FACTS NOT GENERALLY KNOWN CONCERNING ELECTRICAL INVENTIONS.

By J. S. BEEMAN, M. S. T. E.

From "The Engineer."

The history of the transmission of electric energy by high-tensioned currents, its storage and distribution, is very interesting, as showing the development of an idea which has for long exercised a certain fascination for practical men, viz., the transfer of the energy of waste natural sources of force to localities where it can be utilized. As these localities may be situated at a considerable distance from the source of force, the capital invested in the collecting, transmitting, and distributing apparatus must of necessity be the ruling factor in the case from a commercial point of view, and therefore the economy to be effected by the application of some system for storing the energy and changing its tension to such as is found by experience to be the most expedient, is of great importance. Having in view the improvements in secondary batteries which have already been published, and those which we have still a right to expect, the day may not be far distant when the ideas that have been promulgated regarding such a system, applied to the distribution of electricity, will take concrete form. The various steps by which it has attained its present position, and been brought into the ranks of practically applied natural sciences, form the subject of this article. One of the earliest notices formulating such a design appeared in an article in the *Chemical News* in the year 1862, and ran as follows;—"Sitting by the seashore a few days since, we could not help noticing the vast reservoir of mechanical power existing in the ocean. We do not refer to the noisy dash of the waves as they break upon the beach, but to the infinitely mightier although silent and progressive energy exerted in the gradual rise and fall of the tides. By means of appropriate machinery connected with this tidal movement, any kind of work could be readily performed. Water could be pumped or air compressed to any desired extent, so as to accumulate power for future use, or for transport to distant stations. Light of surpassing splendor could be generated by means of magneto-

electric machines; and with a very little extension of ingenuity, every lighthouse on the coast could be illuminated with sun-like brilliancy and with absolutely no expenditure of fuel; the very same mechanical power of the ocean, which in its brute force would dash the helpless vessel to pieces against the rocks, being bound and coerced like the genii in Eastern tales, and transformed by man's intellect into a luminous beacon to warn the mariner against the approach of danger." This idea of utilizing the tidal power was, in 1871, actually embodied in a patent by Ferdinando Tommasi, for the working of which it was proposed to form a company. The apparatus, called the flux-motor, was a machine worked by the action of the tides, by which compressed air was to be delivered to customers through a network of pipes, on lines somewhat similar to those employed for the supply of gas. The idea was certainly somewhat unique, but, as far as can be ascertained, has never been practically tried; had it been carried to a practical commercial issue we should by this time have been in possession of a very cheap source of power for the production of electric energy. The possible application of this great natural force, by enclosing tidal waters just before the ebb sets in, and working motors through their release, has, however, been shown, in most cases, to be probably less remunerative than the utilization of the land so enclosed for other purposes. In 1877 public attention was drawn to this question by the late Sir W. Siemens in his opening presidential address to the Iron and Steel Institute. He then stated that the energy of the Falls of Niagara might be made available by the then known mechanical means—though undoubtedly in a very wasteful manner—saying, to quote his own words, "Time will probably reveal to us effectual means of carrying power to great distances, but I cannot refrain from alluding to one which is, in my opinion, worthy of consideration—namely, the electrical conductor. Suppose water power to be employed to give motion to a dynamo-electrical machine, a very powerful electrical current will be the result, which may be carried to a great distance through a large metallic conductor, and then be made to impart motion to electro-magnetic engines, and ignite the carbon points of electric lamps, or to effect the separation of metals from their combinations. A copper rod, 3in. in diameter, would be capable of transmitting 1000-horse power a distance of, say, thirty miles; an amount sufficient to supply a quarter of a million candle-power, which would suffice to illuminate a moderately sized town." It is of historical interest that in the following year—1878—Sir Wm. Armstrong practically applied this method for the utilization of natural forces in lighting his house at Craigside during the night, and working his lathe and saw bench during the day, by power transmitted through a wire from a waterfall nearly a mile distant from his mansion. In October, 1878, Mr. St. George Lane Fox patented his system for electric distribution by means of secondary batteries, and as far as information is obtainable, this is the first public mention of secondary batteries being employed in such a system, although, as has been shown in a previous article, they had been patented at an earlier date as regulators to the circuit. Mr. Lane Fox's system is an extension of the idea of regulation, and it is worthy of passing note that at this period high-tensioned current dynamos, such as we are now accustomed to—the Brush, for instance—were practically unknown, the Brush patent not then having been published in this country. In November of the same year, a French engineer patented an invention, entitled "Improved means of Transmitting Electricity to Great Distances." Although his views and ideas were erroneous, the patent is interesting as showing that inventive talent was directed towards the subject of this paper, and because of the crude ideas contained in it as to the character of dynamic electricity. In December, 1878, Edison patented a system showing duplicate sets of batteries, one set being in the discharging circuit whilst the other could be in the charging circuit—there was practically no difference of tension in the two circuits. An interesting part in the patent was the apparatus whereby the sets of batteries, when fully charged, were cut out of the charging circuit; this was effected by utilizing the gas given off to lift a diaphragm which actuated a switch or

contact-breaker. In May, 1879, Sir William Thomson gave evidence before the Select Committee of the House of Commons on lighting by electricity, saying, in reference to the utilization of the energy of the Falls of Niagara for the purpose of generating electricity, and in answer to question No. 1799, "There is no limit to the application of it on a general scale. It might do all the work that can be done by steam engines;" he qualified this, and explained how it could be done in a later part of his evidence, by stating, in answer to question No. 1903, that the current sent along the wire would be used to turn electro-magneto machines, which current would be produced by magneto-electric machines. In October, 1879, Luke took out a patent for Houston and Thomson, wherein they showed means by which the tension of the discharging circuit might be raised far in excess of the tension of the current produced by the generator, this being accomplished by charging the batteries in sections and discharging them in series. The special feature of the patent bearing on one subject is contained in the twenty-second claim, and is the method described of bringing a dynamo-electric machine into circuit with a storage battery by first bringing the machine to its normal condition of working by the interposition in its circuit of, first, a normal working resistance, and afterwards of the secondary battery. In March, 1881, Professor Perry, whilst lecturing at the Society of Arts, said that for the future development of the transmission and distribution of electric energy it would be necessary to use electric machines of great electro-motive force. During the lecture, and afterwards in the discussion, reference was made to Professor Ayrton's paper read at the British Association meeting, held the previous year at Sheffield wherein he pointed out, in a full and comprehensive way, how the energy of the Falls of Niagara might, theoretically speaking, be conveyed to New York, along a single ordinary telegraph wire. The system, which was based on theoretical grounds only, and was dependent on exceptionally good insulation, was explained thus: Only a small amount of current would be conveyed along the wire, but it would have an enormous potential; and although the difference of potential between the ends of the telegraph wire would be nearly *nil*, yet the difference of potential at all points along the route, between the line wire and the earth, would be extremely large, so that much energy could be put in at the generating place—viz., Niagara—and a nearly equal amount would be delivered at the motor place—viz., New York. Thus, through the current being so very small, the waste of energy in electric friction would be very slight. In April, St. George Lane Fox, when lecturing before the Society of Telegraph Engineers, carried the system a step further by expounding a scheme whereby the energy transmitted under high potentials could be safely and usefully applied. He is reported to have used the following words: "Electricity could be conveyed from a very great distance at an enormous electro-motive force, which electromotive force could be reduced to any extent by means of a system of condensers, or of secondary batteries, and a suitable arrangement of commutators working automatically, so as to suit the requirements for distribution." Mr. Crompton, during the discussion, said: "As to the use of the secondary or reservoir batteries to act as a store of electrical energy, it is one of those things much talked of, but never yet practically carried out. M. de Meritens told me a few days since that a secondary battery had been recently brought out in France, which gave surprising results; five or six times as much energy can be stored in it as in the Plante battery." About May 16th appeared a letter in the *Times* announcing the arrival of the above secondary battery, called, after its inventor, the Faure accumulator. On May 18th Mr. Alexander Siemens lectured at the Society of Arts, upon "Electric Railways and Transmission of Power by Electricity." He described a number of ways in which high potential currents could be utilized. He drew attention to large central stations fitted with powerful steam engines and dynamos supplying current to secondary batteries which would kep the electric energy stored until required for use. On June 6th appeared the celebrated letter of Sir Wm. Thomson anent the Faure secondary battery, in which he stated that the Faure accumulator always kept charged from

the engine by the house supply wire, with a proper automatic stop to check the supply when the accumulator is full, will be always ready at any hour of the day or night to give whatever light is required." On the 9th he further wrote, when alluding to credit due to the late Sir William Siemens as the originator of the idea of utilizing the Niagara Falls as the natural and proper chief motor for the whole of the North American Continent. "Under practically realizable conditions of intensity, a copper wire of ½ in. diameter would suffice to take 26,500 horse-power from water-wheels driven by the Fall—losing only 20 per cent. on the way—to yield 21,000 horse-power at a distance of 300 British statute miles; the prime cost of the copper, amounting to £60,000, or less than £3 per horse-power actually yielded at the distant station." Early in September he, when delivering his opening address as President of the Physical Science Section of the British Association at the York meeting, went still further, and pointed out that through the introduction of accumulators much smaller conductors could be used than were anticipated by the late Sir William Siemens in 1877, and further, that the necessarily high potential current employed to transmit the current long distances could be converted at its center of distribution into such a low potential current as should be desired for the several purposes at the points of consumption of the electric energy. He went into fuller details regarding the contents of the letter alluded to above, stating that until he heard of Faure's inventions he could only think of step-down dynamos at a main receiving station, to take the energy direct from the electric main with its 64,000 volts, and supply it by secondary 200-volt dynamos through proper distributing wires to the houses and factories and other places, where it could be used for every suitable purpose requiring power. Now, the thing could be done much more economically, and with much greater facility and regularity, by keeping, say, 40,000 secondary battery cells always being charged direct from the supply main, and applying a methodical system of removing sets of 100, and placing them on the town supply circuits, while other sets of 100 were being regularly introduced into the great battery that is being charged. He further described and showed an automatic apparatus which he had designed and constructed to break and make the circuit between the battery and the dynamo. On the 24th of September, Duprez and Carpentier obtained a patent, wherein was described a system very similar, but, of course, giving more definite details of apparatus to be employed than did Sir William Thomson in the address referred to above. They referred to an automatic galvanometer which actuated a commutator or switch, thus causing the connection of the discharging circuit to be always in contact with a very constant supply of electricity. In November, Professor Sylvanus Thompson, at a lecture at the Society of Arts, stated that we had in the tidal basin of the Avon enough energy to light Bristol, and in the channel of the Severn there was sufficient power—if only one-tenth were conserved in accumulators—to drive every loom spindle and axle in Great Britain. By this time the inventive public was thoroughly aroused, and the records of the patent office show that the details of the working out of the above system has received great attention. Nothing new has, however, been shown affecting the principle. Last month news reached this country that the Bell Telephone Company has two wires connected with the Niagara Falls, by means of which the Exchange at Buffalo has recently been operated. As an experiment, a generator was placed on the paper mills of Quimby & Co., at Niagara Falls, and the machinery connected with twenty miles of wire, the result being a success. The various attempts that have been made in this and other countries, notably France, with systems worked on the line herein set forth, are practical evidence of the value of the foresighted idea of the late Sir William Siemens.

At a meeting of the Edinburgh Royal Society, Prof. Tait read a paper by Mr. W. F. Petrie, on the old English mile. The old mile was longer than the present, and consisted of 5,000 feet of 13 inches. It seemed to be identical with the old French mile. The furlong had no connection, originally, with the mile, which was modified to suit the former.

THE APPROPRIATE ORNAMENTATION OF WORKS IN IRON.*

By G. RICHARDS JULIAN, A.R.I.B.A.

From "Iron."

When your able honorary secretary did me the honor to invite me to read a paper at one of your meetings of this session, I was for a few moments doubtful as to whether there was any subject on which I had a right, without presumption, to address you. I, however, ventured to think that the appropriate ornamentation of works in iron, although not a new subject, either to my mind or to yours, was one on which I might offer some suggestions worthy of your consideration. I think that I shall not be considered overbold if I say that this is a problem which still awaits solution, and that, if it is to be solved at all, it must be done either by the united efforts of the engineer and the architect or by the development of a new species of workers, who might be denominated artist-engineers. Some of my professional brethren will at once tell me that every architect is, or should be, an artist engineer, and such a name doubtless defined the architect when engineering was a much less complex business than it has now become. I am not one of those who regret the division of labor which has taken the designing of great bridges, gigantic shed roofs, and similar works out of the hands of the architect; and, to explain my position, let me glance at the work which is left to us. The architect has to make himself acquainted with the history of his art from the earliest times, with its developments in various countries and under differing circumstances, and with the characteristic forms and ornaments associated with those developments. He has to be familiar with the arrangements necessary to be considered in planning domestic buildings, from the cottage to the palace, ecclesiastical structures of all sorts, warehouses and manufactories, banks and offices, hospitals and town halls, hotels and public offices, &c., &c., *ad infinitum*. And in planning these various buildings he must not only arrange them conveniently and economically, but ever keep before him the æsthetic effects which should be produced, dignity or magnificence, or picturesqueness, as the case may be. He must use his materials not only scientifically but artistically; he must keep out the wet, and keep down the damp; he must provide for warming and ventilation, and for those sanitary matters, in which if he fails he will now-a-days soon meet with the reprobation which he will deserve. In his practice he will have to deal with questions of rights of light and air, with the requirements of building acts and local regulations, with dilapidations and valuations, with estimates, and, alas! with the builder's little bill for extra work. He must keep abreast of new discoveries and inventions, as they may affect his work, and he must be an authority on wall papers and other decorative materials, and often, and very properly, on furniture. Add to this several other matters which would take too much time to enumerate, and I think you will agree with me that there is enough for any one man's lifetime without his being called upon to master "strains under moving loads," and so on. Life *is* short, although art and science are long, and I do not believe that, with the continual additions made to our store of knowledge, it is possible for one man to be both a good architect and a good engineer. I will not attempt the unnecessary task of detailing to you the engineer's daily work, but will only say that, busy with his calculations of loads and strains, with his railroads and other great works of utility, he does not, as a rule, find much time to devote to the study of the principles that lie at the root of artistic design, a study which has, too, formed no part of his professional education; and this is my excuse for coming before you to-night. The application of artistic principles to the design of the iron supports, both vertical and horizontal, now so very commonly used in building, has been both delayed and deliberately avoided. The principal reason of this is doubtless the very rapid spread of

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the use of iron, and especially of cast iron, for structural purposes; the designing has been done in a hurry, and all the real thought has been bestowed on the scientific—the utilitarian—side of the question. The engineer has been too busy with experiments, as to the best practical forms and proportions, to give due consideration to artistic treatment, and the architect, educated in the use of other materials more pliable for his purposes, has been too apt to avoid structural ironwork, dismissing it with the summary remark that it is "an inartistic material," and, when compelled to use it, concealing it as much as possible. These remarks, of course, apply only (so far as wrought iron is concerned) to its use for structural purposes; for ornamental purposes, the gates and grilles, the well-covers, hinges, vanes, and finials, the lock-plates and numerous other applications of wrought iron in the middle ages and in the sixteenth and seventeenth centuries, show us truthful, beautiful and skillful design and workmanship, in perfect harmony with the nature and capabilities of the material used. Cast iron is the more serious offender against good taste, as although there are in existence, in the farm houses of Sussex and in other places, panels forming the backs of fire-places, dating from the seventeenth century, of excellent design, and pretending to be nothing but what they really are, yet, in more recent ornamental works, such as railings and gates, cast iron too often imitates the forms either of stone or of wrought iron, and but too rarely is treated honestly and truthfully, as a material cast, and not fashioned or wrought.

It is, however, principally to structural works in iron that I wish to direct your attention, and I think we shall all be agreed that it is now quite time that the appropriate mode of ornamentation of such works should be, if possible, defined; that we must be prepared more and more to use iron in construction of all buildings of any great size, is, I think, a fact that is evident to all; and to prove that some sort of ornamentation must be applied to that ironwork, needs but little argument, since man, so soon as he takes the first step towards civilization, commences to apply ornament to all his surroundings, and the character and the quality of the ornament continue to advance with the subsequent steps, until in the highest and most refined states of society, we find it developed into the most perfect forms of what is called fine art. The desire for ornament in fact, seems to be as much a part of human nature as the desire for food; and it is certainly to be hoped that future ages shall not be obliged to say of this that it could do everything with iron except ornament it appropriately. Many definitions of architecture have been proposed, but the one which I consider the best is, that it is "the useful art of building elevated to a fine art," and directly engineering attempts to do anything more than to erect structures of simple utility, unadorned in any way, directly a curve is introduced, or a moulding is added for the purpose of pleasing the eye, it become amenable to the same artistic laws, and must, or should, conform to the same æsthetic principles which rule and govern architectural design; in short, the engineer who desires to make the structure which he erects ornamental as well as useful, ranges himself at once among the artist workers of the world, and should, therefore, endeavor to make his ornamental work truly artistic. The expression of the sense of beauty, which is inherent in man, is the aim of all the fine arts; and every mode of its expression, whether it be poetry or music, painting or sculpture, or architecture, is subject to positive laws, such as those of harmony and proportion, and to what I may call negative laws, such as the avoidance of falsehood, of coarseness, of tricks, and of vulgarity. Our work is artistic, so far as it obeys these various laws, and ornament then becomes appropriate to the material by means of which we attempt to give expression to the sense of the beautiful.

Let me state a few of the laws which appear to me to apply more especially to our subject, a consideration of which may, I hope, point to the direction in which lies the solution of our problem. First, as to the avoidance of falsehood; any work to be beautiful must be truthful; it must appear to be what it really is. A construction of iron must not be made to simulate one of stone or of wood; cast iron must not be so treated as to look like wrought iron. To say this seems almost unnecessary, it appears so

self-evident; yet, how often—to cite instances which come to one's recollection at once—do we see the cast-iron coping and balustrade of a bridge imitating, as well as they can, the common forms of a stone parapet, and then carefully painted to make the deception as complete as possible; or how often in cast-iron gates and railings are false bands and rivet heads to be found. Such treatments of material not only excite disgust in anyone with even the most elementary knowledge of art, but exhibit a melancholy poverty of thought and want of artistic inventive power in the people who perpetrate them. It may be objected that some forms of stone architecture have originated in imitations of wooden construction, as seen in rock-hewn tombs in Lycia and in Egypt, and in the details of the entablature of the Grecian Doric order; the first attempts at using a new material would naturally, in the infancy of art, be imitative, but before any true style of architecture arrived at maturity such conscious imitations had faded, never to be renewed except in periods of decadence. But this truthfulness must go a step further. The forms of ornamentation appropriate to one material must not be imitated in another, the character and capabilities of which are different. We see how a recognition of the capabilities of the respective materials has produced the refined and delicate forms of Greek detail executed in hard marble, and the boldly undercut moulding and foliage of the best Gothic work executed in comparatively coarse and soft stone. We have also opportunities of seeing how unsatisfactory is the attempt to reproduce the Greek detail in the coarser material, when most of the refinement vanishes; and, on the other hand, deeply-worked carving or moulding in marble always produces an unpleasant feeling of labor lost, of an expenditure of energy which is disproportionate to the artistic result obtained. I may summarize this by saying that ornament to be appropriate must be the natural outcome of the character and capabilities of the material employed, and such as can be executed without undue expenditure of labor; and that ornament should be made, so far as possible, to tell its history truthfully; that which is cast should not imitate forms appropriate to carving or to forging, and *vice versa*.

Another application of this law is: That construction, where it is shown or indicated, must be that which actually contributes to the support of the structure. We need not, of course, show all the construction but when anything is shown that looks like construction, it must not be a sham, constructed merely for ornament; this condemns the cast-iron arches and spandrels sometimes filled in, quite uselessly, under a girder, and many similar arrangements with which we are all familiar. All this points to the conclusion that, to obtain appropriate ornamentation, we must be guided by the best and simplest method of manipulating the material with which we are dealing. We must not start with any preconceived ideas as to forms and details, obtained from the treatment of other materials, but allow the scientific use of each to suggest its artistic forms.

Works in iron have been subject to many severe criticisms when considered in relation to proportion. Proportion is, of course, one of the main elements of beauty in architectural compositions; many buildings, which are almost devoid of ornament, satisfy the eye by such an arrangement of their parts as is generally recognized as being in good proportion; and, on the other hand, no amount of decoration will compensate for the absence of this quality. Proportion finds its expression in the relation of voids to solids, and in the forms and relative heights and breadths of both; in the preservation of a proper scale between the details of ornamentation and the features to which it is applied; in such a combination of supported parts and supports as shall give *apparent* as well as real stability, and in the amount and distribution of light and shade. The adverse criticisms to which I referred have been based on the fact that most of our ideas on proportion have been derived from the observation of buildings, the materials of which require, for structural purposes, relatively larger supports than is the case with those erected in iron. I venture to think that these critics have allowed this fact to lead them astray when they tell us that "true architecture does not admit iron as a constructive ma-

terial";* they are even inconsistent, for they do not themselves desire a wooden post to be of the same proportion as a Doric column; and they are satisfied not only with the Doric proportions, but with those of the Gothic cathedrals, although the material used in both is practically the same. It will take some little time, doubtless, to enable our eyes to be satisfied that the supports in an iron structure are strong enough for their work. To some extent this has already been accomplished, and when the process is complete we shall doubtless hear (other things being satisfactory), praises of the elegant lightness and soaring beauty of such buildings, similar to those which are lavished on the slender piers and groined vaults of the middle ages. I mention these criticisms because I feel that much harm has been already done by them, and more will follow, unless it be understood that they do not receive the assent of all artists and art students. Iron will continue its onward march, even though a Ruskin stand in the way, and if those who work in iron are continually told that they are dealing with an inartistic and an unarchitectural material, how can any improvement be expected? I believe that the time is coming when the idea of an alliance between iron construction and fine art will fail to provoke a smile, even upon the faces of the most thoughtless. Use your iron, then, in the most scientific way. Find your proportions, first of all, by calculations which tell you what is necessary, but do not forget that where there is more than one way of arranging the parts (and when is there not?) you should not rest satisfied until that one has been attained which combines the greatest utility with that which best satisfies the cultivated eye; above all, do not play any tricks, let the apparent construction be the real construction everywhere, and the world will soon learn to see the beauty of the proportions.

That ornamentation may be harmonious, we must take care that no one part of a design be too rich or too bold for the others; this does not, of course, prevent us from making use of the element of contrast by arranging our decoration about certain features; in fact a study of harmonious contrast will lead us to emphasize the construction by means of the ornament, as we find it done in all the great works of the past. In all works of art, refinement must be present, if the result is to be of a high order; not merely the absence of coarseness and clumsiness, but the actual presence of some sort of refinement in delicacy of details, modulation of shadow, and all the little things that show careful study, painstaking attention, cultivated taste, and the determination to be satisfied with nothing short of the best that the circumstances permit. I have dwelt at some length on these principles of art, because in the present day, when we have lost the habit of instinctive artistic workmanship, owing to the various revivals and changes of fashion from which we have suffered, it is only by constant attention to first principles that we can hope to keep on the right path. The great mistake in the revival of one style after another has been that, instead of the principles which guided the architects of the past to such great successes, having been studied, the results have been reproduced in a more or less slavish manner; but that phase is, I hope, passing, and I look to the development of an original and artistic treatment of iron to assist in the movement. Let us now endeavor to arrive at some conclusions as to the manner in which these principles may be applied.

Before coming to the subject of exposed or visible ironwork, I should like to say a few words on the covering of such structural ironwork as is necessarily concealed, I mean in buildings which it is desired to render fireproof. In these cases it is now generally accepted as an axiom "that no building can be fireproof unless all constructive ironwork is protected." And here arises a strong temptation, in protecting the construction with concrete or terra-cotta, to assume that one is at liberty to imitate the forms of stonework, and to revert to old and well-established proportions. The argument commonly advanced is, that in such a case your ironwork is the skeleton only, and that as in the human body, the bones are clothed with a beautiful form unlike themselves, we may clothe our iron skeleton in any beautiful form that we like; but the analogy, as commonly applied, will not hold good; to cover the iron

* Ruskin's *Seven Lamps*.

construction with an imitation of stone construction is to cover a skeleton of iron —which is supposed to be ugly—with a sham skeleton proper to another body —which is held to be beautiful—instead of surrounding the bones with a beautiful exterior that shall still indicate their forms and proportions. The application of our principles to such cases will lead us to indicate the forms of our construction; if, for instance we have to protect a stanchion of the **H** form, its general outline being a square or an oblong, suggests a clothing of similar form; if of the + pattern, the plan of its covering should preserve the cruciform shape in its faces, while the angles might be filled with splays or moldings. Such treatments are truthful, exhibit thoughtful design, and better preserve the proportions of the constructive forms. A circular column would naturally have a circular encasement, but, again, the proportion should not be destroyed, and direct imitation of stone columns should be avoided as far as possible. Girders and cantilevers should be dealt with in the same manner. The faces of the casing can, of course, be decorated with paneling, banding, and so on, in any way suitable to the material used for the purpose, and to its position. We thus see that, even where our ironwork is hidden, attention to artistic principles will guide us to original and appropriate ornamentation.

Coming now to exposed or visible ironwork, let us first glance at the natural treatment of wrought iron. We have here a material which can be rolled, hammered and forged, bent, twisted, or perforated, built up, or framed and jointed in various ways; the webs of plate girders and of cantilevers can be ornamented by perforation, while in built-up girders of the lattice or the Warren kind the actual construction often gives an ornamental form, the various parts—the lattices, for instance—might often be made decorative by being fashioned or cut, especially where they are formed from plates rather than angle or tee irons, and the joints should receive more consideration, artistically, than is usually bestowed upon them. Although no unnecessary features are to be added for the mere sake of ornament, a few pounds' weight of iron added to the necessary features, to allow of their being made ornamental as well as useful, is perfectly legitimate. The application of hammered scroll work to some of the parts of built-up structures, if not overdone, is an evident means of obtaining beauty and giving interest to the work. In large roofs or in bridges I look to the further development of the combination of cast with wrought iron, as opening a wide field for ornamental treatment. When we turn to cast iron we are dealing with a material which, as used for structural purposes, is but little older than the century in which we live, and for the treatment of which there is no direct precedent. Here we have a material which, as its name indicates, is cast in a mold. and which, consequently, shares with other materials which are similarly treated, the liability—owing to the cheapness with which such decoration may be produced—to be ornamented with imitations of carved and hand-wrought work. There are, however, difficulties of manufacture connected with the casting of iron, especially in large pieces for structural purposes, which will guide and limit us in seeking for the appropriate ornamentation of it. One principal point is, that in designing for cast iron, the material must as far as possible be of uniform thickness throughout, otherwise we shall have cracks and flaws in the process of cooling; this shows at once that our ornament should avoid anything like undercut carving, any system of design that calls for projecting knobs or blocks, which cannot be easily cored; it also shows that for the same reason our moldings and surface ornament should be of slight projection, and without deep sinkings or hollows, unless the back of the casting can be made to follow the face surface without affecting the strength, and without adding undue difficulty to the process of manufacture.

In designing a column or stanchion for execution in cast iron, we will consider first its form or plan; the circular hollow form gives of course the most economical use of material, but has certain disadvantages. You cannot get at the interior for examination or for painting, and you cannot easily see that you have necessary uniform thickness of material throughout; for these reasons I am inclined to prefer the

stanchion as being better suited to the character of the material. If, however, a hollow column be used, other forms than the circle may be adopted, such as the square with rounded or molded angles, the regular octagon with or without projecting faces, the irregular octagon—that is, one with four main faces and four subordinate ones which may be molded—and others which will suggest themselves to you. The advantages of these forms of plan are, less likelihood of imitation of stone details, greater appearance of stability, owing to the actual increase in size, as well as to the apparent increase when such forms are seen in perspective, and the advantage which flat faces offer for the proper treatment of the capitals. The last reason also suggests that where circular columns are adopted, slightly projecting, vertical fillets should be added, carrying down the lines of the brackets at the top, and giving an appearance of rigidity to the shaft. In detached stanchions the cross-with-equal-arms plan seems the best in ordinary circumstances, but often the form of the stanchion should, I venture to think, be suggested by the superstructure which it has to carry. In dealing with these supports in elevation, I will speak first of the capital, or top; a capital, in the ordinary sense, as applied to a stone column, is not wanted at all; such a capital is a separate block of stone placed on the shaft, and ornamented by molding or carving cut into its solid mass; these have, however, formed the types from which cast-iron capitals have been generally derived, and most unsatisfactory we all know their effect to be; for instance, we see a Corinthian capital, or some approach to one, in such a position; now from association of ideas we expect to find, below a Corinthian capital, a column of the usual Corinthian proportion, that is, about ten diameters high, including the cap and base; so, by putting this capital, we challenge comparison between the proportion of our iron column and the old stone one, and the consequence is that the iron is denounced as a wretched, skinny abortion, or in some other equally uncomplimentary phrase.

In designing the top of a column or a stanchion for pure utility, as in some position where it is not to be seen, what do you do? You put at the top a projecting flange, somewhat like what we architects call the abacus, but with projection enough to allow of the proper bolting of the girder, or whatever else is to be carried; you then arrange brackets, cast on to carry this top flange; let us treat our ornamental capitals on the same lines and we shall satisfy our artistic principles, and not set agoing any invidious comparisons between our works and those of the masons.

I have ventured to prepare a sheet of illustrations, showing how this may be attempted. I do not, of course, put these designs of mine forward as being perfect, or claim for them anything more than that they are attempts to grapple with the problem of uniting to sensible and natural construction such ornament as is appropriate to the nature of the material. I have everywhere used what I will call the bracket form of capital, and shown how it may be applied to different kinds of columns and stanchions. In dealing with the shafts all the edges may well be molded in a very simple and refined manner; this is almost suggested by the difficulty of obtaining absolute squareness in section, when you have to withdraw a casting from a mold; the surfaces may be paneled and the panels enriched; but all sinkings should, of course, be very slight, not only for the practical reason to which I have already referred, but because this again suggests the character of the material and its method of manufacture. Flutes running the entire length of the column should certainly be avoided; it is difficult to get true lines in them; they make a small column look smaller and taller than it really is, and they are imitations of a familiar form of stone ornament. Horizontal bands around the columns, however, will always tend to increase the apparent diameter, and so are valuable. Short lengths of fluting between these bands, and in such positions as the top of the columns, where they add apparent stiffness and power to support the weight, may be used, but they should be small and sharp in section, instead of wide and flat, as in stonework, as they then suggest the hardness of the material. In the cross-shaped stanchion I have introduced small horizontal stiffeners, or flanges, and endeavored to treat them

ornamentally. Although these only add slightly to the actual strength, they are very valuable artistically, as seeming to bind the whole together. They also serve the same purpose as the bands on the columns by adding apparent breadth. When foliated ornament is used, although we should not imitate stone or wood carving, we can hardly expect to invent an entirely new system of foliage for our purpose, and it is not only quite legitimate to turn to the works of the past for suggestions, but it is our duty to do so, otherwise we throw away a part of our heritage.

In what direction should we make our investigations? We require low-relief, and that sharpness and crispness of outline which should accompany it if it is to be effective; this we shall find in the early Greek work, which was executed in a hard material, in the Byzantine work which inherited the traditions of the Greek, and for surface and paneled decoration—in much of the early Renaissance detail; learning from these beautiful works of former ages we may, without servile imitation, obtain in time a modified and consistent system of iron foliated ornament. One word more under this head: our foliated ornament must be conventional; naturalesque decoration is seldom satisfactory in effect when applied to any part of a structure which appears to be doing work, that is, carrying a load or resisting a thrust, and never, unless it comes direct from the hand of the artist, in the form of carving or of painting. The remarks which I have made as to moldings generally apply, of course, to the ornamental bases of columns. These should have very slight projection, and be in no way imitations of the ordinary stone base with its deep hollows and bold rounds. The use of stanchions will get over all this difficulty, as each face would have its own projecting moldings, possibly connected by a horizontal band. In cantilevers or large brackets, modeled or perforated ornamentation will be used in the spandrel panels, which the forms of these features will suggest; such ornament might be arranged with projection producing the effect on one side and sinkings on the other, so that the thickness of the whole might be fairly uniform in all its parts. Cantilevers should never be cast hollow, but always with a visible web of the necessary thickness only.

In America, whole fronts of business premises are sometimes constructed of iron, and in many cases in our own city, where every inch of window space is of importance, the same system might be adopted with advantage. Such fronts, to be artistically good, will need very careful designing; we must cast ourselves loose from old traditions, and work in the spirit which I have tried to indicate to you. There must be no great hollow sham cornices and strings carried on hollow, closely-spaced cantilevers, such as I have read of in accounts of the American examples. With a sheet of drawing paper and time to spare, I think that I might work out something that should obey the laws of art, but until I should have done that, I am afraid that I can hardly launch into a detailed description of such a front; I should certainly start with honest stanchions and girders, and where I had any surfaces to cover, I think that wrought-iron plates with ornamentally-cut edges, or cast-iron modeled panels would be the line I should first try.

With these few suggestions I must leave the subject to you, only saying further that we must not be disheartened at any want of success, but remember that it took a long, long time, and much patient and loving work, to evolve beautiful and appropriate ornament in other materials.

What good gift have my brothers, but it came
From search and strife, and loving sacrifice.*

I once heard the following sentiment expressed by a speaker at a meeting of the Royal Institute of British Architects: "I suppose that if the question of iron were brought before the Institution of Civil Engineers, and anyone spoke of treating the material beautifully, it would raise a laugh;" more than once as I sat writing this paper this sentence rose before me like a grizzly specter shaking a finger of warning, but that my opinion does not coincide with that, is proved by the fact that I have dared to come before you to-night, and to speak of the possible connection of structural iron-work with true beauty. If I have suc-

* Edwin Arnold's *Light of Asia*.

ceeded in throwing out one suggestion that may set any of you in the way of giving to your works that appropriate ornamentation, which will add to them greater human interest and artistic vitality, I am more than satisfied.

ON THE REAL VALUE OF LUBRICANTS AND ON THE CORRECT METHOD OF COMPARING PRICES.

By ROBERT H. THURSTON, Mem. Am. Soc. C. E.

From Transactions of the American Society of Civil Engineers.

The real value of any lubricant is a quantity which seldom has any direct relation to its market price, and depends not only upon the intrinsic qualities of the unguent itself, but upon the economical conditions under which it is to be used. It is dependent to a greater extent upon the magnitude and cost of power than upon the expense of its purchase or preparation for use by the consumer. The correct method of comparing prices, from the user's standpoint, is not one involving merely a determination of the properties of the material as a reducer of friction, and the true value of the oil is not simply proportional to its endurance and its power of reducing lost work; it includes a study of the method by which it reduces the total expenses of lessening friction, and the extent to which total expense for power is reduced by such reduction of work wasted by friction. The usual systems of comparison are entirely wrong, and are only justifiable by the fact that hitherto it has been impracticable to obtain the data required for the establishment of a correct method. This difficulty no longer exists, and every intelligent purchaser of lubricants is coming to see that he may often effect enormous economies by the careful study of the variation of the total cost of lubricant and of waste power.

The total cost of the lost work in machinery includes two distinct items—the cost of lubricant, and the cost of doing the work of overcoming friction of the lubricated surfaces. Of these, the latter is usually enormously the greater, and it is at once seen that a saving in cost of lubricant is of slight importance in comparison with a saving of equal proportion in the reduction of the cost of the power demanded to overcome friction, and which is thus wasted. A dollar expended in the substitution of good oil for one of lower grade may save a hundred by the reduction of the waste of fuel and other expenses of power production. Such expenses include fuel, salaries, interest on capital invested in motive power, taxes and insurance on the driving machinery, boilers and building, and other and minor costs, which every proprietor can readily estimate with fairly accurate figures, if not with perfect satisfaction to himself.* The total cost of steam power thus foots up to about $100 per horse-power per annum in New York City, and to a minimum of, perhaps, $50 under more favorable conditions. Water-power often costs considerably less, although the cost of dams, reservoirs and machinery is large.

If, in any case, we call the total expense per hour K, the cost of the lubricant on the journal k, the quantity used q, the total cost of power per horse-power per hour k', and the amount of power used in overcoming friction of lubricated surfaces U, the total expense chargeable to "lost work" will be

$$K = kq + k'U \qquad (1)$$

The work done in overcoming friction U is proportional to the mean pressure on the lubricated surfaces P, to the speed of relative motion of rubbing surfaces V, to the time taken for comparison t, and to the magnitude of the coefficient of friction f. Thus we may write

$$K = kq + bf \qquad (2)$$

in which b is a constant, the value of which, k' P V t, is easily ascertained in any given case, and the calculation of the cost of friction is then readily made.

* "Friction and Lubrication," R. H. Thurston: New York, 1879; § 75, p. 200. "Friction and Lost Work in Machinery and Millwork," R. H. Thurston: New York 1885; Chapter VIII.

Where two oils are to be compared, to determine the economy to be secured by the substitution of the one for the other, the values of q and of f, and the cost per gallon, k, of each will be known, and the two values of K thus obtained will exhibit the relative economy of their use. If K is the same for the two, it is a matter of indifference which is used; if K is greater in one case than in the other, that oil is the more economical which gives the lower value, even though it may cost more per gallon, and may require to be more freely used than the other. Thus, suppose, for the two cases we have

$$K_1=k_1q_1+bf_1\ ;\ K_2=k_2q_2+bf_2.$$

If these two values of K were equal, $K_1=K_2$, and the gain by purchasing of the second oil is just compensated by the loss due to increased demand for power to overcome the increased friction, and

$$k_1q_1-k_2q_2=b(f_1-f_2) \tag{3}$$

$$k_2=\frac{k_1q_1+b(f_1-f_2)}{q_2}. \tag{4}$$

Any price paid for the second oil, *delivered on the journal*, less than k_2 gives a profit; any greater price produces loss. This last equation is thus a criterion by which to determine what price, k_2, may be paid for any oil proposed to be substituted for the first oil, costing k_1q_1 per hour.

Where the same quantity is used of each, as may be the case frequently, $q_2=q_1$, and

$$k_2=\frac{b}{q_1}(f_1-f_2)+k_1 \tag{5}$$

The question sometimes arises whether it is better to use a larger quantity of a certain oil already in use; in this case $k_2=k_1$, and the quantity allowable without loss is

$$q_2=\frac{b}{k_1}(f_a-f_2)+q_1 \tag{6}$$

Where the relative endurance, and the relative values of the coefficient of friction are determined by experiments made under the conditions of proposed use, if e and h represent the two ratios, since the quantity used will be inversely as the endurance, and the power wasted will be directly as the coefficients of friction,

$$k_2=ek_1+bef_1\frac{1-h}{q_1}; \tag{7}$$

and this expression becomes the criterion of values.

Instead of taking the time as one hour, and the unit of power as the horse-power, it may be convenient to adopt other units. Thus, on railroads the costs are measured by the cost of oil and of power per train mile, and

$$K=kq+df \tag{8}$$

in which q is the quantity of oil used per mile, and df is the cost of power for the same distance. Also, as a criterion,

$$k_1q_1-k_2q_2=d(f_2-f_1)\ ;\ k^2=\frac{k_1q_1+d(f_1-f_2)}{q_2} \tag{9}$$

In illustration of the application of these principles, take the following cases:

(1.) The proprietor of a large machine shop informs me that he finds the total expense of power to be nearly \$100 per horse-power per annum, of which power one-half is estimated to be expended in doing work wasted in friction; that he uses 0.02 gallon per hour of good lubricants, costing an average of \$0.50 per gallon. The mean coefficient of friction is judged to be about 0.05. The value of b (eq. 2) is found to be 0.6 horse-power, or 30 for 50 horse-power; then

$$K_1=0.01+1.50=\$1.51.$$

Supposing it be proposed to substitute for the oil in use one which costs but \$0.25 per gallon, and of which 0.03 is required per hour, and that the coefficient of friction with the cheaper oil is $f_2=0.06$, then

$$K_2=0.0075+1.80=\$1.80\tfrac{3}{4},$$

and a gain of one-quarter of a cent per hour, or \$7.50 per year, is effected at the expense of a loss in cost of power of 30 cents an hour, or \$900 per year, and a net loss of \$892.50.

(2.) A cotton mill, using 200 horse-power, in work of overcoming friction of lubricated surfaces, uses 0.7 gallon of oil per hour, at \$0.70 per gallon; it is proposed to substitute an oil costing \$0.40, and of which one gallon per hour will be required to do the work, while the coefficient of friction will rise from an average of 0.10 to 0.12. Taking b at 60, as before:

$$K_1=0.49+12.00=\$12.49\ ;$$

$$K_2=0.40+14.40=\$14.80.$$

A gain in expense for oil amounting 9 cents per hour, or $270 per year, produces a loss in cost of power of $2.40 per hour, or $7,200 per year, assuming 3,000 working hours per annum. The net loss is $6,930, *i.e.*, nearly 30 times the profit on the oil account. This is not an unusual or an extraordinary case, as matters are now going on in the business.

(3.) A railroad train requires 1 cent's worth of oil per mile, and costs 10 cents per mile for power expended in friction, using a good oil, costing 50 cents per gallon, at the rate of 0.02 gallon per mile, with a mean coefficient of friction of 1 per cent. It is proposed to change, using an oil costing but 25 cents, at the rate of 0.03 gallon per mile, and obtaining a coefficient of $f_2 = 0.015$; then

$$df_1 = 0.10;\ d = 10 \text{ (eq. 8)};$$
$$K_1 = 0.01 + 0.10 = \$0.11;$$
$$K_2 = 0.0075 + 0.15 = \$0.15.$$

In this case a gain of one-quarter of a cent per train-mile in cost of oil brings about a loss of 4 cents—sixteen times as much—in increased train resistance.

Using the equations given as criteria of values (eq. 4, 6, 9), we find the estimated value of k_2 to be, in the three cases given, respectively: −$19, −$2, and +16⅔ cents, nearly, for the cases as taken. That is to say, the proprietor of the machine shop will lose $19, nearly, on every gallon of the proposed oil that he may use; the owners of the cotton mill will lose about $2 on every gallon of the inferior oil that they may purchase; while the railroad will lose money, unless it can get the second oil for 16⅔ cents.

But suppose, in further illustration, that it is found possible, by increasing the supply of oil in the case of the machine shop, to reduce the mean coefficient of friction to 0.02, by using four times as much of the cheaper oil as was at first thought advisable. Applying our criterion to this case we get (eq. 4):

$$k_2 = 0.03 + 0.60 = \$0.63,$$

and a gain is effected of nearly two-thirds the original cost of lubrication. An expenditure of $60 gives a profit of about fifty times that amount.

It must not be assumed that these figures are more than rough approximations to fact; for it is difficult to obtain exact values of the quantities involved, and especially of the true mean value of f; but they are sufficiently correct to answer as illustrations of the principles involved, and are near enough to the truth to give a fair idea of the magnitude of the losses which are each day met in consequence of the practice of the system of false economy now generally practiced in the purchase of lubricants.

The values assumed for the coefficients of friction are probably fairly representative of those found in common practice. The experiments made by the writer show that, under ordinary conditions of everyday practice, the value for mechanism working under as light pressures as are met with in spinning frames, for example, different oils will give values from 0.10 to 0.25; under the usual pressures of heavy mill shafting, the figures range from 0 5 to 0.10; with pressures of greater intensity, such as are met in the steam engine and under railroad axle bearings, it often varies, using different lubricants, from about 0.01 up to 0.025, the first value being given by the best oils and the second by heavy greases. Under the exceptionally high pressures and at the speed of rubbing reached on the crank-pins of some steam engine (500 to 1,000 pounds per square inch, 35 to 70 kgs. per sq. cm.), f may fall to one-half the last given values. In endurance, the same variations are met with. The endurance decreases as pressures increase, and is twice as great with the best oils as with others of good reputation. The market prices of oils have no relation to these relations of quality. The best oils for any given purpose may be either more costly or cheaper than others less well fitted for the work. In some cases prices are made in the most arbitrary manner.* Sperm, lard, olive, and some few standard grades of mineral oils probably have fair and well-settled values; as a rule, however, the price of a mineral or of a mixed oil is no guide to selection.

Should time permit, and statistics prove to be attainable, the writer will endeavor to develop this subject more completely.

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* The writer has been informed of one case in which the dealer purchased an oil for 12¼ cents per gallon, gave it a trade name and sold it, unchanged, at $1.25. It was worth that amount, however, if compared with other oils in the market that may have cost the "maker" much more.

A STANDARD METHOD OF STEAM-BOILER TRIALS.

Report of Committee to the American Society of Mechanical Engineers.

From Advanced Copy of Transactions of the American Society of Mechanical Engineers.

II.

Appendix to Code.

I. OBJECT OF THE TEST.

In preparing for and conducting trials of steam boilers, the specific object of the proposed trial should be clearly defined and steadily kept in view.

1. If it be to determine the efficiency of a given style of boiler or of boiler setting under normal conditions, the boiler, brick-work, grates, dampers, flues, pipes, in short, the whole apparatus, should be carefully examined and accurately described, and any variation from a normal condition should be remedied if possible, and if irremediable, clearly described and pointed out.

2. If it be to ascertain the condition of a given boiler or set of boilers with a view to the improvement of whatever may be faulty, the conditions actually existing should be accurately observed and clearly described.

3. If the object be to determine the relative value of two or more kinds of coal, or the actual value of any kind, exact equality of conditions should be maintained if possible, or where that is not practicable, all variations should be duly allowed for.

4. Only one variable should be allowed to enter into the problem; or, since the entire exclusion of disturbing variations cannot usually be effected, they should be kept as closely as possible within narrow limits, and allowed for with all possible accuracy. J. C. H.

II. GENERAL OBSERVATIONS.

All observations are to be made by the expert, either personally or by his assistants. No statement of any kind is to be received from the owner or persons in charge of the boiler. All possibility of anything that would falsify the results must be closely guarded against; all pipes not used must be taken away or blank flanges inserted.

The two great points that are to be determined in every test of a steam-boiler, whatever the special and precise purpose of such test may be, are, the pounds of fuel burned, and the pounds of water evaporated.

To arrive at these we need to know, first, the pounds of fuel put into the furnace, and second, the pounds of water fed into the boiler.

To ascertain these facts with certainty is the fundamental requisite in all cases. The possibility of an error in either of these respects throws doubt upon all the results or indications of the test. The coal supplied to the furnace and the water fed to the boiler should, therefore, each be ascertained in a manner that proves its own correctness and excludes doubt.

All tests of this nature are properly regarded with suspicion. I often myself read of tests and results that I put no faith in, and the same must be true of every one who is experienced in this matter. I am therefore strenuous on this point, that a system of firing and a system of measuring the feed water should be employed that will prove the correctness of the record, and if errors are made, will clearly expose them.

If possible the steam generated should be condensed by passing it through a surface condenser, where it is cooled by a strong current of water in a closed chamber. By this means the number of thermal units added may be ascertained with precision.

A boiler test cannot be conducted properly when it is complicated by being combined with an engine test. C. T. P.

III. PRECAUTIONS TO BE OBSERVED IN MAKING A BOILER TEST.

It should be steadily kept in mind that the principal observations to be made are the quantities of coal consumed and of water evaporated. If these quantities are ascertained accurately, and the conditions made the same at the beginning and end of the test, the most important

requisites of a boiler trial will be secured. Other observations have their value both for scientific and practical purposes, but are in most cases subsidiary.

Boiler tests are often undertaken with insufficient apparatus and assistance. It is possible for a single person to test one boiler or even several in a battery, but it requires a great deal of labor to do so, and in many cases such person would be so fatigued as to be liable to make a simple error, vitiating the results. He would moreover at no time be able to give proper oversight to the test, so as to prevent accidental or unauthorized interferences. It is very desirable, in fact almost indispensable, that an assistant be detailed to weigh the coal, and another to weigh or measure the water; if calorimeter tests are to be undertaken still another assistant should be provided. The engineer in charge is then left free to oversee the work of all, and relieve either temporarily when necessary. Engineers are frequently called upon to make boiler trials in connection with parties whose interests are antagonistic to a fair test, and frequently voluntary assistance of busy-bodies is likely to produce errors in the results. It is therefore essential to have trustworthy assistants, and those of sufficient caliber not to be confused by interested parties, who will frequently endeavor in the most plausible manner to make out that a certain measure of coal has been already tallied, or that a certain tank of water has not been tallied.

In the first engine trials at the American Institute Exhibition (1869), in the Centennial boiler trials (1876), and since in private trials respecting performance of boilers as between the contractor and purchaser, the writer has arranged for both interests to take the data at the same moment, with instructions, if agreement could not be had, that the difference be at once referred to him.

In weighing the coal, the barrow or vessel used should be balanced on a scale and then filled to a certain definite weight. The laborer will soon learn to fill a vessel to the same weight within a few pounds by counting the number of shovels thrown in, when the change of a lump or two, to or from a small box alongside the scale will balance it.

The water may be measured in one tank by filling it to one mark and pumping down to another, but this involves stopping the pump when filling the tank, thereby failing to maintain uniformity of conditions. Two tanks arranged so that each can be filled and emptied alternately are much better. A still better plan is to have a settling tank to pump from and a measuring tank which is emptied into it, and this plan is improved by setting the measuring tank on a scale, and actually weighing the water. For large operations three tanks are necessary: a lower tank to pump from and two measuring tanks, one of which is filling while the other is being emptied. The writer has made several double measuring tanks with a horizontal section like the figure "8," there being a partition between the two tanks lower than the rim of the tanks. Water is conducted at will in either of the two tanks by a pipe swinging over the partition. One tank is allowed to fill until the water in it overflows into the other (which has been emptied and the cock shut), when the filling pipe is shifted into the empty tank, and as soon as the water level subsides in the full one, the water in that tank is allowed to flow out, the cock shut before the other tank is filled, and the operation repeated.

A simple tally should never be trusted. Nothing seems more reliable to an inexperienced observer than to mark 1, 2, 3, 4, with a diagonal cross mark for 5; but when there are waits of several minutes, between the marks, and several operations performed after a tally is made, there will be confusion in the mind whether or not the tally has been actually made. The tallies both of weights of coal and of tanks of water, should be written on separate lines, the time noted opposite each, and the records always made at the beginning or termination of some particular operation; for instance, in weighing coal at the time only when the barrel or bucket is dumped on the fire-room floor. It is desirable to have a number of coincident records of coal and water throughout the trial, so that in case of accident it may be held to have ended at one of such times. The uniformity of the operations may also be tested in this way from time to time. For this reason it will be found convenient to fire from a wheelbarrow set on a scale

and to have a float or water-gauge connected with the tank from which the water is pumped; by which means the coal and water used may, in an evident way, be ascertained for any desired interval.

As to calorimeter tests, note from the special article on that subject (Appendix XVII.) that the results are liable to be untrustworthy simply from an improper connection to the boiler. Scales and thermometers very finely graduated are desirable, but if they cannot be procured, good instruments with medium graduations carefully standardized may be employed, when if the observer will take the precaution mentioned in the appended article of the writer on calorimeter experiments, and simply make each record according to his best judgment at the time, the average of the results will be substantially accurate, although the several experiments may disagree somewhat with each other. C. E. E.

IV. WEIGHING THE COAL.

Where practicable, a box consisting of sides, back and bottom, capable of holding 500 pounds of coal for each boiler having twenty-five square feet fire-grate area, and in proportion for larger grates, should be placed on scales conveniently located for shoveling from it upon the fire grate.

The exact time of weighing each charge of say 500 pounds, should be noted, and the net weight, whatever it be, set down. [The box should be balanced by a fixed counterpoise, so that the readings of the scale beam may be net pounds of coal.]

On the instant of closing the fire door after each firing, the weight should be taken and the exact time noted as well as the weight. The box should be completely emptied each time, and the accuracy of the counterpoise observed, and, if necessary, adjusted. The differences of weight at each firing will give the several quantities fired; the differences of time will give the intervals in minutes and seconds between successive firings; and the differences of time between the successive charges—500 pounds, more or less—on the scales, will afford a check on the record of the firing. A chart or diagram should be plotted from the figures, which will clearly show the degree of regularity with which firing has been carried on, and reveal any omission or error. J. C. H.

V. WEIGHING THE COAL.

I would recommend that on a test no coal be brought into the furnace room except as follows:

A barrow to be employed, and be loaded each time at the coal pile with an equal amount, say 600 lbs. of coal, weighed on platform scales at the pile. The time when it is thus wheeled into the furnace-room to be noted. The barrow to be wheeled upon another platform scale before the furnace for the following purpose:

In separate columns, the times of charging the furnace to be noted, and the reading of the scales after each charging. The coal to be shoveled from the barrow directly into the furnace.

Now, here the log would show at once, by the great inequality of the intervals, if a barrow load of coal had been added or omitted, and the weights charged on the fire would check the barrow loads, and should also show the rate of firing.

No other coal being convenient to the furnace, reasonable watching will give assurance that none is surreptitiously added to the fire. C. T. P.

VI. WEIGHING THE WATER.

The best way is to have two tanks capable of holding 1,200 to 1,800 pounds, say 20 to 30 cubic feet, or two weighing tanks and one feeding tank, 144 to 216 gallons, each placed on a pair of scales, to be filled and emptied alternately. To avoid suspicion of leakage of stop-cocks, it is better to draw out the water by a flexible pipe or suction hose put alternately into the two tanks. The time of each weighing of each tank, to be designated as tank No. 1 and tank No. 2, should be accurately noted, and a method of checking the weighings by a diagram or chart as in respect to the coal, should be adopted. J. C. H.

VII. MEASURING THE FEED-WATER.

I would recommend that on all tests of any magnitude the water be fed to the boilers from a single tank of known capacity. That the tank be always filled so as to overflow, while the feed pump is

stopped, and also the communication to it is closed.

That the inlet pipe shall terminate above the tank so that its orifice is always visible. That after the supply has been shut off, and the overflow has ceased, the communication to the feed pump be opened and the pump be started. That the water be drawn down to a point that is determined by a line on a graduated rod attached to a float that has been well painted so as not to absorb the water; and that then the pump be stopped, communication with it be closed, and the tank be refilled.

The time of starting the pump each time to be carefully noted.

The regularity of the intervals would leave no room for doubt as to the number of tanks that had been emptied. The watch of opposite interests would insure the accuracy of the line at which the pump is stopped each time, and at which the test was closed. C. T. P.

VIII. KEEPING TIME OF OBSERVATIONS.

All time-keepers should be set at the start, and compared at the close; a gong should be used to give a signal for all observations designed to be synchronous and isochronous, in order that such observations may be conveniently arranged.
J. C. H.

IX. RECORDING STEAM GAUGE.

A good recording steam gauge, Edson's or other, carefully adjusted, should be used and accurately compared with the steam gauge at stated intervals. Such an automatic record, nicely integrated, is a good check on the record of the steam gauges. J. C. H.

X. AIR THERMOMETERS.

The air thermometer is the best instrument for taking the temperature of flues, smoke boxes, etc., from 300° to 700° or 750° F. These instruments cost but a trifle, $3 to $5, and can be made anywhere, by any competent expert, or by any one of his assistants under his directions, and can be relied on for ordinary temperatures, say 60° to 90°, up to any temperature which glass will bear without deformation. Ordinary machine-divided paper scales can be used with them. The great point is to deprive the interior of the bulb and tube of all moisture, and to fill the bulb and the upper half of the leg of the inverted siphon connected with the bulb, with dry air. (Appendix XI.). The expansion of dry air is practically uniform for all useful ranges of temperature, and its volume is directly proportioned to its temperature from absolute zero, say 461.2° F. below zero F., equal to 493.2° F. below the temperature of melting ice, to which the conventional zero of the air thermometer, at the accurately observed and noted temperature of the air when the mercury in the two legs of the inverted siphon is exactly level—the tubes being exactly vertical—can be conveniently referred. For instance, if the temperature of the air when the mercury in the two legs is level, be 73.8° F., add to this 461.2°, and we have 541.0° F. absolute, as the true absolute temperature corresponding to our zero. Double this temperature—1082° F. absolute (equal to 1082° − 461.2° = 620.8° F. above zero F.), would double the volume of the air; but the volume being nearly constant—since the capacity of the tube may generally be disregarded, a difference of level will be produced in the height of the mercury in the two legs of the inverted siphon exactly equal to the height of the mercury column in a mercurial barometer at the time. No correction for capillarity is required, since the negative capillarity is equal in the two legs. No correction for temperture is required, unless the temperature of the *mercury* in the air thermometer is higher than that of the mercury column of the barometer. If there is an observable difference, it must be corrected for, at the rate of 0.0001 per degree F.

There should be at least two of these air thermometers, three would be safer, in readiness for each test, to avoid disappointment by accident. The legs of inverted siphon must be vertical, but the tube from the upper end of the leg to the bulb may be straight, or bent to any angle.

For the determination of the heat of flue gases, this instrument is indispensable, up to the limit of the softening of glass; but since no flue will always, or even usually, contain volumes of gas of equal temperature throughout at the same instant, at least two tubes of gas pipe, welded up at the lower end, and

filled with mercury, should be placed in opposite sides of the flue, near the air thermometer, for observing the differences with chemical thermometers graduated on the glass. Sir Wm. Thomson highly commends thermometers incased in hermetically sealed glass tubes, with scales graduated on paper for use up to a point below the temperature required to scorch the paper. Dampness being excluded by the glass case, the paper scales are of unchangeable length, and the graduations and figures are very distinct and legible. J. C. H.

XI. DESCRIPTION OF AN AIR THERMOMETER OF CONSTANT VOLUME (AFTER REGNAULT) AND OF THE MODE OF CONSTRUCTING AND USING THE SAME.

This instrument may be made in many forms, and of materials of several kinds—metals, or glass, or metal and glass. A simple, inexpensive, and convenient form* consists of a U tube of about three-eighths of an inch external diameter, and about one-sixteenth of an inch caliber, or a little less; having a short leg about 39 inches long, and the other leg longer by 12 inches or more; the latter surmounted by a bulb blown out of the tube, $1\frac{5}{8}$ inches in diameter, $6\frac{5}{8}$ inches in extreme length, and 5 inches long in its straight, cylindrical portion.

The two legs, or branches, of the U, are two inches apart between centers.

They are separate tubes, each one bent to a right angle, by a curve of short radius, ground square and true at the ends which are to meet, and hermetically united by a short coupling of rubber tubing, firmly bound on each with wire.

In blowing the bulb, a small, short tube, about $\frac{1}{16}$ inch in caliber, and 2 or 3 inches long, is formed on top for use, making the instrument—to be sealed by fusion when it is done.

Having formed the U tube by uniting its branches, the next thing to be done is to dessicate its interior perfectly and to fill it with dry air. For this purpose it is put in any convenient position—reclining, probably—a piece of rubber tubing is secured to the small tube on top of the bulb and connected with a U tube about 6 inches long in its branches, and $\frac{5}{8}$ inch or $\frac{3}{4}$ inch in diameter, filled with dry lumps of chloride of calcium and surrounded by crushed ice, to lower its temperature, and the temperature of the air passing through it to about 32° F., at which point air parts with a larger portion of its moisture than at any higher temperature.

An aspirator is now connected by a piece of rubber tube, with the open end at the short branch of the instrument, and a stream of air is drawn in through the chloride of calcium tube and discharged by the aspirator.

A simple and efficient form of aspirator is merely a piece of $\frac{1}{4}$-inch gas-pipe, bent, when hot, into three or four sharp zig-zags, with an inlet at its upper extremity for water, and at its side for air. A stream of water flowing through the zig-zag tube draws air in at the side orifice, and the air becoming entangled with the water, flows off with it—its place being supplied through the chloride of calcium tube and the U tube of the thermometer. This operation can be carried out conveniently in any office or other room supplied with a flow of water and a set wash-basin; and once arranged, requires only so much attention as to see that it remains undisturbed.

When the tube is completely dessicated (in so far as air at 32° F. will give up its moisture to chloride of calcium), which will be in about four or five hours, shut off the water, bend over the rubber tube connecting the calcium-chloride tube with the bulb, or, another rubber tube connected with the outer branch of the calcium-chloride tube, so as to prevent any mixture of moist air with the dry air in the bulb and U tube, and lay the instrument on its side in such a manner that the longer branch shall slope from the bulb down to the rubber coupling, and that the shorter branch shall also slope from the rubber coupling down to the extremity of this branch, which should be kept closed by the finger until it is immersed in mercury to prevent the admixture of moist air. If the mercury is in a wide, shallow dish, like a plate or a saucer, the sloping end of the branch may be immersed in it sufficiently; or a short piece of glass tube coupled on *in advance*, when preparations were made for the desiccating

* Constructed and brought to my notice by Fred. W. Prentiss. Originally devised by Regnault.—J.C.H.

process, may be held down in the mercury. Then apply the lips to the calcium-chloride tube or to the rubber tube connected therewith, and, by inspiration of breath, draw out air from the bulb until the mercury, forced into the shorter branch, fills it, and shows just beyond the rubber coupling in the lower end of the long branch. Then pinch the rubber tube, set the instrument upright (keeping the open end of the shorter branch closed with the finger until it is upright). See that the branch tubes are exactly vertical; carefully relieve the pinched rubber tube, so that air may escape, until the surface of the mercury in the two branches is exactly level; then pinch the rubber tube and fuse and seal the small glass tube into a little button on top of the bulb. Now hang up an accurate chemical thermometer, graduated on the tube, close beside the bulb, until this thermometer and the bulb and the dry air inside of it are certain to have come to a common temperature, and read and note this temperature; and make a distinct and permanent mark on the back-board of the instrument, at the level of the mercury in the two branches. This back-board may be 4 or 5 inches wide, ½ to ⅝ inch thick, and about as long as the shorter branch, and the tube may be secured to it by little staples of annealed iron wires, going around (*i. e.* over) the tube and through holes in the back-board; and twisted together at the back. A bit of soft leather at the staple, between the board and the tube, will form a secure bed for the tube, and obviate danger of breaking. Such staples are indicated on the drawing, at *c*, *c*, *c*. There may be two such staples at the bottom, passing over the rubber coupling, to further aid in keeping the two parts (branches) of the U tube in proper position. At the same time that the temperature is noted, note also the height of the mercury column of a barometer. On the air thermometer, hanging in my office, the notes are:

"Temperature 81.5° F.

"Barometer, Hg, 31.03 in."

Attached scales complete the instrument. For these, engine-divided paper scales will answer; and they may be graduated to inches and tenths of inches, as I have indicated, or to millimeters.

Since the instrument is to be used chiefly or wholly for temperatures above any atmospheric temperature at which it may be set, the scale on the long leg need not exceed much above, nor that on the short leg much below, the level line; but a ½ inch, or an inch may be worth while, if the instrument is set as high as 80° F., for convenience of comparison with ordinary thermometers.

FOR USING THE INSTRUMENT:

Let t_1 = temperature at which thermometer is set.

t_2 = temperature sought from observation.

$\pm h$ = difference of level of mercury, when the thermometer was set; + when mercury is highest in short leg; — when mercury is highest in long leg; $h = o$, when mercury is level at the noted temperature when the thermometer is set.

Let b_1 = mercury column of barometer when the thermometer was set.

b_2 = mercury column of barometer at the time of observation.

$\pm h_2$ = difference of level of mercury in the two branches when observed:

Then,

$$t_2 = \left\{ (461.2 + t^1) \frac{(b_2 \pm h_2)}{(b_1 \pm h_1)} \right\} - 461.2$$

If made of good hard glass, this instrument is safe at 800° F., say 426⅔° C., and will not be very likely to fail at under 850° F., say 454⅔° C.

The part of the tube below the bulb BI, may be of any convenient length, and may be bent as at F, to any angle to suit requirements of location.

J. C. H.

XII. PYROMETER.

So far as known to me the only way to measure temperatures between 600° or 700° F., or above the range of the air thermometer, and 2500° or 2700° F., or up to the melting point of commercial platinum, is by the platinum water pyrometer.

One form of this pyrometer is described in the journal of the Franklin Institute, Vol. 84, pp. 169 and 252, September and October, 1882. J. C. H.

XIII. PYROMETER.

The temperature of the escaping gases should be ascertained, not by pyrometers, but by means of certified mercury thermometers introduced at a number of different points in the same plane transverse to the flue. The velocity of the current should be ascertained at each of these points. The distance of the transverse plane of observation from the boiler should be noted. C. T. P.

XIV. DRAFT GAUGE.

Some instruments for indicating the force of chimney draft:

a. A bent glass tube filled with water.
b. A bent tube with two fluids.
c. An incased aneroid.
d. A differential pressure gauge.

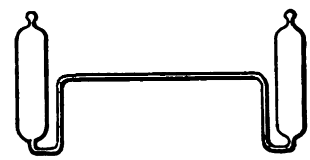
The incased aneroid, having inches of mercury indicated by spaces of about two inches, divided to $\frac{1}{100}$, answers well. The case is air-tight, and by means of a three-way cock the interior of the case may be put alternately in communication with the external air and with any flue into which a suitable pipe is inserted.

The differential pressure gauge was devised and put to use at the Massachusetts Institute of Technology, and similar instruments should be manufactured for sale. I will not attempt to describe it further than to say that a column of water in a glass tube, acting on a small diaphragm, balances the weight of the movable parts when a large diaphragm is in equilibrium of pressure. Now if this large diaphragm have chimney pressure on the inner side, and atmospheric pressure on the outside, the difference of pressure will be shown by a rise of water in the glass tube to a height proportioned to the ratio of the areas of the small and large diaphragm.

Draft should be measured in different parts of the flue, in order to detect infiltration of air through cracks in the brick-work and through the brick-work itself. J. C. H.

XV. DRAFT GAUGE.

Mr. C. P. Higgins, of Philadelphia, has recently made the draft gauge shown in the sketch. The gauge is filled with water above the level of the horizontal tube, so as to leave a bubble of air about half an inch long near one end of the horizontal tube when the water is level in the side tubes. The inside diameter,

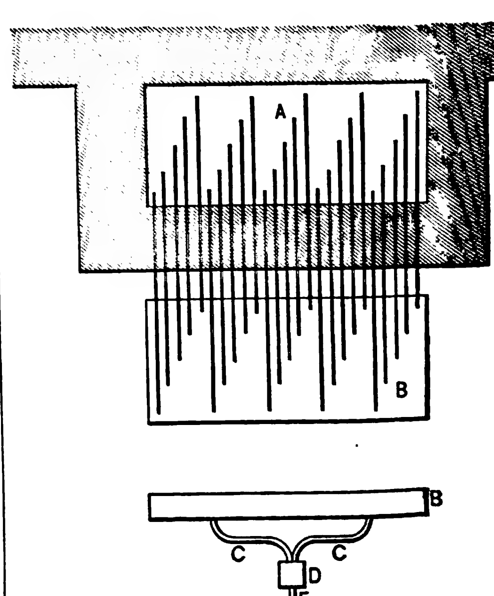


of the two vertical tubes being the same, say half an inch, and the diameter of the horizontal tube $\frac{1}{8}$ of an inch, a draft equal to one inch of water, or which will cause the difference in the level of the two tubes to be one inch, will cause the bubble to move eight inches in the horizontal tube.

The readings of the ordinary U tube draft gauge are thus multiplied by 8, with the additional advantage that the position of the air bubble can be read more accurately than the difference of level in the ordinary gauge. The scale applied to the horizontal tube requires to be standardized for the ratio of areas of the small and large tubes and for irregularities in the caliber of tubes. W. K.

XVI. SAMPLING FLUE GASES.

Very great diversities in the composition of flue gases often exist in the same



flue at the same time. To obtain a fair sample, it has been found sufficient to have one orifice to draw off gases through for each 25 sq. inches of flue cross section. The pipes must be of equal diameter and of equal length. $\frac{1}{4}$ in. gas pipes, all alike at the ends, and of equal lengths, answer well. Similar steel tubes will be still better.* These should be secured in a box or block of galvanized sheet iron, equal in thickness to one course of brick, in such a manner that the open ends may be evenly distributed over the area of the flue, and their other open ends inclosed in the receiver B. If the flue gases be drawn off from the receiver B by four tubes, C C, into a mixing box, D, beneath, about 3 inch cube, a good mixture can be obtained. Two such "samplers," one above the other a foot apart, in the same flue, will furnish samples of gases which show by analysis the same composition. J. C. H.

XVII. CALORIMETER EXPERIMENTS.

In all boiler experiments it is important to ascertain the quality of the steam, *i. e.*, 1st, whether the steam is "saturated" or contains the quantity of heat due to the pressure according to standard experiments; 2d, whether the quantity of heat is deficient, so that the steam is wet; and 3d, whether the heat is in excess and the steam superheated. The best method of ascertaining the quality of the steam is undoubtedly that employed by a committee which tested the boilers at the American Institute Exhibition of 1871–2, of which Professor Thurston was chairman; but this plan cannot always be adopted. When all the steam generated is not condensed, the method of making the connection for the purpose of taking out a sample is of the utmost importance. Unless great care be exercised, the results will frequently show that the steam is superheated when the boiler has no superheating surface. The cause of this is pointed out at p. 82 of the writer's general report on the exhibits referred to the Judges of Group XX., Centennial Exhibition. It is not fair to take the steam direct from the boiler, for if there be no steam circulation at that point the steam will of course show dry. The samples should be taken from the main steam pipe, but not from the bottom, as this would take all the water draining to that point. The method of taking it through a perforated pipe crossing the main steam pipe is sure to cause difficulty whenever the velocity of steam flowing to the calorimeter is sufficient to reduce the pressure in the supply pipe, for in such case the temperature of the steam in that pipe falls at the inlet, and the steam of full pressure and higher temperature flowing through the main pipe adds heat to that flowing into the calorimeter pipe, so that the latter, when referred to the pressure from which it is derived, shows superheating. The same effect takes place in a less degree when the steam for the calorimeter is taken through a lateral opening of small diameter, the metal surrounding the opening being kept warm by the current passing through the main pipe, and imparting its heat to the steam flowing in the lateral pipe to the calorimeter. To avoid this difficulty, the writer recommends making the lateral opening leading to the calorimeter $1\frac{1}{2}$ to 2 inches in diameter, and then at a little distance from the main pipe, say 1 foot, reducing the supply pipe to calorimeter to $\frac{3}{4}$ or $\frac{1}{2}$ inch diameter.

For general use the writer prefers the ordinary barrel calorimeter, which has the advantage over a continuous calorimeter operating at a slow rate of flow, that with the latter the condensation in the connecting pipes may cause the small quantity of steam flowing to the calorimeter to be moist, and thereby vitiate the results. With the barrel calorimeter it is desirable to heat the water promptly, so that the question of condensation in connecting pipes is of minor importance. At the same time the quantity of steam drawn off should not be so great, in connection with that passing to other points, as to cause the boiler to foam, or to reduce the pressure.

The practice of the writer is to use a barrel, holding preferably 400 lbs. of water, which is set upon a platform scale, and provided with a cock or valve for allowing the water to flow to waste. I have always provided a small propeller made with blades simply cut out of a disc of sheet iron, twisted to give the pitch and bolted on to the bottom of a vertical rod supported in a wooden step in the

* Because smoother and more uniform.

bottom of the barrel, and passing through a cross piece on the top of the barrel. The rod terminates at the top in a crank, and a collar is placed on the vertical shaft under the upper support. A fixed thermometer is run through a cork in the bunghole of the barrel. The pipe conducting the steam from the main steam pipe is made of graduated sizes, as previously referred to, and the smaller pipe provided near the calorimeter with a valve connected by means of a coupling with a rubber hose. In the coupling is to be placed a disc of metal, provided with a regulating hole of from $\frac{3}{16}$ to $\frac{1}{4}$ in. in diameter.

To operate the calorimeter the barrel is filled with water, the weight and temperature ascertained, steam blown through the hose outside the barrel until the pipe is thoroughly warmed, when the hose is suddenly thrust in the water, and the propeller operated until the temperature of the water is increased to the desired point, say about 110° usually. The hose is then withdrawn quickly, the temperature noted, and the weight again taken. The object of the particular details adopted will be readily understood. The simple propeller insures a uniform heating of the whole of the water. The little disc in the supply pipe enables the stop valve in pipe from boiler to be opened wide without drawing off so large a quantity of steam as to lower the pressure or produce priming. To avoid the jar when the steam hose is in the water, it is better to cut some lateral holes in the hose near its lower end. In this way a circulation is induced through the holes which prevents most of the jar and noise.

The weight of water in calorimeter should be increased proportionally to the weight and specific heat of all metal exposed to changes of temperature with the water. An addition of one-ninth of the weight of the propeller and submerged portion of shaft and fastenings will be substantially correct if the apparatus be made of iron.

The importance of errors of measurement or observation are inversely proportional to the magnitudes of the quantities. The weight of water added by condensation of steam being comparatively small, it must be weighed accurately, say within a quarter of one per cent. The writer has done this on an ordinary platform-scale in good order by using a second movable poise, in addition to the customary one, and of one-tenth its weight. In weighing, the lighter poise is adjusted to bring the free end of the beam to a fixed mark. The same result may be obtained by loading the platform with small known weights to bring the lever to a fixed point each time, and deducting such weights from the reading of scale in regular notches.

The above must be considered a makeshift, but a valuable one. When possible, delicate scales should be employed, and, in the opinion of the writer, better satisfaction can be obtained in this direction than by the use of the more complicated apparatus required to weigh the water of condensation separate.

In making the calculations the following notation and formula prepared by the writer for the report of the Committee having in charge the testing of the boilers of the Centennial Exhibition will be found convenient:

Let W=original weight of water in calorimeter.

Let w=weight of water added by heating with steam.

Let T=total heat in water due to the temperature of steam at observed pressure.

Let H=total heat of steam at observed pressure.

Let l=latent heat of steam at observed pressure = $(H-T)$.

Let t=total heat of water corresponding to initial temperature of water in calorimeter.

Let t'=total heat in water corresponding to final temperature of water in calorimeter.

Let Q=quality of steam.

Then

$$(1)\qquad Q=\frac{1}{l}\left(\frac{W}{w}(t'-t)-(T-t')\right).$$

Then when $Q<1$, percentage of moisture in steam $=100\ (1-Q)$.

When $Q<1$, number of degrees steam is superheated $=2.0833\ l\ (Q-1)$.

The later practice of the writer when there are a large number of calculations to be made is as follows:

Add to above notation the following:

Let m = percentage of moisture in steam.

Let s = number of degrees steam is superheated.

Let A = number of heat units lacking per pound of steam condensed. Equals quantity in parenthesis, Equation (2).

Let Σ = sign of summation. To be read: Some of values of—

Let n = number of experiments to be averaged.

Then

$$(2)\qquad m=\frac{1}{l}\Big(\overbrace{(H-t')-\frac{W}{w}(t'-t)}^{A}\Big).$$

$$(3)\qquad Q=1-m.$$

When A or m is minus.

$$(4)\qquad s=-2.0833\ A.$$

Averaging several experiments.

$$(5)\qquad m=\frac{\Sigma A}{nl}$$

$$(6)\qquad s=-2.0833\frac{\Sigma A}{n}.$$

C. E. F.

XVIII. NOTE ON USE OF THE BARREL CALORIMETER.

In the use of the barrel calorimeter not less than 300 lbs. of water should be used, and it is an advantage, when practicable, to cool the water by means of pulverized ice. By vigorous agitation the water may thus be cooled to 36° F., or even 34° F., in a few minutes, when the remaining ice is to be completely removed. As the ice floats on the surface, this can be readily done. The weight of steam condensed can thus be often doubled, and still the temperature of the water not be raised above 100° F., at which point no sensible loss of heat will be suffered through evaporation. The greater the weight of steam condensed, the less will be the unavoidable percentage of error.

If the barrel be covered with a non-conductor, it will be found that no sensible change in the temperature of the water will take place in a long time.

C. T. P.

XIX. EFFECT OF SMALL ERRORS OF OBSERVATION IN CALORIMETER TEST.

Suppose a case in which errors of observation occur, as in the following table:

	Observed Reading.	True Reading.	Amount of error.
Weight of condensing water, corrected for equivalent of apparatus, W	200.5 lbs.	200 lbs.	½ pound.
Weight of condensed steam, w	9.9 "	10.0 "	$\frac{1}{10}$ "
Pressure of steam by gauge, P	78. "	80 "	2 pounds.
Original temperature of condensing water, t	44.5° "	45° "	½ degree.
Final " " " t'	100.5° "	100° "	½ "

The formula for calculation is

$$Q=\frac{1}{H-T}\left(\frac{W}{w}(h-h_1)\right)-(T-h_1)$$

in which Q = quality of the steam, dry saturated steam being unity.

H = total heat of steam at observed pressure.

in which T = total heat of water at observed pressure.

h = total heat of condensing water, original.

h_1 = total heat of condensing water, final.

			Moisture per cent.	Error per cent.
Substituting in the formula the "true readings" in the table, we have for the value of		Q = 0.9874	= 1.26	= 1.
All readings true except	W = 200.5,	Q = .9906	= 0.94	= 0 32
" " "	w = 9.9,	Q = 1.0000	= 0.00	= 1.26
" " "	P = 78. ,	Q = .9880	= 1.20	= 0.06
" " "	t = 44.5,	Q = .9986	= 0.11	= 1.15
" " "	t' = 100.5,	Q = .9994	= 0.06	= 1.20
" " incorrect		Q = 1.0272	= (minus)	= 3.98

The last case, Q=1.0272, is equivalent to 50.2 degrees superheating.

The errors above noted are all such as may easily occur even with good apparatus. The condensing water being usually weighed in a barrel on an ordinary platform scale, an error of ½ a pound could easily be made if the scale were not carefully tested and standardized. To make as small an error as $\frac{1}{10}$ of a pound in the weight of the condensed steam, when it is weighed in the bulk with the condensing water, taking the difference of readings before and after the test, is almost more than can be expected. The probable error of such a method of weighing the condensed steam is usually more than a quarter of a pound. The error in this weight is the most important of all those given in the table, showing dry steam, Q=1.00, instead of 1.26 per cent. moisture, the true result. If the error of the weight of the condensed steam were ¼ lb., it would be equivalent to an error of 3 per cent. in the calculated moisture in the steam, and consequently of 3 per cent. in the total result of the boiler test. The error of steam pressure, 2 lbs., is well within the limit of error of many steam gauges, but as seen in the result, it is the least important of all the errors, giving 1.20 per cent. moisture instead of 1.26 per cent. The errors of ½ a degree in temperature of condensing water are also quite important, and show the necessity of having thermometers carefully standardized. The effect of an error of weighing the condensed steam is so serious, and it is so likely to occur, that in the writer's opinion the method of making tests with a barrel on a platform scale, without any special weighing of the condensed steam, is so inaccurate that it should be discouraged, or at least that the results obtained by it should be considered as having a probable error of 3 per cent. It is questionable whether averaging a large number of results so obtained will give any greater approach to truth, for the errors of weighing in a barrel on a coarse platform scale, of the condensed steam together with the condensing water, due to personal equation, to absorption and evaporation of water, to error of sliding or stationary poise, and to friction of scale are apt to be, comparatively, constant, and may by no means be expected to balance each other.

W. K.

XX. COIL CALORIMETER.

The following is a description of a calorimeter, which the writer has found to give fairly good results, but sufficient experiments have not yet been made with it to determine its limit of error:

A surface condenser is made of light weight copper tubing ¾″ in diameter and about 50′ in length, coiled into two coils, one inside of the other, the outer coil 14″ and the inner 10″ in diameter, both coils being 15″ high. The lower ends of the coils are connected by means of a brazed T-coupling to a shorter coil, about 5′ long, of 2″ copper tubing, which is placed at the bottom of the smaller coil and acts as a receiver to contain the condensed water. The larger coil is brazed to a ¾″ pipe, which passes upward alongside of the outer coil to just above the level of the top of the coil and ends in a globe valve, and a short elbow pipe which points outward from the coil. The upper ends of the two ¾″ coils are brazed together into a T, and connected thereby to a ¾″ vertical pipe provided with a globe valve, immediately above which is placed a three-way cock, and above that a brass union ground steam tight. The upper portion of the union is connected to the steam hose, which latter is thoroughly felted down to the union. The three-way cock has a piece of pipe a few inches long, attached to its middle outlet and pointing outward from the coil.

A water barrel, large enough to receive the coil and with some space to spare, is lined with a cylindrical vessel of galvanized iron. The space between the iron and the wood of the barrel is filled with hair felt. The iron lining is made to return over the edge of the barrel, and is nailed down to the outer edge so as to keep the felt always dry. The barrel is furnished also with a small propeller, the shaft of which runs inside of the inner coil when the latter is placed in the barrel. The barrel is hung on trunnions by a bail by which it may be raised for weighing on a steelyard supported on a tripod and lifting lever. The steelyard for weighing the barrel is graduated to

tenths of a pound, and a smaller steelyard is used for weighing the coil, which is graduated to hundredths of a pound.

In operation, the coil, thoroughly dry inside and out, is carefully weighed on the small steelyard. It is then placed in the barrel, which is filled with cold water up to the level of the top of the globe valves of the coil and just below the level of the three-way cock, the propeller being inserted and its handle connected. The barrel and its contents are carefully weighed on the large steelyard; the steam hose is connected by means of its union to the coil, and the three-way cock turned so as to let the steam flow through it into the outer air, by which means the hose is thoroughly heated; but no steam is allowed to go into the coil. the water in the barrel is now rapidly stirred in reverse directions by the propeller and its temperature taken. The three-way cock is then quickly turned, so as to stop the steam escaping into the air and to turn it into the coil; the thermometer is held in the barrel, and The water stirred until the thermometer indicates from five to ten degrees less than the maximum temperature desired. The globe valve leading to the coil is then rapidly and tightly closed, the three-way cock turned to let the steam in the hose escape into the air, and the steam entering the hose shut off. During this time the water is being stirred, and the observer carefully notes the thermometer until the maximum temperature is reached, which is recorded as the final temperature of the condensing water. The union is then disconnected and the barrel and coil weighed together on the large steelyard; the coil is then withdrawn from the barrel and hung up to dry thoroughly on the outside. When dry it is weighed on the small scales. If the temperature of the water in the barrel is raised to 110° or 120° the coil will dry to constant weight in a few minutes. After the weight is taken, both globe valves to the coil are opened, the steam hose connected, and all of the condensed water blown out of the coil and steam allowed to blow through the coil freely for a few seconds at full pressure. When the coil cools it may be weighed again, and is then ready for another test.

If both steelyards were perfectly accurate, and there were no losses by leakage or evaporation, the difference between the original and final weights of the barrel and contents should be exactly the same as the difference between the original and final weights of the coil. In practice this is rarely found to be the case, since there is a slight possible error in each weighing, which is larger in the weighing on the large steelyard. In making calculations the weights of the coil on the small steelyard should be used, the weights on the large steelyard being used merely as a check against large errors.

It is evident that this calorimeter may be used continuously, if desired, instead of intermittently. In this case a continuous flow of condensing water into and out of the barrel must be established, and the temperature of inflow and outflow and of the condensed steam read at short intervals of time. W. K.

XXII. REPORTING THE RESULT.

As to reporting the results of boiler tests—two things are necessary, in order to make the reports (*a*) generally intelligible, and (*b*) strictly comparable.

1. The number of pounds of water actually evaporated under stated (actual) conditions of feed-water temperature and steam-gauge pressure, into steam containing not over three per cent. of entrained water, by each pound of coal burned—coal of good mercantile quality, dry; water dried out of a sample and allowed for, or, containing not over one-half per cent. of surface moisture, by actual experiment of drying samples. In this latter case, the one-half per cent. of water in the coal, like the three per cent. of entrained water in the steam, and the stated quantity of ashes and refuse in the coal, are taken in for the sake of representing usual conditions. So much for *general intelligibility*.

2. The equivalent evaporation in pounds of water of $t=212°$ F. converted into dry saturated steam of one atmosphere pressure, = 760 mm = 29.92 in. mercury—with one pound of *dry* combustible consumed. This for *strict comparability*.

It is obvious, if attention be given to the subject—too often neglected—that all the surface water in the coal, if not ascertained and allowed for, will appear

as combustible and disappear as water evaporated.

For example, two per cent. of water in the coal, passing over the bridge wall and going up chimney, leaving no weight to represent it in the "ashes and residue," will increase the item of "combustible" by two per cent. of the gross weight of the coal; and if ashes and residue $=\frac{1}{6}$ of the gross weight, the addition will be 2 per cent $\times \frac{6}{5} = 2.4$ per cent. At the same time about two-ninths of one per cent. of the water evaporated will escape observation, going up chimney unnoticed.

There should also be introduced into general practice an equivalent statement, of

3. The equivalent evaporation in pounds of water from feed-water temperature = 100° F. into usual steam containing not over three per cent. entrained water of seventy pounds per square inch pressure by steam gauge, above 1 atmosphere = 760 mm. = 29.92 inches mercury—for each pound of commercial coal containing not over one-sixth ashes and residue including surface water; one pound of such commercial coal being capable of imparting to the water in a boiler of good proportions about 10,000 British thermal units.

XXIII. REPORTING THE TEST.

The report should include a complete description of the boiler, which, for special boilers, should be written out at length, but generally can conveniently be presented in tabular form, substantially as follows:

Type of boiler.
Diameter of shell.
Length of shell.
Number of tubes.
Diameter " "
Length " "
Diameter of steam drum.
Width of furnace.
Length of furnace.
Kind of grate bars.
Width of air spaces.
Ratio of area of grate to area of air spaces.
Area of chimney.
Height of chimney.
Length of flues connecting to chimney.
Area of flues connecting to chimney.

GOVERNING PROPORTIONS.

Grate surface.

Heating surface $\begin{cases} \text{Water.} \\ \text{Steam.} \\ \text{Total.} \end{cases}$

Area of draft through or between tubes.
Ratio grate to heating surface.
" draft area to grate.
" " " total heating surface.
Water space.
Steam space.
Ratio grate to water space.
" " " steam space.

C. E. E.

XXIV. OBSERVATION BLANKS.

The observations taken during a test should be recorded on a series of blanks prepared in advance so as to be adapted to the purposes of the trial. The number of sheets and the particular items on each may be varied to suit the number of observers and the work designated for each. The following are copies of observation blanks used in the Centennial trials with a few lines of figures inserted, without reference to each other, for the purposes of illustration. The columns should, of course, be of sufficient length to contain the number of observations expected. C. E. E.

XXV. HORSE-POWER.

The writer's preference for rating boilers in horse-power is:

Capacity to evaporate into dry steam, *i. e.*, not containing over three per cent. of entrained water, and the water actually entrained allowed for and deducted:

1. 34½ pounds of water from and at 212°, equal to

2. 30 pounds of water of $t=100°$ under $p=70$ pounds per square inch above one atmosphere; with easy firing, moderate draught, and ordinary fuel, implying good economy, and capability of fifty per cent. increase to meet emergencies.

As to the last condition, "capability of fifty per cent. increase to meet emergencies," it must be obvious that a boiler which, under most favorable conditions of fuel, draught, firing, and everything else, just capable of evaporating into dry steam 3,450 pounds of water from 212° into

Log of Trial of Boiler.

No. 1.—Record of Feed Water.

1	2	3	4	5	6	7	8
Time.	Tank A.			Tank B.			Height of Water in Glass.
	Initial Weight.	Final Weight.	Temper-ature.	Initial Weight.	Final Weight.	Temper-ature.	
Hrs. Min. A. M.	Lbs.	Lbs.	Deg. Fah.	Lbs.	Lbs.	Deg. Fah.	Ins.
5.22	—	—	—	—	—	—	7
6.19	1442.5	136	68	—	—	—	0
6.40	—	—	—	1421.5	169.5	68.5	6
7.05	1447	131.5	68	—	—	—	6
7.28	—	—	—	1431.5	193.5	67	7
8.00	1445.5	316	67	—	—	—	5

Deduct 56.25 pounds of feed-water for difference of level in boiler.

Log of Trial of Boiler.

No. 2.—General Observations—Coal and Ashes.

9	10	11	12	13	14	15	16	17	18	19	20	21
Time.	Steam Pressure.	Temperatures. (Fahrenheit.)					Coal and Ashes.				Barometer.	Height of Water in Glass.
		Air.	Fire-Room.	Steam.	Uptake. Ther.	Uptake. Pyr.	Coal Weighed out on Floor.	Coal Con-sumed.	Coal found in Ashes.	Weight of Ashes. *Net.*		
Hrs. Min. A. M.	Lbs.	Deg.	Deg.	Deg.	Deg.	Deg.	Lbs.	Lbs.	Lbs.	Lbs.	Ins.	Ins.
9.22	70	49	88	310	—	375	230.5	170	578	304.5	30.20	10
10.00	70	51	82	299	—	401	229.5	Wood.	—	—	—	7
10.30	70	57	83	308	—	446	229.5	235.5	—	—	—	3
11.00	70	—	80	309	—	446	229.5	229	—	—	—	0.5
11.30	70	55	80	299	—	432	223.5	221.5	—	—	—	10
12.00	70	—	80	298	—	444	231	227	—	—	—	8

Log of Trial of Boiler.

NO. 3.—RECORD OF CALORIMETER EXPERIMENTS.

22	23	24	25	26	27	28
Time.	Water.			Temperatures. (Fahrenheit.)		Steam Pressure.
	Weight of Barrel.	Initial Gross Weight.	Final Gross Weight.	Initial.	Final.	
Hrs. Min.	Lbs.	Lbs.	Lbs.	Deg.	Deg.	Lbs.
A. M. 5.35	80.5	400	412.5	73.5	106.125	70 –66
5.55	80.5	400	413.375	68.25	110.50	70 –67
6.15	80.5	400	411.375	72.50	111	70 –68.5
6.35	80.5	400	417.25	66	122	70 –68
6.55	80.5	400	415.125	67.5	114.25	70 –64
7.15	80.5	400	416	74.5	122.25	70 –66
7.35	80.5	400	411.75	74.5	113.75	69 –70
7.55	80.5	400	413.25	72.5	115.25	70 –65
8.15	80.5	400	413.25	71	112.75	70 –62

the atmosphere, with open safety valve—or, what comes to the same thing, 3,000 pounds from $t = 100°$ to $p = 70 +$ atm. could not be called a 100 horse-power boiler with any propriety. Good ordinary practical conditions should do that, with satisfactory economy; and then fifty per cent. more should be obtainable to meet a sudden call, or to supply a brief deficiency. J. C. H.

XXVI. STEAM UNITS.

All measurements of the quantity of heat are based on the *thermal unit*, which, for British measures, equals the quantity of heat required to raise the temperature of one pound of pure water at or near its freezing point one degree Fahr.*

The unit commonly used to express the evaporative power of the fuel is the quantity of heat required to evaporate one pound of water at a temperature of 212° under the ordinary pressure of the atmosphere corresponding to that temperature. This was called by Rankine a "peculiar thermal unit," and its value given at 966.1 British thermal units, but has since been called the "*unit of evaporation*," which term is adopted in the foregoing general report of the committee. Its value, however, in the prominent American tables is given at 965.7 thermal units.

The *mechanical equivalent* of a thermal unit equals very nearly 772 foot-pounds of work, but the power that can be utilized practically per unit of heat depends on so many conditions that a universal standard of work or power (the rate of work) based on heat units, is impossible. Compound engines operated with high steam slightly superheated, require a little over 14 pounds of feed-water evaporated per hour, while there are still in use poor engines, ill-proportioned steam pumps, and the like, that require over 60 pounds, or say one cubic foot of water per hour, which was considered as about equivalent to a horse-power of steam in the days of

* Compare Rankine on Steam Engine, Art 298; Porter on the Richards Indicator, page 48.

Watt. It has of late years, however, been well accepted that 30 pounds of feed-water per hour is a fair standard of horse-power for average good high-pressure engines, such as are used for manufacturing purposes. Bearing in mind that this quantity of steam must be furnished by the boiler under actual conditions, the writer, in preparing the report of the committee of the judges of Group XX. appointed to test the boilers at the Centennial Exhibition, suggested to his associates, Messrs. Chas. T. Porter and Joseph Belknap, that the value of the "Commercial horse-power of a boiler be fixed at 30 pounds of water evaporated at 70 pounds gauge-pressure from a temperature of 100°."* This standard having been adopted in the foregoing report of the committee of the American Society of Mechanical Engineers, may be considered as established both by precedent and authority. It is fixed as equal to 34½ units of evaporation per hour, and is, for all practical purposes, equal to 33,333 thermal units per hour, making it convenient to obtain the horse-power by multiplying the total number of thermal units derived from the fuel per hour by 0.00003. It is of interest also to note that a cubic foot of steam at 70 pounds gauge pressure weighs 1-5 of a pound avoirdupois, so that a Commercial Horse-power on the above basis is also represented by 150 cubic feet of steam per hour at 70 pounds pressure.

In administering the steam supply of the New York Steam Company, the writer provided for selling steam at a fixed rate per thousand "*kals.*," explaining that a "*kal.*" meant a pound of water evaporated into steam. This term has been in use in that business since February, 1883, and has proved so convenient that the writer has suggested that it can possibly be utilized to express the unit of the Commercial Horse-power above referred to. On this basis a boiler horse-power would equal simply 30 "Kals" per hour.†

In preparing the general report of the judges of Group XX., Centennial Exposition, it was observed that if a boiler supplying any kind of pumping machinery be proportioned to utilize 10,000 heat units per pound of coal consumed (corresponding to an evaporation of about 9 pounds of water at 70 pounds gauge-pressure from a temperature of 100°) the number of foot-pounds of work obtained in the engine for each thermal unit would also represent the duty, in millions of foot-pounds of work obtained in the engine, for each thermal unit would also represent the duty, in millions of foot-pounds per 100 pounds of coal.*

From this it will be seen that the Commercial Horse-power above referred to corresponds to a duty of 59.4 millions of pounds lifted one foot high with 100 pounds of coal, which is about the average duty of the simpler class of pumping engines, but not of first-class engines. Evidently, for the better class of steam machinery of all kinds, the steam producing capacity of the boiler must be made to conform to the actual amount of steam to be used by the engines. Any standard of the horse-power of a boiler necessarily relates simply to its steam-producing capacity, referred to the arbitrary standard of a horse-power above mentioned. C. E. E.

XXVII. MEMORANDUM RELATIVE TO A STANDARD METHOD OF TRIAL-TEST FOR STEAM-BOILERS.

The method customarily pursued in the course of the work of the Mechanical Laboratory of the Stevens Institute of Technology, as instituted by the writer, and that practiced in his own professional work, has usually been such as to secure data sufficient to enable the observer to fill out all the columns of the Log-blank and Table of Performance, copies of which are appended.

In starting the trial, which is usually of at least ten hours' duration, it is customary, where it can be conveniently done, to get up steam with a fire of wood, which is raked out after steam has begun to form freely, and the trial commences with the introduction of a new fire, in which wood is used to ignite the coal, and is charged as a certain percentage of its weight of coal—forty per cent. is probably as accurate as need be. The damper should be carefully closed during the few

* See Report of Committee at page 181 of the report of the Judges of Group XX. International Exh., 1876. J. B. Lippincott & Co., Philadelphia.

† See "Estimates for Steam Users," Vol. V. Transactions American Society Mechanical Engineers, page 284.

* See Report of Judges of Group XX., Cent. Exh , pp. 21 and 115.

TABLE II.

AVERAGE AND TOTAL RESULTS OF TRIAL OF MECHANICAL LABORATORY, DEPARTMENT OF ENGINEERING, STEVENS INSTITUTE OF TECHNOLOGY.

TRIAL MADE

at ______________________

on ______________________

FUEL.

____

COMPOSITION.

____

Number of Trial.	Date of Trial.	Length of Trial.	Areas.				Ratio of Grate to Heating Surface.	Average Temperatures.				Average Pressures.			Consumption of Fuel.			Ashes.		Total Combustible.	Remarks.
			Grate.	Heating Surface.	Super-Heating Surface.	Least Cross-Section of Flues.		Boiler Room.	External Air.	Entrance to Chimney.	Feed-Water.	Barometer.	Steam Gauge.	Draught Gauge.	Total.	Per square foot of Grate per hour.	Per sq. ft. of Heating Surface per hour.	Total.	Proportion of Total Fuel.		
		Hours.	sq. ft.	sq. ft.	sq. ft.	sq. ft.		Fahr.	Fahr.	Fahr.	Fahr.	ins.	lbs.	ins.	lbs.	lbs.	lbs.	lbs.	per ct.	lbs.	

Apparent Evaporation.									Real Evaporation.											
Per Pound of Fuel.			Per Pound of Combustible.			Per square foot of Heating Surface, per Hour.			Per Pound of Fuel.			Per Pound of Combustible.			Per Square foot of Heating Surface, per Hour.			Sq. ft. of Heating Surface required to Evaporate one Cu. ft. of Water.		
From actual temp. of Feed-Water and at actual S. P.*	Equivalent fro. & at 212° F.	Equivalent fro. 212° F. & at actual S. P.*	From actual temp. of Feed-Water and at actual S. P.*	Equivalent fro. & at 212° F.	Equivalent fro. 212° F. & at actual S. P.*	From actual temp. of Feed-Water and at actual S. P.*	Equivalent fro. & at 212° F.	Equivalent fro 212° F. & at actual S. P.*	From actual temp. of Feed-Water and at actual S. P.*	Equivalent fro. & at 212° F.	Equivalent fro. 212° F. & at actual S. P.*	From actual temp. of Feed-Water and at actual S. P.*	Equivalent fro. & at 212° F.	Equivalent fro. 212° F. & at actual S. P.*	From actual temp. of Feed-Water and at actual S. P.*	Equivalent fro. & at 212° F.	Equivalent fro. 212° F. & at actual S. P.*	From actual temp. of Feed-Water and at actual S. P.*	Equivalent fro. & at 212° F.	Equivalent fro. 212° F. & at actual S. P.*
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	sq. ft.	sq. ft.	sq. ft.

* Steam Pressure.

TABLE II.—*Continued.*

TOTAL WATER FED TO BOILER.			Average Priming.	Total Water Primed.	WATER EVAPORATED INTO DRY STEAM.			REMARKS.
From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.			From actual temperature of Feed-Water and at actual steam pressure.	Equivalent from and at 212° F.	Equivalent from 212° F. and at actual steam pressure.	
lbs.	lbs.	lbs.	per cent.	lbs.	lbs.	lbs.	lbs.	

Average Amount of Superheating.	EVAPORATION FROM AND AT 212° F., EQUIVALENT TO TOTAL HEAT UNITS DERIVED FROM FUEL.			EFFICIENCY.			Values of A and B in $F = A\sqrt{H} \pm B$		HORSE-POWER.		REMARKS.
	Per pound of Fuel.	Per pound of Combustible.	Per sq. foot of Heating Surface, per hour.	Experimental.	Estimated.	$R = \frac{\text{Experimental.}}{\text{Estimated.}}$	A	B	Rated.	Actual.	
Fahr.	lbs.	lbs.	lbs.	per cent.	per cent.						

it became necessary to arrange for a comparison of several competing boilers of, fortunately, widely different types and forms. Through the liberality of Mr. J. B. Root, and with the earnest co-operation of Mr. Chas. T. Porter and others of the then exhibitors, it was rendered possible to construct a large surface-condenser, in which to condense *all* the steam made by each boiler during its trial. The arrangements were made with great care, and conducted under the writer's own personal direction and supervision, by carefully selected observers, and with the most cheerful and gratifying co-operation on the part of all the competing exhibitors. The result was the determination, with the most satisfactory certainty, of the real amount of total priming, as ascertained by observation of the total amount of water passing of as steam, and of the total amount of water carried out of the boiler unevaporated. Two of the boilers superheated their steam slightly; the others primed from three to seven per cent. The main object of the investigation, the determination of the question whether sampling steam can give fairly correct measures of the character of the mass, was in the writer's opinion well settled affirmatively by these experiments.

As to the best form of calorimeter, the writer is not yet fully satisfied, and hopes to find a way of making one that shall be at once simple, easily transported, and accurate. He has a strong impression that it will be a continuous calorimeter, but has very little doubt that improvements in accessory apparatus now in progress may make the Hirn form of instrument, sooner or later, a satisfactory one. The best work thus far has been done probably by the intermittent form of coil condenser, although experience with the continuous instrument has been very encouraging. Mr. Hoadley has done some beautiful work, and the apparatus described by Mr. Kent gives a means of checking weights, which is a very useful and almost essential improvement upon that type of instrument.

A steam-boiler trial in which the quality of the steam is not, as least approximately determined, cannot be accepted to-day as giving any reliable measure of the efficiency of a boiler.

Near the end of the series of data recorded in the blanks appended, are columns intended to include the constants, as derived from the trial, for introduction into the formulas of Rankine for efficiency of boiler, and of the writer for that of chimney. It was the writer's expectation to be able, in course of time, to accumulate such an extensive set of data in this form as would enable Rankine's formula to be adjusted for use in all trials of the usual forms of boiler, and with our native fuels. The American fuels, and our common boilers, cannot be estimated, in respect to efficiency, by the use of that formula, with the degree of exactness that is desirable. The writer has been accustomed, in making such estimates, as a rule, to adopt a value of the constant multiplier less by about ten per cent. than that given by the author of the formula. It is hoped that an opportunity, ere long, will be afforded to make the comparison here alluded to. R. H. T.

ON REPAIRING THE CABLES OF THE ALLEGHENY SUSPENSION BRIDGE AT PITTSBURGH, PA.

By FRANCIS COLLINGWOOD, M. Inst. C.E., M. Am. Soc. C.E.*

Selected Papers of the Institution of Civil Engineers.

The author was called upon, in August, 1883, to examine the suspension bridge across the Allegheny River at Pittsburgh, and to report upon its condition. The bridge had been in constant use since 1861, having been built by the late Mr. John A. Roebling, well known as a designer of this type of bridge. It consists of two full spans of 343 ft. 2 in. length each, and two half spans of 179 ft. 1 in. each. The floor is about 41 ft. wide, and is supported on four cables made of iron wire of an average diameter of 0.145 in. The inner cables are 22 ft. between their centers at the lowest point. spreading to 26 ft. at the points of suspension. Each cable contains two thou-

* In recognition of the merits of this paper the Telford Prize and Medal were awarded to Mr. Collingwood by the Institution of Civil Engineers.

sand one hundred wires, laid up in seven strands, and measures 7½ in. in diameter. The outer cables are 42 ft. between their centers at the lowest points, reduced to 38 ft. at the point of suspension. Each cable contains seven hundred wires, laid up in two strands, and is 4½ in. in diameter. There are heavy iron parapets at the outer sides of the sidewalks, and a system of long stays to each cable, to stiffen the bridge against vertical oscillations. The serving or so-called wrapping wire on the cables measures 0.098 in. in diameter, and is included, of course, twice in the diameter given for the cables.

The strands pass at the anchorages around cast-iron shoes, and the shoes are attached by pins to wrought-iron anchor bars. At each end of the small cables there are three, and at each end of the large cables nine, such bars. Those for the large cables are in two sets, so arranged that a rectangle surrounding the strands at the point of attachment is 22 in. square. These bars are the upper set of a series of similar ones, the lowest of which take hold of the anchor plate, and all the bars and the strands (to the clamp to be mentioned) were buried in the masonry of the anchorage.

From the shoes the strands converge for about 12 ft., at which point they are brought to a round form and held in close contact by a heavy iron clamp. From thence they are wrapped throughout, except where they pass over the saddles at the towers. Between the shoes and the clamp the wires in each strand are brought compactly together by a seizing of several turns of annealed wire at intervals of about every 7 inches. As originally finished, this unwrapped part of the cable was enclosed in heavy cotton duck, and all interior spaces were filled in solid with hot coal-tar, which had been boiled and treated with quicklime to neutralize any acid it might contain.

As an additional precaution, that portion from the clamp 3 ft. back into the masonry was surrounded by ¼-in. boiler-plate tightly clamped on, and coal-tar pitch was poured around the outer end. The whole cable for 12 ft. back, including the shoes, was then enclosed in brickwork and cement; 6-in. flags were laid in cement over the brickwork, and finally a second set of flags above these, to form the sidewalk. Over the side cables the foundations of the tool-houses took the place of the sidewalks.

Examinations had been repeatedly made at the points where the cables emerged from the masonry to see that the coverings were intact, but no more extended examination had been deemed necessary. The author felt, however, that the responsibility was too grave to allow him to assume that all was sound simply because the exterior appeared so. The sidewalk and flagging were therefore removed from over one end of a large cable, and on cutting through the canvas it was found that the tar had partially disappeared, and that the cavity was nearly full of a dirty, grayish liquid. There was also extensive rusting of the wires, so that the seizing wires, 0.06 in. in diameter, were in many cases rusted through, and the cable wires deeply pitted. A second cable end was opened, with similar results. A general survey of the bridge was then made, revealing fine cracks in the paint on the cables, which admitted moisture, grave defects in the masonry, particularly of one of the piers, and a number of other minor flaws.

A full statement of the facts was thereupon made to the directors, with the recommendation that every cable should be examined throughout and repairs made immediately; also that the cables and other ironwork should be scraped and repainted, all defective stones in the piers replaced by sound ones, the masonry repointed, and that the wrought-iron protecting-plates to the pier nosings should be repaired, &c.

Authority was given to the author to proceed with the work at once. As parts of it were entirely novel, it required close personal attention throughout. The first step was to determine how far the damage to the cables extended. One of the openings already made was therefore enlarged, and the boiler plate and wrapping removed, so as to expose the strands back to and around the shoes. It was found that the rust extended outside of the anchorage, and under the wrapping. It was then decided to remove every seizing, to cut out a slit about 45 ft. in the bridge floor to where the cable emerged above it, and to remove the clamp and

also the wrapping so far as might be necessary to discover the full extent of the damage. Accordingly about 10-ft. length of cable was unwrapped, leaving the wires exposed and entirely free for about 22 feet of their length.

On examination the serious damage to the wires was found to extend about 3 ft. from the anchorage outward. Beyond this there was a little dry rust, but no pitting; and still farther from the anchorage the paint on the interior wires was yet gummy.

The rust seemed to be of two kinds. First, a red oxide, where the wire appeared to have been attacked as if by acid, the so-called fiber being exposed. In some such cases the rust had eaten through the exterior in a narrow slit, and had then attacked the interior, leaving a shell only. The second form of rusting was by the formation of a hard blackish substance containing much sulphur, which, when scaled off, left a deep pit, as if gouged out by a chisel.

In one cable end eight wires had been eaten entirely through, and one wire or more in each of the others. Referring to the composition of the rust and the liquid found among the strands, the following letter to the author is pertinent. The writer, Professor Otto Wuth, is a practical chemist residing in Pittsburgh, and has been engaged in the manufacture of the various products resulting from the distillation of coal-tar.

PITTSBURGH, *Aug.* 18*th*, 1883.

"I have carefully examined the specimens of scales you took from the wires of the cables of the suspension bridge, and found them to be a combination of the hydrated peroxide of iron and sulphate of iron. The liquid consisted of a weak solution of carbonate and sulphate of ammonia, colored by tarry matter, and is almost identical with tar-water from the gasworks. The cables, as you stated, were first coated with boiled linseed oil, and afterwards with coal-tar. The tar had evidently not been heated long enough and high enough to drive off all the water and the salts of ammonia contained in all coal-tar at the rate of 5 to 7 per cent.

"The reaction of coal-tar is always alkaline—*very* alkaline—never acid. The reaction of the acids contained, such as carbolic and cresylic, is alkaline; they do not act like the mineral acids on iron. By heating coal tar with caustic lime you only convert the carbonate of ammonia into caustic ammonia. Now, the action of the coal-tar upon the wires has undoubtedly been this:

"The oils contained in the tar first dissolved the coat of linseed oil; then the sulphuret of ammonia, which is contained in the tar in considerable quantity, acted upon the surface of the iron, converting it into the sulphuret of iron, which again was converted into the sulphate by the oxygen of the air, which could not have been completely excluded. This alternate action of the sulphuret of ammonia and air was continued until the sulphuret was entirely exhausted. The oxidation was further carried on by the atmospheric air in the presence of water and carbonate of ammonia."

How the water came to be where found it is not easy to say. The upper surface of the strands was but 18 in. below the surface of the sidewalk, and exposed to considerable alternations of heat and cold. A cavity was evidently formed by the tar gradually oozing into the surrounding brickwork when exposed to the heat of hot summer days. Air would then slowly percolate to and fro as the masonry changed in temperature, and moisture would probably be condensed, and the water slowly collected. It is possible that part of the sulphur and ammonia accumulated in this way, as they must be constantly present more or less in the atmosphere of such a smoky city as Pittsburgh.

It was evident that the only course to pursue was to cleanse the wires thoroughly. This was at first attempted by the use of solvents, such as benzole, kerosene, &c., but it was impossible to cleanse the interior strands in this way, even by drenching them. Scraping each individual wire was then reluctantly resorted to. After trying various scrapers, none of which worked satisfactorily, one was adopted which proved cheap and effective. A $\frac{5}{8}$-in. steel rod had one end flattened and turned up for about $\frac{1}{2}$ in., so as to make with the rod an angle of about 80°. A semi-circular notch, of about the same diameter as the wire, was then filed in the end, and the edge made sharp, hardened and tempered. With

these, four men could clean about one hundred and seventy-five wires in ten hours. To get at the interior strands, wedges were used to force them apart. Whenever a damaged wire was found, it was marked by a bit of wire twisted on. All such wires were examined, and if the loss of strength was 10 per cent., or more, it was repaired, otherwise it was passed. It was soon found that the damage was almost entirely confined to the outer two layers of wires in each strand. The seizings around the strands had held the wires so close in contact that the destructive agent had not penetrated farther.

While this preliminary work was going on, experiments were made to determine the best method of repairing the damaged wires. The only previous work of this kind, so far as known, was that done at Niagara Falls, where, in making the final splice, the two parts of a wire were kept under strain by a bar having a lever pivoted across each end. The shorter arms of these levers were provided with clamps and thumb-screws for taking hold of the ends of the wire to be joined, and gave a lever arm of 2 in. The other arms were 10 inches long. The short lever-ends having been clamped to the extremities of the cable wire, one of the long arms was fastened by a wire to some fixed object; to the second long arm a spring balance was attached, and to this a pair of small pulleys and tackle, leading finally to some other fixed object. By this means a definite pull on the balance could be transmitted to the two branches of the cable wire, since the separation of the long arms would draw the short ones towards each other.

The difficulties with this apparatus were, first, that it was not self-contained; secondly, the range of motion being small, it required exact fixing on the wires to ensure the ends being in line when the strain was on; thirdly, the room was insufficient for wrapping the splice.

The author devised an apparatus as follows: A stiff, square bar 2 ft. 6 in. long, had its sides finished smooth; one end was flattened, and to this end a stiff cross-head about 7 in. long was firmly riveted. One end of the cross-head was provided with a clamp and thumb-screw for holding the wire to be spliced. The other end of the cross-bar had a notch for receiving the ring of a heavy spring balance. A second cross-bar about 11 in. long was fitted with a long socket near its center, so as to slide freely on the long bar. One end of the sliding bar had also a clamp and thumb-screw for holding the opposite end of the wire to be spliced. Through the remaining end of the sliding bar was a hole parallel to the main bar of the machine, through which an eye-bolt passed freely. The eye-bolt had a long thread cut upon it, and carried a hand-wheel with a corresponding internal thread. The hook of the balance passed through the eye of the bolt.

This machine gave plenty of movement for extending the balance, and taking up the slack of the wire, and was very convenient in all contracted places.

According to Mr. Roebling's original notes, an abstract only of which was available, the maximum working strain per wire is 267 lbs., of which 109 lbs., or 41 per cent., is live load, and 158 lbs., or 59 per cent., is dead load. A piece of wire, said to be from the original bridge wire, was tested, with the undermentioned results. The stretch in this and the following tests was taken on one foot of length by a finely divided vernier gauge, having a multiplying lever, and a second vernier for the finer readings.

Diameter of specimen, 0.144 in.; area, 0.16286 square inch. The readings were uncertain up to 200 lbs. strain, owing to inaccuracy in the adjustment of the gauge:

Strain. Lbs.	Gauge-reading. Foot.	Difference. Foot.
200	1.00017	0.00023
300	1.00040	0.00024
400	1.00064	0.00023
500	1.00087	0.00022
600	1.00109	0.00022
700	1.00131	0.00020
800	1.00151	0.00033
900	1.00184	

At 1,260 lbs. the wire broke with a measured set of 0.013 foot. Diameter at point of rupture, 0.110 inch; strength per square inch of full section, 77,365 lbs.

A new splice in a wire when tested gave, under a strain of 300 lbs., an addition of 0.018 in. in length by slip and stretch. Now, in splicing in a new piece of wire, the final splice must evidently be

made under an excess of strain sufficient to compensate for three things: first, for the probable slip in the splice in taking up a working strain; secondly, for the stretch that will occur in the part (about 2 ft. long) contained in the machine while splicing, when it comes under strain; and, thirdly, for the extra strain induced by pulling the wire out from a straight line while splicing.

To determine this excess of strain the following calculations were made:

The slip at the splice is, say..........	0.0180
Taking 200 lbs. as the average strain in a wire with the bridge in use, and 0.00023 foot as the stretch per 100 lbs., the 2-feet length of wire in the machine will stretch 0.00023 foot $\times \frac{200}{100} \times 2$ feet $\times$ 12 =......................	0.0110
Suppose the wire in splicing to be drawn 4 inches from a straight line at the center of 20 feet in length, the additional length required is =..........	0.1332
Total excess of length requisite in making the splice =..............	0.1622

On the supposition that the wrapping around the cables is tight enough to oblige the first 10 ft. length under the wrapping to take up the extra strain, and inasmuch as the wires pass around the shoes, and have, therefore, a double length, the strain would act on about $(12' + 10' + 10' \times 2)$; or, say, 60-ft. length of wire. According to the previous test, to stretch 60 ft. of wire 0.1622 in., a strain would be required equal to

$$\frac{0.1622 \text{ inch}}{12} \div \frac{0.00023 \text{ foot} \times 60 \text{ feet}}{100} = 98 \text{ lbs.}$$

This would give the strain on the balance as 200 + 98, or, say, 300 lbs.

A balance with 400 lbs. capacity was therefore provided, but on trial it was found that a pull of about 500 lbs. was necessary to ensure a full strain in the wire after completion of the splice. The reasons for this seemed to be, first, that the stretch did not probably reach so far as 10 ft. under the wrapping; secondly, the friction in passing around the shoe had not been taken into account; thirdly, the wires were frequently partially bound by other wires, and the final splice was nearer one end of the free wire than 10 feet.

Another element of uncertainty was the constantly varying load, as the traffic on the bridge is very heavy and subject to rapid and extreme fluctuations. Still another practical difficulty was the fact that, from loosening the strands and removing the clamps and wrappings, the angle made by the unwrapped strand with the round cable was removed 10 ft. farther from the shoes and the several cable wires were no longer equally strained.

For these various reasons it became necessary to assume limits of strain, within which differences would be allowed. As the limit of elasticity of the wire was from 800 to 1,000 lbs., and the extreme working strain 267 lbs., it was evident that if the minimum strain per wire was sufficient, the maximum strain might be largely increased without danger of rupture or inharmonious working. Furthermore, any excess in pull in the sound, new wires would tend to relieve the slightly damaged wires which were not to be repaired. This reasoning, of course, would not apply if carried too far, since an excess of strain introduced into a large number of wires for a considerable length would have changed the curve assumed by the cable.

The minimum fixed upon was 200 lbs. per wire, and was ensured in the following manner: Each wire, as spliced, was marked by a tag, and once a day all the wires were tested by applying a spring balance at the center of their free length, and pulling them out two inches from a straight line. Suppose the balance to then mark 8 lbs., by the parallelogram of forces the proportion $\frac{4}{12}$ inch : 10 feet = 8 lbs. : 240 lbs. = strain of wire. This simple test saved all necessity for immediate inspection of each splice.

Two men could repair from eight to ten wires daily, making two splices in each. They soon found that they were able to dispense with the weighing apparatus, and to judge the tension closely enough by feeling.

A simpler tool was then constructed for holding the wire while splicing. It was made of two bars or legs hinged together, and each bent so as to assume a form much like a pair of pin dividers when they are opened wide, and the pin and point are in position for use. A tightening-rod, with a head at one end and a thumb-screw on the other, passed through the legs at the knuckles. The lower end of each leg was grooved across on each side, and a wedge-key fitted for

clamping an end of the wire to be spliced. Most of the splicing was done with this tool, but the one with the balance was best where the space was contracted.

The total number of wires spliced was four hundred and eighty-four, of which one hundred and seventy-five were in one cable end and one hundred and seven in another. Care was taken to distribute the splices lengthwise, so as not to interfere with the smooth wrapping of the cable. The splices were made by filing the end of the wire to a flat, sloping face of 3 in. length, and so as to reduce the wire at the extremity to about one-third its diameter. The wire was then laid face downwards on an iron anvil, and the convex side nicked for 3½ inches with a tool, having spaces of 0.033 in. to correspond with the diameter of the splicing wire.

After preparing the two ends, they were placed in the machine, the proper strain was applied, and the flat surfaces were brought in contact and tightly clamped by a hand-vice on each side of the center of the splice.

The splicing wire was next tightly wrapped by hand; beginning at the middle of both splice and wrapping wire, and wrapping up to one of the hand vices, then removing this vice to a second and third hold, and each time wrapping up to it. The final finish at this end was given by wrapping ¾ in. beyond the filed portion of the wire, and fastening the splicing wire by passing it twice under its own coils. The opposite half splice was finished in the same manner.

The integrity of the splice depends upon the care taken in adjusting the parts accurately to each other, and keeping a constant strain on the splicing wire.

Comparative tests were made of a piece of wire from one of the cables, and of a new splice in the same, and the results are given below.

Diameter of wire=0.144 in., area= 0.16286 square inch. The whole wire behaved as follows:

Strain. Lbs.	Gauge-reading. Foot.	Difference. Foot.
200	1.000050	0 000240
300	1.000290	0.000245
400	1.000535	0.000250
500	1.000785	0.000210
600	1.000995	0.000270
700	1.001265	0.000275
800	1.001540	

At 1,450 lbs. the wire broke with a set of 0.0208 foot on 1 foot. The reduction of area at the point of rupture was 51 per cent. The strength per square inch was 89,015 lbs.

The splice from the same piece of wire gave results as follows:

Strain. Lbs.	Gauge-reading. Foot.	Difference. Foot.
100	1.000390	0.000225
200	1.000615	0.000665
300	1.001280	0.000505
400	1.001785	0.000550
500	1.002335	0.000405
600	1.002740	0.000450
700	1.003190	0.000285
800	1.003475	0.000180
900	1.003655	

At 1,350 lbs. the spliced wire broke with a set of 0.0243 foot on 1 foot. The rupture occurred in one wire at 2½ inches from the end, or 1 inch from the center of the splice, and with a large local reduction in area. In making the test the coils of wrapping wire were left slightly loose at the ends to represent a probable case in actual work.

In comparison with the uncut wire, the splice shows a strength of 93 per cent. Since the splice at one end of each piece of new wire introduced has been subjected to a strain far above the working strain, and the final splice has resisted a pull of at least 200 lbs., it is evident that the greatest possible additional slip would be that arising from the slip at the final splice, due to the difference between a strain of 200 lbs. and a maximum strain of 267 lbs. As this amount, which is very small, must be distributed over about 60 ft. length of wire, it may be neglected.

A piece of new Bessemer-steel wire was tested with the following results:

Diameter of wire =0.147 inch, area = 0.01697 square inch:

Strain. Lbs.	Gauge-reading. Foot.	Difference. Foot.
300	1.000725	0.000200
400	1.000925	0.000230
500	1.001155	0.000345
600	1.001500	0.000125
700	1.001625	0.000230
800	1.001855	0.000280
900	1.002135	0.000270
1,000	1.002405	0.000335
1,100	1.002740	0.000405
1,200	1.003145	

At 1,550 lbs. the Bessemer-steel wire broke with a set of 0.0243 foot on 1 foot.

The reduction of area at the point of rupture was 47½ per cent. The strength per square inch was 90,442 lbs.

The tensile strength of similar sized iron-wire is given by the Trenton Iron Co. at 91,278 lbs per square inch.

Considering that the old wire was more or less damaged by rust, its uniformity of stretch, large reduction, and close approximation in strength, show it to be entirely unchanged by use.

All the wires in a cable having been repaired, the first step towards closing it up was to jar it thoroughly with mallets, and to get out all loose rust and dirt, after which it was thoroughly saturated with raw linseed oil. To reach the interior wires with the oil, a chisel bar had to be forced through in every direction until no uncoated wire could be found. Two days afterward a coating of boiled linseed oil was applied, and then the seizings were replaced on the part of the strands not to be wrapped. The most serviceable tool for compacting the wires of a strand to a round form, prior to replacing the seizing, was one devised for the occasion. It consisted of two semicircular pieces of iron hinged together at one end of each, and with the free ends bent radially outward. One of the free ends was left longer than the other, and had a hole slotted radially in it, to allow play for the passage of the screw. The other free end was tapped for the screw. The screw had a thread about 4 inches long, and a stem about 1 foot long terminating in a T for convenience in turning. A collar between the screw and stem gave a bearing against the slotted end of the clamp. The interior strands, which were hard to get at, were compacted with this clamp with facility.

The work of bringing the strands together again to the round form of the cable, just within the anchorage, was troublesome but not new. The wrapping of that portion outside the anchorage was at first accomplished with the special wrapping machines used when the bridge was built. As it was impossible to employ these at all points, the ordinary serving mallet familiar to sailors was afterwards adopted, as with care it was found that good work could be effected.

The cable was saturated with white lead and oil in advance of the wrapping. Several coats of the same composition were afterwards applied over the wrapping and strands. Wherever direct vision could not be obtained, the aid of a mirror was found necessary to ensure a perfect covering.

As it would afterwards have been difficult to paint thoroughly that portion of the strands near to where they converge to a round form, it was considered best to protect them by filling for a space of 2 ft. back from the clamp with paraffin. To do this a dam of putty was made across the cable, and the 2-ft. length was surrounded by canvas, into which melted paraffin was poured until when cold the canvas would hold no more. Changes of temperature have thus far had no effect upon the paraffin as a protection.

Experience having demonstrated the imperative necessity of having all parts of the cable accessible for examination, it was decided to leave tunnel-openings around the strands in the anchorages. The bottom of each opening in the masonry was therefore cut so as to drain to an opening in the front wall. The sides were removed to about 6 ft. by 16 feet, and lined with 13-inch brick walls laid in cement-mortar, the cable passing freely through the front wall, with space all around for painting. A jacket was afterwards put on each cable to keep out moisture as far as practicable. As the head-room was insufficient for brick arches, the side walls were capped by an iron box-frame, which was covered by a series of cast-iron plates grated on the upper surface, which surface was at the side-walk grade. The plates were locked together and cemented, to prevent water from dropping on the cables; and the last plate of the series was provided with lifting rings.

In the alternation of mild and extreme cold weather, water of condensation was found to drip from the plates; to prevent this a light wooden frame has been covered with well-painted canvas and placed in the tunnel in an inclined position over the cable. Cork paint may be hereafter applied to the interior of the tunnels.

The only other point of interest was the method pursued in removing the old paint from the cables preparatory to repainting. The necessity of this was made apparent by the cracks retaining moisture after every shower; as it was

reasoned that fresh paint would soon crack in the same lines, and but temporarily cure the evil. Ordinary scrapers were first tried, but were ineffective on account of the gumminess of the paint. This led to the use of cutting-tools such as chisels and drawing-knives, by which to slice it off. By keeping these sharp, the work was fully trebled in quantity over that at first attainable. The average length of large cable cleansed by one man in a day was about 25 ft.

REPORT OF THE ROYAL COMMISSION ON LONDON SEWAGE.

By Capt. DOUGLAS GALTON, C.B., F.R.S.

From the "Journal of the Society of Arts."

The disposal of the London Sewage has been a burning question for the metropolis during the greater part of the last forty years, and it will be convenient in the first place to give a brief historical summary of the various proceedings which have taken place. The present state of the question is principally due to what may be termed the hap-hazard way in which the metropolitan drainage system came into existence. The main metropolitan sewers were originally the streams and brooks which conveyed the water of the higher levels direct to the river; these were the Fleet, the Ranelagh, the Falcon brook, the Effra, &c., &c. There were, moreover, in the low-lying lands of the metropolitan area, which were under high-water level, cuts, or channels, for drainage, which were guarded by sluices in the river bank, to be opened at low water, to allow of the passage of upland water, and closed on the rising of the tide. These cuts, sluices, and brooks, were under the jurisdiction of various local bodies, termed Commissioners of Sewers. As houses spread over the ground between the streams, brooks, and cuts, the natural water-supply diminished, and they became the outlets for the refuse water of the population. The drains from side streets were turned into them; they were gradually covered in, upon no systematic plan; so long as the refuse water passed away out of sight, it did not seem to matter what happened to it. These covered sewers occasionally became choked with deposit, and had to be cleaned out, which seems to have given rise to the idea that sewers should be of sufficient size to be entered by a man for the purpose of cleansing; the idea of a self-cleansing sewer did not prevail in those days. Before the introduction of water-closets, fœcal matter was received in cesspools, which were emptied periodically; but when water-closets were introduced, an overflow from the cesspool was carried into the sewer, and subsequently the water-closets were discharged directly into the drain. When it passed to the Thames, the wide foreshores of which lay in the heart of the metropolis, it formed banks of black, fœtid mud, containing considerable quantities of organic matter of a most putrescent kind. Alternately immersed in water and exposed to the action of air, which, in consequence of its porous condition, it absorbed in large proportion, this mud united all the conditions favorable for the most active putrefactive fermentation, evolving not only gaseous emanations, but diffusing also a large amount of putrescible soluble matter through the river, which supplied additional material to the process of decomposition which was going on in the water itself.

These various evils, taken in connection with the defective construction of the sewers, resulted in producing serious dangers to health. The Metropolitan Sanitary Commission, of which Mr. Edwin Chadwick was a prominent member, was appointed in 1846, and reported in 1848. They recommended a revision of the drainage system of the metropolis, and enunciated the view that the rainfall should be separated from the sewage proper, the rainfall being carried direct to the river, in the old brook courses, and that the drains should be made on new lines, and of a size which would insure a sufficiency of sewage in the drain to cause an adequate velocity of flow, so as to pre-

vent stagnation and deposit. They further recommended the consolidation of the various authorities which dealt with sewers and with roads under one jurisdiction. Other reports from the General Board of Health recommended that the water-supply of the metropolis should similarly be placed under one authority, so that the supply might be more effectually regulated and controlled, both in quality and quantity. These all pointed to the vast importance of creating a united government for London.

The report of the Metropolitan Sanitary Commission led to the consolidation, under the Metropolitan Commissioners of Sewers, of the jurisdiction exercised up to that time by eight separate Commissions of Sewers, but the sewers in the City of London remained under the jurisdiction of the city authorities. Upon this new body devolved the duty of devising a scheme of drainage. In the first place, they obtained that the Ordnance Department should make a survey of London on the scale of five feet to the mile. They did not adopt the views of the Royal Commission as to the separation of sewage from rainfall, but the engineer to the new Commission, Mr. Frank Forster, laid down certain principles of design, and proceeded to prepare plans for intercepting the sewage from the upper districts, and conveying it to the river direct, so as to prevent the flooding of the low-lying districts. He also proposed to intercept the sewage from the Thames within the metropolis, and convey it to points lower down the river. He appears to have completed a design for dealing with the sewage on both sides of the river, but in the long discussions which ensued, his health failed, and he retired, and died. His successor was Mr. (now Sir Joseph) Bazalgette, who was charged by the Commission with the duty of preparing, in conjunction with Mr. Haywood, engineer to the Commissioner of Sewers for the City of London, a plan for dealing with the Sewage of London north of the Thames. In 1855, however, the Metropolis Local Management Act was passed.

This Act vested in the Metropolitan Board of works all the main sewers which at that time were under the jurisdiction either of the Commissioners of Sewers for the City of London, or of the Metropolitan Commissioners of Sewers; but the district sewers were placed under the management of the Vestries, and thus there was no single authority to whom was committed the duty of controlling the whole system of drainage of the metropolis.

The Act went on to enact as follows:—

"Such Board shall make such sewers and works as they may think necessary for preventing all or any part of the sewage within the metropolis from flowing or passing into the river in or near the metropolis, and shall cause such sewers and works to be completed on or before the 31st day of December, 1860."

It was then further enacted that—

"Before Metropolitan Board of Works commence any sewers and works for preventing the sewage from passing into the Thames as aforesaid, the plans of the intended sewers and works, together with an estimate of the cost of carrying the same into execution, shall be submitted to the First Commissioner of Her Majesty's Works and Public Buildings, and no such plan shall be carried into effect until the same has received the approval of the said Commissioner."

It is unnecessary here to follow the detailed proceedings which took place upon this new arrangement; they are given in the "Report of the Royal Commission on Metropolitan Sewage Discharge." It will suffice to state generally that the Metropolitan Board appointed Sir Joseph Bazalgette as their engineer, and that plans were at once prepared.

The main principles upon which the design was based were—(1) the acceptance of the existing state of things involving the removal of sewage combined with a proportion of rainfall, and the rejection of the suggested separation of sewage from rainfall, which would have entailed the consequent construction of a new system of sewers of limited size; (2) the retention of the brook courses as main sewers; (3) the protection, by means of a new system of intercepting sewers, of the low-lying districts from the sewage of the upland districts; (4) the removal of the sewage from the Thames within the metropolitan area, and its conveyance to parts of the Thames outside and below that area.

The main sewers followed generally the lines of the sewers now executed. They

were proposed to terminate, on the north side of the Thames, in an outfall at Barking Creek, and on the south side, in an outfall in the Plumstead Marshes. The cost was estimated at £2,300,000. When these plans were submitted to Sir Benjamin Hall, then First Commissioner of Her Majesty's Works and Public Buildings, he referred the question of outfall to Captain Burstal, R. N., who reported that the northern outfall should be removed lower down the river, at least as far as Rainham Creek, and that the southern outfall should be placed in Erith Reach. The plans were subsequently submitted to three referees, to report their views generally on the proposed main drainage of the metropolis, and on the points of outfall. The referees were Mr. James Simpson, engineer to the Chelsea and Lambeth Water Companies, now dead; Mr. Blackwell, engineer to the Kennet and Avon Navigation, also dead; and Captain Douglas Galton, the author of this paper.

The referees agreed generally with the principles upon which Sir Joseph Bazalgette's scheme proceeded; that is to say, they agreed that a separation of the sewage and rainfall would not be expedient in the case of the metropolitan sewage; that the question of chemical deodorization, or of utilization of the sewage on land, would, under the circumstances, present very great difficulties, and entail a heavier expense than a properly considered scheme for turning the sewage into the Thames at a fitting outfall. But they did not concur in the manner in which these principles were proposed to be carried into effect by the scheme of the Metropolitan Board of Works. Their objections may be summed up as follows:—

1. The scheme did not sufficiently provide for the future; the population of the metropolis in 1851 was about 2,400,000; it has increased regularly by one-fifth in every ten years since the beginning of the century, and in 1881 it amounted to nearly 4,000,000; in 1901, at this rate of increase the population will amount to about 6,000,000. The scheme of the Metropolitan Board only provided for a prospective population of 3,255,000, and the referees objected that, on that account, and on account of the limited provision it made for the removal of rainfall, the sewers would frequently overflow into the Thames, and that the low-lying districts would be liable to be flooded.

2. That the scheme of the Metropolitan Board would remove by complete gravitation the drainage of only 27 square miles, out of 118 square miles, to the outfalls at Barking and Plumstead, and the drainage of 31 square miles would be lifted once, and the drainage of 43½ square miles would be lifted twice, whereas, by constructing the tidal channels for the removal of the sewage as suggested by the referees, all the sewage which would flow to a point five feet above Trinity high water mark near the metropolitan boundary, that is to say, the drainage from 81 square miles of the metropolitan area could have been removed by gravitation to Sea Beach.

3. That the proposal to turn the metropolitan sewage into the Thames, near Barking, or even at the outfalls in Erith Reach, suggested by Captain Burstal, would not prevent the sewage from returning within the metropolitan limits, and would be injurious to the eastern districts of the metropolis which lie adjacent to the Thames, the population of which was rapidly extending.

4. The referees consequently proposed that the sewage should be taken to a point on the north side of the Thames between Mucking Lighthouse and Thames Haven; and on the south side to Higham Creek in the Lower Hope, because the strength of the current at both of these places is sufficient to prevent any deposit of materials brought down by the sewage; and the great expanse of water, the continual accession of clean tidal water, and the rapidity of current would ensure the mixing of the sewage with water under the most favorable circumstances, and at a point where the shores were almost uninhabited. The referees added that these were the only places in the river, either above or below, which entirely fulfill the conditions essential to the object in view.

5. The referees were not, however, satisfied with the removal of the sewage to a point lower down the river, but contemplated uniting with the removal a form of purification. Instead, however, of deodorization by chemicals, they proposed the principle of partial purification by dilution and movement during the flow of the sewage through the outfall

channels for a distance of nearly twenty miles. To effect this, they proposed that there should be added to the sewage, at the head of the outfall channel, a volume of water direct from the Thames, at high water, equal to six times the then estimated dry weather flow of the sewage.

6. The referees estimated the total cost of the intercepting and collecting sewers within the metropolis, and of the outfall channels, at £5,438,000, of which about £2,250,000 was the cost of the outfall channels.

Upon this report of the referees, the Government declined to sanction the plans of the Metropolitan Board of Works; a long correspondence ensued, and during the discussion the Liberal Government then in office was succeeded by a Conservative Government, and Lord John Manners became First Commissioner of Her Majesty's Works. The new Government passed an Act of Parliament, in 1858, relieving the Metropolitan Board of Works from the necessity of obtaining the sanction of the Government to their scheme of drainage, or to the position of the outfalls, on the ground that as the metropolis paid for the works, they had a right to construct them in any way they thought fit. This doctrine was sound so far as the sewers within the metropolis are concerned, but it was most unsound, short-sighted, and unstatesmanlike in respect of the outfalls outside the metropolitan area, because upon the position of the outfalls depends the condition of the Lower Thames—that great navigable highway—in the length of which the metropolis occupies but a comparatively small portion. It is, however, clear, from the discussion which took place in Parliament, that it was intended that the sewage should be deodorized at the outfalls; and should not be turned into the river in a crude state.

This oversight of the interests which the public at large, independent of the metropolis, have in the Thames, was attempted to be remedied twelve years subsequently by the Thames Navigation Act of 1870, which provided that the Metropolitan Board of Works should, at their own expense, keep the Thames free from such banks or other obstructions to the navigation thereof as may have arisen or may arise from the flow of sewage at the outfalls, for the time being, into the river; but this Act makes no mention of the necessity of maintaining the purity of the Thames water, or of conditions which might affect health.

Upon the passing of the Act of 1858, the Metropolitan Board of Works proceeded to construct their sewerage system, which was estimated to cost somewhat under £3,000,000. The northern and southern outfalls were completed and in use in 1864, and the main drainage system was formally opened by H. R. H. the Prince of Wales, in 1865. The works are stated to have cost about £4,600,000, but no provision was made for deodorization, the crude sewage being turned into the river at the outfalls.

It is also to be observed that the criticism of the referees upon the limited provision for carrying off the sewage made by the Metropolitan Board have been subsequently fully justified. In the report of the Metropolitan Board for 1881, occurs the following paragraph:

"The floodings of heavy rains which have occured on several occasions, in recent years, in some of the populous districts of London, principally those on a low level, made it necessary for the Board to determine upon the construction of some additional sewers to carry off the storm water. The cost of these additional works is mentioned at £1,000,000.

The report further says:

"The Board has lately resolved to enlarge the reservoirs at Barking and Crossness outfalls, to 50 per cent. beyond the present capacity, to admit of the largely increased quantity of sewage being stored until the ebb tide, instead of, as have been occasionally necessary, having to be discharged on the flood tide, and thus giving rise to complaints that the Thames water at Woolwich was impure."

And the Royal Commissioners on Metropolitan Sewage Discharge report that these extensions will bring the total expenditure to £6,250,000, without any works for deodorization, as against the £5,438,000 stated by the referees to be necessary.

Whilst these large works were going on under the Metropolitan Board, it may be said that the whole system of minor sewerage and drainage in the metropolis was also undergoing revision, and considerable efforts have been made, by the use of catch-pits and by regulation, to

prevent road grit passing into the sewers. The subsidiary drains may be said generally to have been to a considerable extent changed from being sewers of deposit into self-cleansing sewers, although no doubt instances to the contrary may still exist.

In the central districts of the metropolis the extensive foreshores of the river, with their banks of mud, have been much diminished by the construction of the splendid embankments which extend on the north from Blackfriars, to above Chelsea, and on the south from Westminister to Vauxhall. In other parts wharves have been raised to prevent the overflow of high tides. The general result of these works must be judged by the health of the population. If the death-rate be accepted as a test, it is noteworthy that the average death-rate of the decade 1841-50 was 24.8 per 1,000. If the conditions had remained *in statu quo*, except as to increase of population, the death-rate of 1871-80 ought to have been 26.2, according to Dr. Farr's tables for increase due to density of population. In point of fact it was only 22.5. It may be contended that this is too high; but at any rate the diminution may fairly be put down to improved sanitary conditions, amongst which the drainage system stands prominently forward.

But in looking back at the history of the sewage question it seems astonishing that good results could have emanated from the arrangement made in 1855. That arrangement abolished uniformity of control over the main roads of the metropolis; and whilst it gave the jurisdiction over main sewers to the Metropolitan Board of Works, it placed the district sewers under the control of separate Vestries; the control of the water supply remained in the hands of eight different companies. If there is one thing more certain than another, it is that a uniform control is especially necessary in the drainage and water supply of a district; the control over the sewerage especially should exist from the reception into a public sewer of the sewage from the house drain until it reaches the point of outfall. In the metropolis, Parliament has done its best to prevent uniformity, and to prevent the Metropolitan Board of Works from having a fair chance. If there had been such uniformity of control over the water supply and drainage, the quantity of water consumed, and consequently of sewage, might have been more effectually regulated: and if the details of the district drainage had been under one and the same authority as the main sewerage, it is quite possible that inasmuch as the conditions of the districts vary, so the sewerage might have been arranged in special cases on somewhat different lines. It is conceivable that in some parts, sparsely occupied, the separate system might have been to some extent introduced, although it was inadmissable in the more densely inhabited portions of the metropolis.

There have been now three public inquiries into the evils alleged to arise from the sewage discharge at the outfalls at Barking and in Erith Reach. The first was by Sir Robert Rawlinson in June, 1869, upon a complaint from the inhabitants of Barking that the river was silting up so as to affect the navigation, and that the pollution was dangerous to the health of the inhabitants of Barking. Sir Robert Rawlinson reported the allegation to be only partially proven, adding that the unsanitary condition of the town of Barking prevented the inhabitants from being in a position to establish deterioration of health from the London sewage, and that the main channel of the Thames had not been reduced; but that the Thames is polluted by the metropolitan sewage, and that deposits of mud had taken place on the shores of Barking Creek, but from what cause had not been proven.

The second inquiry was by means of an arbitration, under the Thames Navigation Act of 1870, between the Conservators of the Thames and the Metropolitan Board of Works. The Conservators of the Thames contended that certain mud banks had resulted from the discharge of sewage at the outfalls, and had injured the navigation. The arbitrators determined that the Barking and Halfway Reaches, in which the banks are situated, were better for navigation than when the outfalls were opened. That the banks had arisen from dredging operations carried on by the Conservators of the Thames, or sanctioned by them, such dredging having affected the direction of flow of the currents and altered the sectional area of the river.

The third inquiry was that recently

held by the Royal Commission on Metropolitan Sewage Discharge, which was directed to ascertain whether any evil rerulted from the discharge of sewage into the Thames by the Metropolitan Board of Works, and if so what measures could be applied for remedying or preventing the evil. The report shows generally that the pollution of the river arises from the large volume of the sewage thrown into it, in a locality where the area of the river is small in comparison to the volume of sewage discharged into it. The referees estimated that the total quantity of sewage from the metropolitan area discharged into the Thames in dry weather amounted, at the time of their inquiry, viz., in 1857, to 15,250,000 cubic feet per day. The population, which was then about 2,500,000, has now increased to something between 3,800,000 and 4,000,000, and in the report of the Royal Commission the volume of the dry weather sewage is now stated to amount to 23,000,000 cubic feet per day, which is an increase very nearly proportionate to the increase of population. The report of the Royal Commission states that the low water area of the river near the outfalls is about 30,000 square feet, and that this volume of sewage would fill a length of about 750 feet of the river at low water. At the present rate of increase of the metropolis, the population will amount to 6,000,000 in 1890, and at a proportionate rate of increase the sewage will amount to nearly 35,000,000 cubic feet per day.

It must not, however, necessarily be assumed that the volume of sewage will continue to increase in the same proportiton as hitherto, inasmuch as care in regulating the water supply would materially reduce the dry weather flow of sewage in London, as it has in Liverpool, Manchester, and other towns; but this seems to be one of those questions which must wait until the Government of London has been consolidated under one jurisdiction.

Tne Royal Commission report that "we find it impossible not to be satisfied by the overwhelming amount of concurrent evidence as to the real existence, under certain conditions, of the nuisances complained of." The foreshores in the reaches near the outfalls are covered with black, fœtid mud in a highly putrescible condition, just as was the case with the foreshore in the heart of the metropolis before the construction of the intercepting sewers or of the embankments; they also report "that the fish have disappeared from this point of the river, and that their disappearance is due either directly or indirectly to the sewage discharge." They further report that the evils of the present system of discharging the metropolitan sewage are decidedly such as to require a remedy, and that the public interests require that such a remedy should be applied with the least possible delay.

Before proceeding further, it may be remarked that the report of the Royal Commission, just issued, is a model report, in so far that it is drawn up with great skill and care, and forms the most valuable survey of the state of the question on water-carried sewage disposal which has ever yet been issued.

In order to appreciate the effect of turning in the large mass of sewage into the river in a concentrated form, it is necessary to note the influences exercised by the sewage upon the river. The report of the Royal Commission shows that although the outfalls are at Barking and Crossness, and the sewage may be assumed to be turned into the river only on the ebb tide, yet the float experiments showed that at whatever time of the tide the sewage is discharged, some of it may, under certain conditions, be carried up by the tidal oscillation alone into the heart of the metropolis, and even further. The report states that, in fact, the chemical analyses show that there is a progressive increasing impurity of the river from Teddington downwards to the outfalls, and then a decreasing impurity to Gravesend, beyond which place the sewage is not perceptible. As regards the impurity in the metropolis above the outfalls, it must, however, be recollected that on most rainy days sewage is allowed to flow through the storm overflows into the river in the metropolis, and, moreover, sewage also passes into the Thames in the tidal estuary above the metropolitan area. The oxygen dissolved in the water exhibits a corresponding decrease as the sewage increases, and *vice versa*, which shows that the oxygen does active work in oxidizing, and thus purifying the river from sewage impurity; in addition to the effect of

oxygen, purification is assisted by animal and vegetable life. Thus, the sequence may be summed up as, first, pollution by sewage; second, oxidation of the sewage; third, consumption of the sewage by minute animals, by the re-oxygenation of the river by means of animal and vegetable life, and by renewed absorption of oxygen from the air, which is favored by the movement of the water by tide and wind.

The larger the volume of tidal water in proportion to the sewage, the more rapid is the effect of these processes. An interesting series of experiments which Dr. Tidy made in 1878 and 1879 corroborates this view. They are recorded in a paper read before the Chemical Society in May, 1880. He mentioned one case where a liquid, containing one part of sewage to twelve of water, flowed a distance of one mile in nine hours; the oxygen required to oxidize the organic matter was .538 grains before the experiment, as compared with .187 grains after the experiment, showing as the result of one mile flow a diminution of .351 grs. of the oxygen required. The organic carbon, organic nitrogen, and chlorine were also materially diminished. Dr. Tidy recently remarked upon these experiments, "I am certain that, given a dilution of one-seventh sewage and six-sevenths fresh water (fully aerated, *i. e.*, containing two cubic inches of oxygen per gallon), with a flow of two miles per hour, not a trace of noxious matter would be found at a distance of five miles." It is worthy of remark that, at the places suggested by the referees for the discharge of the sewage, viz., opposite Mucking Lighthouse, the low-water area of the river is 191,000 square feet, and the high-water area 304,000 square feet; moreover, the sewage, as proposed to be diluted by the referees, would, after its course of twenty miles along the outfall channels at a rate of nearly two miles per hour, have reached the river in a condition much more favorable for dispersion than is the crude sewage turned in at Barking and Erith. And there can be no doubt but that if the suggestion of the referees had been adopted, the question of the pollution of the Thames by the metropolitan sewage discharge would not have arisen in the present generation.

In proposing a remedy, the Royal Commission accept the general principles which have guided the design of the metropolitan sewage system. They recognize the difficulties which have been felt of separating the rainfall from the sewage in the metropolis, difficulties which have been equally felt by all the various authorities who have hitherto been consulted on the subject. They state that it is neither necessary nor justifiable to discharge the sewage in a crude state into any part of the Thames. In considering how it should be dealt with, the Commissioners received a large amount of evidence on the different methods of sewage purification, as, for instance, treatment with lime, with perchloride of iron, by means of the A B C process, the filtration through limited areas of land, broad irrigation, &c. Col. Jones suggested that the sewage should be conveyed to Canvey Island, and then allowed to deposit in shallow cuts without the use of chemicals, the liquid, when clear, being run into the river.

There are two points connected with the London sewage which do not seem to have been sufficiently taken into account in many of these proposals. The first is that the time which necessarily elapses before the sewage reaches the outfall precludes it from being in so favorable a condition for treatment or utilization as fresh sewage would be. The second, that the degree of purity which should be required in an effluent depends on the degree of purity of the water into which it flows. The first of these, viz., the condition of the sewage when it reaches the outfall, has an important bearing upon the extent to which treatment with lime will prove advantageous, for in proportion to the freshness of the sewage is the efficiency of the lime process. The degree of freshness of character of the sewage has also an important bearing upon the utilization of the sewage on land, and the prospect of deriving profit from the constituents. This part of the question has not, indeed, advanced much during the last twenty-five years; the remarks of Messrs. Hoffmann and Witt, in their report to the referees, are nearly as applicable to-day as they were in 1857, viz., "notwithstanding the variety of patents which have been taken out, the problem of recovering profitably the valuable constituents of sewage re-

mains unsolved. The valuable constituents of sewage are like the gold in the sands of the Rhine, its aggregate value must be immense, but no one has succeeded in raising the treasure."

After weighing very carefully the evidence which they received on this part of the question, the conclusion at which the Commission arrive is practically the same that has been arrived at in previous inquiries, viz., that the metropolitan sewage had best be got rid of at the smallest cost compatible with efficiency. The Commission state that the suspended solid matters in the sewage are the chief causes of nuisance, and that by precipitation the suspended matter may be almost entirely removed, and the tendency to deposit largely lessened; but that the result of discharging an effluent alkalized by lime into the river at the present outfalls is problematical; they think, however, that lime would probably be as good as any other chemical for purposes of precipitation. The Commission add—

1. That a process of precipitation would effect an improvement on the present state of things. It would lessen the tendency to deposit foul banks and shoals.

2. That precipitation alone would not finally purify the river; nuisance would still occur in dry weather. That the injury to fish and danger to wells would still remain.

3. That the precipitation works themselves might be carried on without sensible nuisance.

4. That the cost of the precipitation would be at least £200,000 a year, or 1s. per head of the population.

5. That it would result practically in the loss of a large part of the manurial value of the sewage, offering no prospect of future realization, except by applying the clarified liquid to land.

For these reasons, and apparently also because a precipitation process could be brought into use more quickly than any other remedy, and if disused would entail a comparatively small loss of capital, they conclude that some process of deposition or precipitation should be applied to the London sewage at the present outfalls. The sludge would be either applied to the raising of low-lying land (it is to be hoped not to serve for the foundations of future dwellings); or burnt, or dug into land, or carried away to sea. The liquid portion would be, as a temporary measure, allowed to escape into the river. The report goes on to say that, as a permanent measure, this liquid must be further purified by being passed through land, or else must be carried down to Hole Haven.

According to the experience of Birmingham, which is not a water-closeted town (that is to say, only one-eighth of the houses have water-closets), the amount of land necessary for the metropolitan sewage, after treatment with lime, would be 6,000 acres, and the cost of the capital outlay for land and necessary works for the metropolis, upon the basis of the Birmingham expenditure, would apparently be fully £1,500,000. Mr. Bailey Denton states, however, in his most recent publication, that one acre of suitable land properly prepared would purify the clarified sewage of 2,000 persons; upon that basis at the present time the clarified sewage of the metropolis would require about 2,000 acres of land. The preparation of the land varies in cost, according to Mr. Bailey Denton, from £30 to £150 an acre. The Commission estimate the annual expenditure for precipitation by lime alone, on the present population, at £200,000 a year, which represents a capital sum of about £6,000,000, at 3½ per cent.; or assuming that a rental could be got for the land of from £6 to £8 per acre per annum, it would still stand against the metropolitan ratepayers at £4,500,000. This estimate, however, appears to be based only upon the present dry weather flow of about 23,000,000 cubic feet of sewage for twenty-four hours. But in times of rain the sewers are capable of bringing down more than three times that quantity to the reservoirs, the excess would apparently flow direct into the river, and this probably on about one day in three. It may be observed, in passing, that whilst the dry weather sewage contains on an average 23.36 grains of solid matter per gallon, of which 9.44 is mineral, and 13.92 is organic matter; in wet weather, when rain is intermittent, the sewage may contain from twice to five times this quantity of solid matter, of

which no doubt the larger part is mineral, but the organic matter is also very largely increased. In continuous wet weather, on the other hand, the sewage may become abnormally weak. The recommendations of the Royal Commission will not, therefore, deal thoroughly with the question, as in wet weather a large quantity of sewage will still pass in a crude form into the river. Moreover, according to the rate of increase of the metropolis, the sewage will amount to 35,000,000 cubic feet in little over twenty years. The cost of settling beds and land for purification, as well as the annual cost of the purification, will have to be increased in proportion, and, therefore, the question may be fairly raised whether it will not be a serious waste of money to adopt so expensive a palliative. And if the final result of the deodorization and filtration through land is to return a portion only of the effluent purified to the river, at a cost equivalent at the present time to a capital outlay of of £4,500,000, which will amount up at no distant date, with the increase of population and consequent sewage, to £6,000,000, in addition to the immediate expenditure of probably £1,500,000, for land and works, might it not be simpler and cheaper, even now, to adopt the plan of modified deodorization suggested by the referees, viz., dilution of the sewage, combined with its flow through many miles of long tidal channels, at a cost of two and a-quarter millions or less.

It will be apparent that one of the principal difficulties of the sewage question in the metropolis arises from the concentration of so vast a quantity of sewage, which is carried down to the outfalls and turned into the river at two points near each other. Sir Joseph Bazalgette, in his evidence before the Royal Commission, contemplates an extension of this amount of sewage, by bringing into one scheme the sewage from the valley of the Lea up to Hertford, and the Thames valley sewage, with its tributaries, from Leatherhead, Epsom, Ewell, Cheam and Sutton.

There is no doubt that the question of the disposal of the sewage of the metropolitan area is only one part of the subject, and that the whole question of the disposal of the sewage of the valleys of the rivers Thames and Lea requires to be taken into account and dealt with in a comprehensive manner. And this subject must daily increase in importance as inducements are held out to the working population of London to reside in more airy localities outside London; and to come up daily to their work; for it is certain that, unless this question of the disposal of the refuse water of these outlying districts is taken in hand earnestly and zealously, the community will in a few years find itself in a much more difficult position than it is now placed in by the question of the metropolitan sewage alone.

It is clearly not advisable to allow the sewage from outlying districts to flow through the heart of the population of London. The referees, in their report, especially alluded to this point. They said: "It is desirable, as far as possible, to prevent the sewage from flowing through the thickly inhabited districts of the town;" and so strongly were they impressed with this view that they recommended "that the low-level sewage west of Somerset House should be carried back to opposite Battersea, and then across the river, to be there raised into a southern high-level sewer," passing through a comparatively sparsely inhabited district, instead of passing as it now does, through the most densely populated part of London.

This brings us to the question as to whether the limits of concentration have not been fully reached, so far as the metropolis and its subsidiary districts are concerned. Indeed, it could be argued, with some show of reason, that it might have been advisable to have adopted, in some parts of the sparsely inhabited western districts of the metropolis, in a modified form, the separation of sewage and rainfall, and possibly to have refrained from pumping twice over the sewage of the districts of the western portion of the metropolitan area, in order to convey it through the heart of the more populous parts of London to Barking or Crossness; and instead of this to have resorted to some form of purification. In the present state of the question, this is only a reflection which occurs as to what might have been best with our present experience if the field were clear.

But with respect to the sewage of districts outside the metropolis, the subject is largely one where future action is less fettered by former proceedings. No doubt, even if it were admitted as an axiom that sewage of outlying populations should not be allowed to flow through densely inhabited districts, yet it is possible that sewers might be arranged to carry the sewage from the places in question to the sea. But is this necessary, or is it desirable? If sewage from places in the Thames Valley above the metropolis, or from the valley of the Lea, is required to be taken to the sea, where are we to stop? The difficulties of the sewage question arise from concentration, and it is therefore a much more rational solution to give up the idea of concentration, and to require each district to make arrangements for the disposal of its own sewage.

If a population concentrates itself on a limited area, it must make arrangements for the wants entailed upon it by that concentration. For instance, it must provide streets to give access to the houses; it must provide open spaces in which to marshal the railway trains which bring in the food or other articles which minister to the daily wants of the population. It must provide gathering grounds for its water supply, parks for recreation, and open spaces in which to bury its dead. Similarly it is equally necessary that every nucleus of population should provide open spaces on which to purify its sewage without being offensive to the neighboring houses.

The report of the Royal Commission makes it abundantly clear that whilst profit must not be expected from sewage utilization, yet that precipitation and utilization are eminently fitted, when properly applied to produce a purified effluent; and, therefore, that were certain conditions of population and of sewage always observed, each district could be made self-contained in respect to its sewage just as it can be in respect to its cemetery.

1. The conditions of population are that the district should be limited in numbers and in the area occupied.

2. The conditions of the sewage are—(*a*) the extent to which the sewage can be separated from the rainfall; (*b*) the degree of freshness of the sewage, as received at the place where it is treated.

In the case of the metropolis it may be accepted that the removal of sewage and rainfall was a necessity, and in this view all the practical authorities who have considered the subject appear to concur. The reasons for combining sewage and rainfall are not always equally strong, and in many cases the strength of the argument is against combination. But no absolute general law can be laid down that sewage should invariably be separated from rainfall. On this question each locality must be governed by the circumstances of the case; but there can be no question that the problem of sewage disposal would be simplified almost in direct proportion to the extent to which the separation of sewage from rainfall can be carried with prudence. The difficulties of separation lie in the numerous foul surfaces which prevail in towns, and especially in streets of large traffic, in which, in proportion as the road surfaces are rendered smooth and impervious, so does the mud and the dust appear to consist chiefly of horse manure, and this could only be prevented by the adoption of a much more perfect system of street cleansing than prevails at present.

The importance of the report of the Royal Commission lies not so much in what it recommends for the metropolis, as in the valuable information which it has collected on the present state of the general question of sewage disposal—information which is applicable to the wants of the whole country. The comprehensive manner in which the subject has been treated is of especial value at the present time, because the country is becoming too closely built over for this question to be allowed to remain any longer in the *laissez faire* condition which it has hitherto occupied, if regard is to be had to the purity of the air, the purity of the soil, or the purity of the rivers and watercourses.

Tracing paper may be made by immersing best tissue paper in a bath composed of turpentine and bleached beeswax. A piece of beeswax about an inch in diameter, dissolved in half a pint of turpentine, is said to give good results. The paper should dry two or three days before use.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—FEBRUARY 18TH, 1885.—Vice-President G. S. Greene, Jr., in the chair. Discussion on the paper by E. Sweet, M. Am. Soc. C. E., on the Radical Enlargement of the Artificial Waterway between the Lakes and the Hudson River was continued.

Mr. E. S. Chesbrough, Past President Am. Soc. C. E., by letter, considered it advisable to make the surveys and estimates recommended by Mr. Sweet.

Mr. Walton W. Evans, M. Am. Soc. C. E., said that when speed was not brought in as an element, no railway can stand in competition with a well constructed water line. Statistics show the influence of the Erie Canal on railway freight charges. The best policy for the State of New York will be to enlarge and improve this waterway. He referred to the fact that Gouverneur Morris was the projector of the Erie Canal. He suggested the possible use of elevators instead of locks, and the use of preserved wood in many places instead of stone.

Mr. T. C. Clarke, M. Am. Soc. C. E., said that the cost of repairs and the interest on the amount to be expended in making the proposed improvement would be very large. The canal would be shut half the year by ice. The railroads are always ready. Hence the fact that the canal carries so small a portion of the freight.

Mr. N. M. Edwards, M. Am. Soc. C. E., estimates that with the proposed waterway wheat could be carried from the head of the lakes to New York City for 5 cents a bushel, with 1 or 2 cents added if tolls are required. He compared the possible rates by various routes, and considered that the advantages of the proposed enlargement should command recognition.

Col Wm. E. Merrill, M. Am. Soc. C. E., considers the chief merit of the project to be, that it will reduce the cost of wheat to a theoretical minimum; the benefit of such a radical cheapening of the chief article of human food is beyond calculation. He considers the project entirely practicable. The current would be somewhat objectionable, but in favor of the heavy traffic. He suggests fixed dams on the Mohawk rather than movable dams. The great military advantages of such a ship canal should not be overlooked.

Mr. John D. Van Buren, Jr., M. Am. Soc. C. E., compared two routes for a ship canal within the State of New York and two which would be international—those from Chicago to New York, by Buffalo and the Erie Canal, by the Welland Canal (or a similar canal on the State side) and Oswego, by the St. Lawrence and proposed Caughnawaga Canal and by the proposed Ottawa and Caughnawaga Canals.

The Oswego route is deficient in water. The Ottawa route is a day shorter in time than the Erie. It is an important question whether the advantages of the international route would outweigh the importance of having the route entirely in the State.

Basing an approximate estimate upon the cost of the present Erie Canal, Mr. Van Buren makes a grand total of $194,000,000 as the probable cost of the proposed Erie Canal route, which, with interest during ten years of construction, would amount to $240,000,000. This is about three times the estimated cost of the Nicaragua Canal, and more than three times the actual cost of the Suez Canal.

The true test for a proposed government or private work is the commercial one, Will it pay? Unless the cost of transportation would be materially reduced below the cheapest present normal rates the enterprise would prove a failure and no benefit. Taking a probable interest charge and an annual expense for repairs and management at the same sum as that expended on the Suez Canal, and a total tonnage of 20,000,000, Mr. Van Buren estimates the charge for the support of the canal at 3 cents per bushel of wheat from Buffalo to New York. Looking at the matter as a private interprise, the conclusion is reached that a private company would hardly dare undertake the work.

Considered as an undertaking by the general Government, the question is, What interest, under the Constitution, has the Federal Government in such a project? It would be difficult to induce Congress to appropriate funds for an improvement likely to seriously damage the interests of neighboring States.

Considered as a State enterprise, it must be as a free canal. The general benefits would be those attending a great increase of business. The people of the State would have to pay about $16,000,000 per annum.

Mr. D. Farrand Henry, M. Am. Soc. C. E., said that the question was not solely how to reach New York, but how to reach Liverpool. Three routes have been proposed: from Chicago to the Mississippi by the Illinois River; the enlargement of the Erie Canal; the deepening of the Welland Canal, and the improvement of the St. Lawrence. Comparing these routes, the conclusion is reached that the latter is decidedly the most feasible.

Mr. O. Chanute, M. Am. Soc. C. E., suggested that the question should be considered whether it may not be practicable to apply some form of heat engine, different from any heretofore tried, for the propulsion of the boats on the present canal, and thus secure economy and speed and large carrying capacity for the canal, as compared with the great interest charges on the waterway proposed by Mr. Sweet.

Mr. C. Herschel, M. Am. Soc. C. E., discussed the regulation of railroad freight rates by the encouragement or the creation of water competition, rather than by a reliance wholly upon the work of railroad commissioners, or upon the successful enforcement of rigid statutes designed to forbid excessive and discriminating freight rates, under pains and penalties.

Mr. Theodore Cooper, M. Am. Soc. C. E., did not consider that freight could be taken economically by the same vessel from Chicago or Duluth to Liverpool. The vessel constructed properly for ocean navigation would not be suitable for the canal or the lake. The vessel constructed to carry freight economically on the lakes is not fit for ocean voyages.

Mr. J. Nelson Tubbs, M. Am. Soc. C. E., by letter, expressed doubt of suitable provision for water supply on the long levels of the proposed canal, as the evaporation and percolation would be very great.

Mr. E. Sweet, M. Am. Soc. C. E., considered the various points that had been brought forward in the discussion. He gave figures to show that the losses by percolation and evaporation could be supplied from Lake Erie by a current of about six-tenths of a mile per hour. This current would increase going eastward till reaching the long level, across which it would not exceed three-tenths of a mile per hour. It would be in the direction of the heavy traffic and a benefit rather than a hindrance to navigation. He stated that the estimates of Mr. Van Buren proceed from erroneous premises as they assume the cost of the present canal at about $49,000,000, which is fifty per cent. more than its actual cost as shown by the State records. These show that the enlargement between 1835 and 1862 cost $31,834,000. The structures of the original canal were discarded in the enlargement, and its route was almost as generally disregarded as the route of the present canal is in the proposed ship canal. The cost of the original canal should not, therefore, be considered in such an estimate. But, even adding this to the cost of enlargement, and all the interest of all the loans for constructing and enlarging the canal, the sum assumed by Mr. Van Buren is not reached. Taking the actual cost and applying the principles he adopts, the estimate becomes $127,000,000. Three per cent. interest charges are more probable than the five per cent. assumed by Mr. Van Buren. The interest charge would thus be $3,810,000 instead of $12,000,000. He also estimates maintenance too high.

The suggestions favoring the adoption of the St. Lawrence route, apart from the questions of State and National interest and patriotism, simply would by that location aid foreign commerce and defeat the chief object of the proposed improvement which is to facilitate the vast domestic traffic between the East and the West. Its tonnage is now many times that of the foreign commerce tributary to either route, and the disparity will surely increase with the facilities proposed.

The suggestions of Mr. Chanute would not result in the speed desired. The essential defect of small canals is that the boats must have too large an immersed surface for the tonnage they carry, and thus enormously larger resistance per ton than large vessels.

Replying to Mr. Corthell, Mr. Sweet said that while the railroad is the most important instrument of internal commerce, it has limitations of capacity and economy. The limitations of economical water carriage are quite different. The economy of water transit increases with the size of the vessel, and the capacity of artificial waterways increases nearly as the cube of their depth of channel.

Mr. Sweet also showed that a speed of five miles per hour could easily be realized by large vessels in the proposed canal from Buffalo to Utica, and of ten miles per hour from Utica to New York. This, with proper allowance for detentions at locks, would give a duration of voyage from Buffalo to New York of 85 hours.

March 4th, 1885.—President F. Graff in the Chair.—The vote on the proposed amendment to the Constitution was canvassed as follows:

For the amendment.............	181
Against the amendment..........	28
Blank votes....	3
Ballot not endorsed..............	1
Total votes received.........	213

This proposed amendment, having received an affirmative vote of two-thirds of all the ballots cast, was declared duly adopted as an amendment to the Constitution of the Society. It read as follows:

Add at end of Article XXII:

Any member of the Society, not in arrears for dues, may compound for the payment of all future annual dues, except as hereinafter provided, by the payment of two hundred and fifty dollars.

Provided, That all resident members, or those who may hereafter become such, shall be and remain liable to the annual payment of the difference between the annual dues of resident and non-resident members, as the same now is, or may be established from time to time; but any member may at any time compound for the future payment of all annual dues of every nature and kind, by the payment of seventy-five dollars in addition to the two hundred and fifty dollars hereinbefore provided for.

Provided, however, that each person duly elected a member shall pay the entrance fee and also the annual dues for the current year of his election.

Provided, also, that any member desiring to compound for future annual dues shall have paid the annual dues for the current year before the compounding sum may be available.

Members compounding shall sign an agreement that they will be governed by the Constitution and By-Laws of the Society as they are now formed, or as they may be hereafter altered, amended or enlarged; and that in case of their ceasing to be members from any cause whatever, the amount theretofore paid by them for compounding, and for entrance fees and annual dues, shall be the property of the Society.

All moneys thus paid in commutation of annual dues shall be invested as a permanent fund, the interest thereof only being subject to appropriation for current expenses.

Engineers' Club of Philadelphia—February 21st, 1885.—President J. J. de Kinder in the chair.

The Secretary presented, for Mr. Henry A. Vezin, a set of Diagrams for Determining Belts and Pulleys and Shafts, with a description thereof. They were derived from Wieber's Skizzenbuch fur den Ingenieur, but modified by Mr. Vezin to suit our units of weight and measure, and their application extended. They present the usual advantages of the graphic method; not only saving the time and mental exertion necessary to calculate by formula, but also presenting a picture of possible and desirable modifications of design.

He also presented similar Diagrams for Cast Iron Cogs.

Mr. J. Milton Titlow contributed a paper, illustrated by drawings and photographs, upon the Strengthening the West Main Abutment of Chestnut Street Bridge, Philadelphia.

The bridge is 42 ft. wide, with two cast iron arched river spans of 185 ft. each, supported by heavy abutments on the shores: on the west are two segmental brick arched spans, of 60 and 58 ft., to two long retaining walls, holding the earth supporting the roadway.

The whole of this west approach is located upon what was called the river flats, partly occupied by wharves and docks, and resting piles from 24 to 40 ft. in length, driven through a stratum of river mud or silt from 16 to 20 ft. in thickness, to the coarse gravel with boulders and cobble-stones lying upon the bed rock.

The masonry was practically completed in 1865, about a year before the cast iron river spans; and any movement of the main abutment, by reason of its maintaining its verticality, transferred a horizontal thrust to and through that part of the approach in the rear, shown by a depression of the river spans, a shortening of and a rising of the crowns of the arches in the rear of the main abutment, and a closing of the joints of the copings on spandrel and retaining walls, as well as pushing back the small approach abutment at the springing line between the retaining walls. This rising of the west arch became very apparent in 1879, when horizontal wooden struts were placed above the platforms of foundations for the purpose of taking up and transferring this horizontal thrust to the retaining walls, making them act as buttresses temporarily until a plan could be determined upon, which should be effective in itself.

Of the several designs suggested for cylindrical iron buttresses, that of J. F. Anderson was adopted, having the great advantage of facility of construction in the small space at our disposal, the archway being occupied by a double track railroad and two sidings, to remove which would probably equal one-half the cost of intended work. This design, as executed, consisted of placing, by means of compressed air, four wrought iron cylindrical buttresses 8 ft. in diameter, filled with concrete, inclined at 45° and parallel with axis of bridge. They are made up of half-inch wrought iron plates, about 2 ft. by 8 ft., the joints being parallel and square with the axis of cylinder, and connected by 3½ in. angle irons riveted around edges of plates and bolted together when in position, forming rings 2 ft. in length and breaking longitudinal joints.

At the upper end of a cylinder a connecting shaft was built, 4 ft. in diameter, extended to the surface, upon which was placed the air lock, with the lower end cut, and flanged to the required direction of cylinder. Parts of two rings were always kept in advance of a completed ring, so that the lower edges approximated a horizontal, holding out the water more readily. After they had been advanced to, and a step made into, the rock, the cylinder was partly filled with concrete composed of one part best German Portland cement, two parts bar sand and four parts broken stone. Two shoulders were then cut into the base of the abutment, and the cylinder completed by extending it up to the abutment and filling with concrete.

A double air compressor, with 10 in. steam and air cylinders, made by De Lematre & Co., N. Y., was used with the Edison system of electric light. Work was commenced October 9th, 1884, and completed February 24th, 1885.

Prof. Haupt did not wish to appear to criticise adversely the conclusions of the writer, but he felt disappointed that the paper did not contain some data, which would support the theory which led to the use of the piles in their present position. This misconception of the problem arises apparently from a failure to distinguish which of the equal and opposing forces is the action and which the reaction, or which the power and which the resistance. If the damage to the west abutment were caused by the *thrust of the iron arches*, as alleged, then its effect would be first manifested on the abutment pier and the adjoining brick arch, before reaching the extreme or land side of the second masonry arch. The writer states that this wall has been thrust *back*, whilst his measurements show that the span has been *reduced* in some places nearly half a foot, and that the reduction of span is greatest at the springing line near the head walls, and also near the ground. He admits that the main abutment-pier has *not* moved, nor has the one to the west of it, between the two masonry arches, as the eastern span of 60 ft. is not affected. The span of the second arch could only be *reduced*, therefore, by its abutment-wall moving *forward*.

Prof. Haupt believed that the defects of the bridge resulted from a slight settlement on springing of the piles under the corners of the foundation of the abutment and approach walls, thus reducing the frictional resistance of the masonry on the grillage, and permitting the excessive pressure of the earth filling, especially when saturated with water, to overcome the inertia of the masonry in these walls, and break the bond, which, in some places, was very weak, causing the large cracks in the approaches and the first arch. He suggested that the remedy should have been applied originally at this point by the introduction of screw or disc piles around the outside of the abutment, by which the unit pressure might have been reduced to any desired extent. The present tubes are being made to abut against a part of the foundation which is admittedly rigid, are placed at some 120 ft. from the weak point, and in such a direction as to oppose no resistance to this thrust.

There is nothing in the paper relative to the effect produced by the former remedies of heavy sills and ties, nor to show that they were so inefficient as to render this last device a necessity in any position.

Mr. Howard Murphy thought that these "heavy sills" should never have been built. They are, in reality, built-up, horizontal struts or columns, connecting the substructures, and were intended to relieve the main abutment of

a portion of the horizontal thrust of the west iron river arch, by conveying it to the approach abutment through the intermediate pier. That they would, if brought to firm end bearings, have increased the stability of the structure, whichever way it has a tendency to move, there is no doubt, if these were all the conditions to be considered. But the Thirtieth Street Extension of the Pennsylvania Railroad passes under one of the arches. The traffic is entirely freight. Ths heaviest engines and trains may be frequently stopped and started under the bridge, as these tracks are practically a portion of the yard. The struts were laid but a short distance under the surface. If loose, they did no good. If tight, they afforded a very convenient rigid medium for the transmission to the bridge foundations of every shock and hammer blow incident to heavy railroad traffic. The often observed effect of light machinery upon the masonry of buildings, would seem to indicate that a bridge which could stand this kind of thing without serious rupture was not such a bad bridge after all.

As to whether or not the original designs for the foundations of Chestnut Street Bridge had been faithfully carried out by the contractor, Mr. Murphy stated that any assumption of careless *supervision* of the contractors was absolutely inadmissible.

He differed with Prof. Haupt as to the easterly or riverward movement of the structure, because there seems to be no cracks or changes which *clearly* show this, and, on general principles, because a pier is not likely to move under the thrust of a short high arch against the thrust of a long, flat arch; and because a failing retaining wall is not likely to bulge, horizontally, *towards* the retained material, particularly when tied at its ends by walls running at right angles to it, and against which latter walls there is a greater thrust of arch, owing to some excess of weight, of the spandrels and parapets, over the earth-filling in the middle. Comparison with the accurate transit points, entirely external to the structure, which Prof. Haupt has located, may, however, reveal changes, in one or more directions, now indeterminable.

PROCEEDINGS OF THE ENGINEERS' CLUB OF ST. LOUIS.—ST. LOUIS, MARCH 4TH, 1885.—The Club was called to order at 8:15 P. M. by President Moore.

The Executive Committee recommended that Mr. Henry B. Wood be elected a member of the Club, being balloted for he was declared unanimously elected.

The next order of business was the reading of a paper on "Treatment of Wood for Street Pavements," by Messrs. Caldwell and Miller. It was discussed by Messrs. Constable, Johnson, Robt. Moore, H. C. Moore and Lansden. Mr. Theo. Plate, President American Wood Preserving Company being present, was called on to give his views. He expressed it at his opinion that the idea, that gum wood was a cheap wood, was a mistake, because when all heart wood was required it necessitated more work, and, consequently, greater expense in securing it has heretofore been anticipated.

Mr. Lansden exhibited a section of water-pipe from Falls River, Mass., which had lain about eighteen months in a bed of cinders, where it was submerged in tide water twice in twenty-four hours. It had disintegrated two-thirds of the way through, and leaving a substance as soft as graphite, the pitch coating of the pipe still being plainly visible both inside and out.

ENGINEERING NOTES.

CANTILEVER BRIDGE AT NIAGARA FALLS.—The preliminary study and estimate for this double-track railroad bridge was made at the request of the Central Bridge Company of Buffalo, by Mr. Schneider, in October, 1882, and he at that time concluded that the river span should be so designed as to allow it to be erected without false works, and that a hinged arch on the cantilever principle would be the proper form of construction, he having in the spring of 1882 designed the Fraser River Bridge for the Canadian Pacific Railway, where similar conditions existed, precluding the use of false works. After securing a profile of the site, the design and estimates were perfected, and a tender for the construction of the entire work submitted by the Central Bridge Company to the Niagara River Bridge Company, and after approval by the consulting engineer, Mr. Charles H. Fisher, M. Am. Soc. C. E., the contract was awarded on April 11th, 1883, to the Central Bridge Company, on condition that the structure be completed on December 1st of the same year. The contracts for portions of the work were sublet; excavations and masonry to Dawson, Simmes & Mitchell, false works to C. H. Turner, Beton foundations to John C. Goodridge, Jr., steel and iron compression members for towers to Kellog & Maurice.

Mr. Schneider was appointed, by the Niagara River Bridge Company, Chief Engineer, on April 26th. The work, both at the bridge site and the shops, was vigorously pushed, and the bridge was completed and opened for traffic on December 20th, 1883, about eight months from the commencement of the work.

The bridge is over the Niagara River, about two miles below the Falls and 300 feet above the Railroad Suspension Bridge. The bridge spans a chasm of 850 feet in width, and 210 feet in depth to the surface of the water. The river is 425 feet wide at the bridge site; the water has a velocity of 16½ miles per hour at the center of the river. The depth is supposed to be from 50 to 80 feet. The banks on both sides slope at about 45 degress from the water's edge to about 50 feet below the top of the cliff, above which they are horizontal. The sloping banks consist of a mass of large boulders and broken stone from the hard limestone layer which forms the upper stratum, mixed with earth and *debris*. This hard limestone, which has been undermined by the water acting upon it and cutting away the argillaceous rocks, has fallen in hard masses and formed a natural rip-rap, preventing further erosion. The pits for towers at the water's edge were in this loose, hard stone. No solid rock was found. It was de-

termined to prepare the bed for the masonry upon the very large boulders at the bottom of the pits by filling to a depth of about 8 feet with Beton Coignet, well rammed into all interstices on bottom and sides. The foundation area for each pair of piers is about 1,000 square feet, and the weight about 5,930,000 pounds, or 5,930 pounds per square foot. The stability of this foundation was considered by two commissions of engineers, both of which commissions expressed themselves satisfied as to to their stability. The members of these commissions were Messrs. George S. Morrison, Charles Macdonald, John A. Wilson, A. W. Stedman, and Theodore Cooper.

The masonry of the piers is limestone laid in cement. They are 38 feet high above the beton. The stones were lowered from the trestle, afterwards used for the erection of the towers and shore arms of the cantilevers. The anchorage piers are on the top of the cliffs. They are built on a platform of plate girders, connected by anchor bars to the shore ends of the cantilevers, thus utilizing the weight of the whole mass of masonry of the piers. Each anchorage pier weighs about 2,000,000 pounds.

The towers are 132½ feet from top of masonry to center of lower chord of cantilevers. Each tower consists of four main posts, with horizontal struts and diagonal tie-rods. Each post is formed of steel plates and angles. The horizontal struts divide the towers into five sections of nearly equal height. The posts have a batter of 1 in 8 at right angles, and of 1 in 48 parallel to the axis of the bridge. The distance between centers of posts at their base is 60 feet 7¼ inches at right angles, and 30 feet 5⅛ inches parallel to the axis of the bridge. They rest on cast-iron shoes at the bottom. The tops consist of steel castings, which support the cantilever on 7½-inch steel pins.

The structure carries a double track. It consists of two cantilevers resting on the towers, the shore ends being anchored to the anchorage piers, and the river ends connected by an intermediate span. The distance between centers of anchorage piers is 910 feet 2½ inches; length of each cantilever, 395 feet $2\frac{5}{16}$ inches; length of intermediate span, 119 feet 9⅞ inches. The moving load assumed in proportioning the structure was a train on each track, headed by two 66-ton locomotives, having 72,000 pounds on three pairs of drivers, spaced 6 feet between centers, followed by a train load of 2,000 pounds per lineal foot. The floor system is proportioned for 78-ton consolidation engines. The lateral system is proportioned to resist a wind pressure of 30 pounds per square foot on a train surface of 10 feet high, and upon the exposed surface of truss and floor system; the pressure on the train surface being considered a moving load. Strain sheets accompany the paper.

The tower posts, lower chords, center and end posts of cantilevers, pins, top-castings for towers are of steel. All the other parts are of wrought iron, except the shoes for tower posts, filling rings, washers and hand-rail posts, which are of cast iron.

Each cantilever consists of a shore arm 195 feet $2\frac{5}{16}$ inches long, one panel 25 feet over the tower and a river arm of 175 feet length. The cantilever trusses are divided by vertical posts into panels of 25 feet, with the exception of the end panel of the shore arm, which is 20 feet $2\frac{5}{16}$ inches; they have a double system of diagonals and are spaced 28 feet between centers; they are 56 feet deep over the towers, 26 feet over the last vertical post at the river end; and 21 feet over the last vertical post at the shore end.

The upper chords of the shore arm receive alternate tensile and compressive strains, loads which are applied between the anchorage and the tower produce compression, and those applied to the river arm or intermediate span produce tension. These chords are of eye-bars, with a compression member of plates and angles, double latticed, packed between the bars.

The upper chord in tower panel and the upper chord of river arm are composed entirely of eye-bars. The lower chords and inclined end posts of the cantilevers are steel compression members of plates and angles. The vertical posts over the tower supports are of steel, two plates and four angles. The intermediate vertical posts are of two channels double laced.

All the principal connections are made by steel pins 5⅝ inches, 6¾ inches and 7½ inches diameter.

The cantilevers are connected to the anchorage piers by rockers permitting horizontal movement.

The intermediate span has five panels of 24 feet.

The iron used was manufactured by Atkins Bros., of Pottstown, and Graff, Bennett & Co., of Pittsburgh. The steel was made by the Spang Iron and Steel Company, of Pittsburgh. The steel pins and castings were made by the Cambria Iron Company. All the materials were manufactured into finished members at the shops of the Central Bridge Works, at Buffalo, except the compression members for the towers, which were made at the shops of Kellogg & Maurice, at Athens, Pa.

The heads of the eye-bars were formed by die forging.

The towers were erected by means of derricks on the false works, the material being lowered from the cliffs to the floor of the false works and thence to the towers. The tower on the American side was begun August 29th and finished September 8th; that on the British side was begun September 10th and finished September 18th. The shore arms of the cantilevers were then erected on the false works, a track laid on them and the travelers for the construction of the river arms put in position. These were substantial wooden frameworks on iron wheels. The travelers were fastened by clamps to the floor-beams of the completed portion. Each traveler had two derricks connected with a hoisting engine, by means of which the materials were lifted from cars and lowered to their place in the structure. A hanging platform was suspended from the traveler.

The American shore arm was begun September 25th, completed October 15th. The Canadian shore arm was begun October 8th,

completed October 22. The American river arm was begun October 28th; the Canadian river arm was begun November 4th. The last connection was made November 22d. The first track was completed December 6th, on which day a locomotive crossed the bridge. The whole structure was completed December 19th.

The bridge was formally opened and tested in the presence of a large number of engineers on December 20th. The tests were conducted by a committee, Messrs. George S. Morison, Theodore Cooper, Charles Macdonald and Thomas Rideout. The inclemency of the weather at that date made another series of tests desirable, which were conducted by the same engineers, whose report accompanies the paper. The specifications for the bridge and the report of tests on eye-bars also accompany the paper—*Transactions of Am. Soc. C. E.*

IRON AND STEEL NOTES.

A New Process for Toughening Steel.—The French Société d'Encouragement have had under prolonged examination a process, invented by M. Clemandot, for working steel. This process is described by the *Revue Industrielle* as consisting in heating the metal until it acquires a sufficient ductility, and then subjecting it to a high pressure during cooling. In this way a modification of the structure of the metal is produced, and the material acquires properties analogous to those developed by tempering. Similar processes have been tried in France, but only upon the same principle—that is to say, by operating upon the metal while yet in the state of fusion. M. Clemandot, on the contrary, takes steel already made, heats it simply to a cherry red, and submits it, by means of a hydraulic press, to pressures of from 1000 to 3000 kilos per square centimeter. After having allowed the steel to cool between the two plates of the press, it is withdrawn with all its new qualities perfectly developed, and does not require any further treatment. The result of the process is to impart to the steel a fineness of grain, a degree of hardness, and a notable accession of strength to withstand rupture. This alteration is most considerable with highly carbonated steel; and in this respect the metal is made to resemble tempered steel, without being in all points identical with it. The cause of the alteration in physical condition is ascribed to the rapid heating and no less rapid cooling of the metal. When the red-hot steel is first strongly compressed, the conversion of the mechanical energy into heat serves to raise the temperature of the entire mass, at the same time that the particles of the metal are more closely cemented together. This effect is followed by a rapid cooling, due to the contact of the plates of the hydraulic press with the surfaces of the metal. The close pressure materially increases this conducting effect of the cold metal.

RAILWAY NOTES.

The locomotives of the German Railroad Union in 1882 consumed, among other fuels, 49,827 tons of peat. The Austro-Hungarian roads in the Union burned considerable wood—209,918 cubic metres, against 974,316 tons of lignite and 655,708 tons of coal. On the German roads 97 per cent. of the fuel used—reckoned by heating capacity—was coal; on Austro-Hungarian roads, only 50¾ per cent. The total consumed on all the Union roads was equivalent to 4,560,628 tons of coal.

ORDNANCE AND NAVAL.

The firing of H. M. S. Sultan at the forts at Inchkeith constituted a confidential experiment. Major O'Callaghan, R.A., was appointed to attend from the department of the Director of Artillery. Sufficient, however, has appeared in the *Standard* of Thursday, August 14th, to give a general idea of what occurred. Although the guns of the works were mounted *en barbette*, very little effect was produced at first by the machine guns at 1000 and 1500 yards, or by the heavy guns at from 1500 to 3500 yards range. Eventually, by a great expenditure of ammunition, the machine guns did considerable execution among the dummy detachments, but not so much as might have been expected. The heavy 12-inch guns with shrapnel, on the other hand, were found so destructive that the firing was discontinued without trying the power of common shell. As the *Standard* observes, in all this there was nothing revolutionary to our present system of organizing armaments. On the other hand, it rather indicated that we ought not to withdraw our confidence from our regular armaments of heavy guns and shrapnel, and depend on new weapons without abundant proof, for the work of firing at the *personnel* of an enemy.

BOOK NOTICES

Publications Received.

Sixteenth annual report of the (Mass.) Board of Railroad Commissioners, Boston: Wright & Potter.

Fifteenth Annual Report of the New Bedford Water Board. New Bedford: Mercury Pub. Co.

Professional Papers of the Corps of Royal Engineers. Vol. IX. London: Royal Engineers Institute.

Selected Papers of the Institution of Civil Engineers: The Steam Engine. By Edward Alfred Cowper, M. Inst. C. E. Independent Engine Tests. By John George Mair, M. Inst. C. E. Abstracts from Foreign Transactions and Journals.

Report on Gedney's Channel Congressional Document.

Cassell's Magazine of Art. New York: Cassell & Company.

Signal Service Notes. No. XIII. The Relation between Northers and Magnetic Disturbances at Havana, Cuba. By G. E. Curtis, Serg't.

No. XVII. A First Report upon Observations of Atmospheric Electricity at Baltimore. By Park Morrill. Washington: Signal Offices.

Reports of the Examiners of the Electrical Exhibition of 1884:

Section No. XIX. Electric Telegraphs.
No. XXIV. Electro Dental Apparatus.
No. XXVII. Applications of Electricity to Warfare.

PRINCIPLES, THEORY AND PRACTICE OF MATHEMATICAL COMMENSURATION. By CHARLES DE MEDIA, Ph. D. Chicago: A M. Flanagan.

THE FALLACY OF THE PRESENT THEORY OF SOUND. By HENRY A. MOTT, JR., Ph. D. New York: Printed for the author by John Wiley & Sons.

These two publications may, for the purpose of a brief review, be considered together, for the reason that they are alike in being late contributions to popular ignorance, and alike in a pernicious tendency to lead the unlearned to reject scientific teachings and to scorn logical methods of deduction.

Both writers belong to the class termed by De Morgan "Paradoxists." Both urgently demand a revision of theories, long since accepted as the best presented to account for observed facts.

The difficulty experienced by the first of these authors seems to lie in the multiplication table. After falling out with this useful and generally credited aid to computation, the attainment, by an original method, of the final object of his research, is direct and easy. This is only the squaring of the circle.

Of course he differs from all the other circle squarers.

A single specimen of the author's unique method of computation is afforded on page 17, where the side of a square being given at 8½, the diagonal is found to be *exactly* 12. Readers who cannot accept this conclusion had better stop at this point in the book.

The difficulty experienced by the author of the second book above-mentioned is not very unlike that encountered by the first, but the sentiment that prompted the preparation of the treatise is not quite the same.

In the first, the author has a theory to promulgate. The second book is only a tirade of fault-finding. The author discourses flippantly of the "mistake of Helmholtz," "the childish experiment" of Tyndall, etc., and offers in refutation of the theories of Tyndall, Lord Rayleigh, Mayer, Rood, Blaserna and Sir William Thomson, the opinions of Dr. Wilford Hall and Professor Carter!

A brief mention of the style of argument is all we have space for here, and is more than we should afford were it not for the fact that the author was permitted to deliver the treatise as a lecture before the New York Academy of Sciences.

Here are the examples: It is claimed that inasmuch as the real average velocity of the prong of a tuning fork is slow; that is, taking into account the distance through which it travels in a given time—the vibrations communicated to the air cannot be rapid enough to produce sound; "the stops and starts cannot produce them."

In another place under the heading of "The Physical Strength of the Locust," the author argues that inasmuch as the stridulations of a locust can be heard a mile, while the insect weighs less than a quarter of a pennyweight, that the strength of this insect must be sufficient to move four cubic miles of air as a mass. This is followed by a remarkable suppositious case, viz., if the above-mentioned insect (whose estimated weight by the way can only be that of a locust who has been for a long time "off his feed") were placed in the center of a cubic mile of iron, his stridulations would (admitting the wave theory) be capable of moving five thousand million tons. This, the author infers, must be taken by modern scientists (excepting Dr. Mott, Dr. Hall and Prof. Carter) to be a measure of the physical strength of the locust.

Leaving this last experiment out of the question, as rather unlikely to be verified, it is apparent from the statement regarding the other. that the author has very ill-defined ideas regarding molecular motion, and none at all of mechanical work.

The author declares that the results obtained in acoustic experiments are, upon the wave theory, incredible. We do not doubt they are to him. But this fact has no relation to acoustics.

INORGANIC CHEMISTRY. By EDWARD FRANKLIN, LL.D., and FRANCIS R. JAPP, Ph. D. London: J. & A. Churchill. Price $8.40.

As this is the latest treatise upon Inorganic Chemistry, it is presumably the best presentation of lately discovered facts. We judge from such examination as could be made in a limited time that the most important improvement over other late works will be found in the first 130 pages, which contain a good compend of the Principles of Chemical Philosophy. A chapter on Thermo-Chemistry is especially valuable.

Throughout the remainder of the 800 pages, the principles and facts of Elementary Chemistry are presented in a manner at once compact, clear, and as full as the ordinary student can desire in an ordinary book of reference.

A TEXT-BOOK OF THE METHOD OF LEAST SQUARES. By MANSFIELD MERRIMAN. New York: John Wiley & Sons. Price $2.00.

This is to replace the edition long since exhausted of The Elements of the Method of Least Squares by the same author—written in 1877. That the book was wanted was made manifest by its wide adoption. We notice with satisfaction that it is quoted as a standard by so careful an engineer as Petrie in his Pyramids and Temples of Gizeh.

The new book is offered as an improvement on the former one, inasmuch as numerous applications are made of principles to the different classes of observations.

CASSELL'S FAMILY MAGAZINE for April, contains among other timely and useful articles: How American Bread is Made, the Road to the Giant's Causeway, Work in the Garden, Wild Birds in London. The illustrations are exceedingly numerous.

THE QUIVER, an Illustrated Magazine for Sunday and General Reading. New York: Cassell & Company.

Each number contains 128 pages of reading matter, mostly of an instructive character.

UNITED STATES GEOLOGICAL SURVEY: GEOLOGY OF THE COMSTOCK LODE AND THE WASHOE DISTRICT. By GEORGE F. BECKER. Washington: Government Printing Office.

The volume of text treats by chapers of—The Comstock Mines, Previous investigations of the Lode, Lithology, the Structural Results of Faulting, the Occurrence and Succession of Rocks, Chemistry, Heat Phenomena of the Lode, the Lode, on the Thermal Effect of the Action of Aqueous Vapor on Feldspathic Rocks, on the Electrical Activity of Ore Bodies, Summary.

Some fine lithographic plates embellish the text.

The Atlas to accompany the above descriptive text contains 21 large plates, illustrating the geography, the geology, and the mining claims of the district; the whole beautifully printed in color.

This famous lode has exhibited many points of interest. It yielded in twenty-one years ore to the value of three hundred and six millions of dollars, forty-four per cent. of which was gold.

It was the deepest mine in America, extending to a depth of three thousand feet below the surface. The united length of its galleries is 185 miles. And finally, the unusual heat at the lower depths has afforded an interesting problem to geologists.

Everything relating to the physics, geology and lithology of the district is fully discussed and elegantly illustrated.

THE WAVE OF TRANSLATION IN THE OCEANS OF WATER, AIR AND ETHER. By JOHN SCOTT RUSSELL, F. R. S. S. Price $5.00.

This work is divided into three parts as follows: Part I. The Wave of Translation. Part II. The Wave of Translation and the work it does as the Carrier Wave of Sound. Part III. On the Great Ocean of Ether and its Relation to Matter.

An appendix containing a Report on Waves to the British Association, presented by the author in 1842–43, forms quite an important portion of the work.

The work is illustrated by ten photo-lithographs of the plates made to accompany the "Report" about fifty years ago. As might be inferred, they are wanting in clearness. The typography of the work is excellent.

SPON'S MECHANIC'S OWN BOOK. New York: E. & F. N. Spon. Price $2.50.

This book contains a collection of useful receipts designed to be serviceable to all classes of artisans. The descriptions of processes seem to be clear and full, and it is difficult to conceive of a trade or handicraft to which this book does not in some part relate.

The illustrations are numerous and good.

FOURTH ANNUAL REPORT OF THE STATE MINERALOGISTS OF CALIFORNIA. By HENRY G. HANKS. Sacramento: James J. Ayers.

This volume includes two reports as follows: California: Information General and Statistical of the Resources and Industries of the State. By Henry Degroot

The minerals of California, as far as known, alphabetically arranged.

The mention of the mineral is supplemented in many instances by an interesting account of the substance, including its natural history and geographical distribution.

Mineralogists will find this report valuable.

THIRD ANNUAL REPORT OF THE UNITED STATES GEOLOGICAL SURVEY. By J. W. POWELL. Washington: Gov't Printing Office.

This volume contains after the administrative reports the following papers:

Birds with Teeth. By Prof. O. C. Marsh.

The Copper-Bearing Rocks of Lake Superior. By Roland D. Irving.

Sketch of the Geological History of Lake Lahontan. By Israel C. Russell.

Abstract of the Report on the Geology of the Eureka District. By Arnold Hague.

Preliminary Paper on the Terminal Moraine of the Second Glacial Epoch. By. Thomas C. Chamberlain.

A Review of the Non-Marine Fossil Mollusca of North America. By C. A. White, M. D.

The illustrations which embellish the text, as well as the maps, especially the latter, are of a superior quality.

MISCELLANEOUS.

TECHNOLOGICAL SCHOOL OF THE BALTIMORE & OHIO RAILROAD COMPANY.—The inauguration of a Baltimore and Ohio Technological School for the promotion of a higher course of instruction for the apprentices of this service than now pursued, with headquarters at Mt. Clare, Baltimore, and conducted under the superintendence of a Board of seven Directors, appointed annually by the President of this company, is announced:

Messrs. John K. Cowen, E. J. D. Cross, Dr. Charles M. Cresson, Andrew Anderson, Dr. W. T. Barnard, Bradford Dunham and Charles Selden, are hereby appointed such Directors for the calendar year 1885.

The course and method of instruction in its several departments, and the operations of the Technological School, shall be governed by regulations prescribed by its Board of Directors.

The examinations prescribed in the course of the Technological School will be very thorough, and will require from the apprentice a close and persevering attention to study, without evasion or slighting of any part of the course, as no relaxation of any kind can be made by the Board of Examiners; and, as the company will hereafter endeavor to advance the graduates of the Technological School to positions of responsibility and trust in its service, only those who demonstrate willingness and ability to qualify themselves for advancement will be retained.

The General Manager will convene a Board of Examiners consisting jointly of two Medical Examiners of the Relief Association and three Instructors of the Technological School, whose duty it shall be to examine and classify all apprentices now in service, in accordance with the standard of qualifications prescribed in his General Order on the subject. This Board will visit each station where apprentices are employed and finally report, in writing, to the General Manager, the result of their labors.

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TELPHERAGE.

By Professor FLEEMING JENKIN, LL.D., F.R.S.

From the "Journal of the Society of Arts."

In the first place, it is necessary that I should define what is meant by this word "telpherage," and perhaps that I should defend its formation. The word is intended to designate all modes of transport effected automatically with the aid of electricity. According to strict rules of derivation, the word would be "telephorage;" but in order to avoid confusion with "telephone," and to get rid of the double accent in one word, which is disagreeable to my ear, I have ventured to give the new word such a form as it might have received after a few centuries of usage by English tongues; and to substitute the English sounding "telpher" for "telephore."

In the most general sense, telpher lines include such electric railway lines as were first proposed by my colleagues, Messrs. Ayrton and Perry. The word would also describe lines such as I have seen proposed in the newspapers, for the conveyance of small parcels at extremely rapid rates. But to-night I shall confine myself entirely to the one specific form in which the telpher line first presented itself to my mind, and which it has fallen to my lot to develop. In this form telpher lines are adapted for the conveyance of minerals and other goods at a slow pace, and at a cheap rate.

The problem which occurred to me was this: Was it not really possible to send vehicles, by means of electricity, along a single suspended wire or rod—in fact, to telegraph goods and passengers instead of messages. The idea is familiar as a joke, but, on consideration, it appeared that there might be good grounds for supposing both that the idea was practicable and useful. I am now able to show you the realization of that idea, and the result of experiments on a large and practical scale has, I think, justified the arguments which have induced me to devote much time and labor to telpherage.

[Here the model was shown in action. This model consisted of two concentric octagons of wire, the length of each outer span being 5 ft. On each octagon there was a single locomotive and train, equal in length to that of the span. These trains ran well and steadily in opposite directions round the lines.]

These arguments may be stated as follows:

We could not, with steam, employ a vast number of little one-horse engines to pull along a number of small trains or single wagons. There would be waste in the production of power, and great cost in the wages of the men employed at each engine. But an electric current, of, let us say, 50 horse-power, will, as it circulates through a conductor of mod-

erate size, drive thirty small engines each of one horse-power, which require practically no supervision, and can be made nearly as economical in their action as a single electromotor of 30 horse-power could be.

But if the power can be distributed economically along a line, say, ten miles in length, this allows us to employ thirty small trains, corresponding each to a waggon pulled by one horse, instead of a single train such as might require 30 horse-power. It we further distribute the weight by making each train of considerable length, we are able to employ an extremely light form of road, such as a suspended rope or rod of, say, $\frac{3}{4}$in. diameter. Later on in the paper I will show the amount of traffic which such a rod can practically convey. Meanwhile, I simply draw your attention to the general principles of the subdivision of power and the subdivision of weights. In distributing the power by means of electricity, it was clear that considerable waste must be incurred, but the amount of that waste is easily calculated, and is by no means prohibitory. Moreover, the power, being obtained from stationary engines, or in certain cases from falls of water, could be produced at a cheap rate in comparison with that obtained from locomotives or traction engines.

When I examined the various forms of possible road by which the distributed power and distributed load could be conveyed, it seemed to me that the single suspended rope or rod offered great advantages. The smallest railway involved embankments, cuttings, and bridges, fencing, and the purchase of land. A single stiff rail, with numerous supports, from which the train might hang, seemed better, and may, in some cases, be employed, but the supports would require to be numerous—say, one post every 10 or 15 feet—and even with these spans, the girder required to carry vehicles weighing 2 cwt. each, would be costly. With a single suspended rod or rope, we may have supports 60 or 70 feet apart. A $\frac{3}{4}$-inch rod, thus supported, will carry five vehicles, each bearing 2 cwt., without excessive strain. No purchase of land is necessary, no bridges, earthworks, or fencing. The line can be so far removed from the ground that it will not be meddled with, either by men or animals. A single wheel-path gives the minimum of friction, and the rolling stock can be much more easily managed than if we attempted to let vehicles run on double swinging ropes. On all those grounds it seemed well worth while to devise means by which trains could be electrically and automatically driven along the single suspended rod.

Before proceeding further, I had better state how far this idea has been realized. The Telpherage Company, Limited, was formed last year, to test and carry out my patented inventions and those of Professors Ayrton and Perry for electric locomotion. On the estate of Mr. M. R. Pryor, of Weston, two telpher lines, on my plan, have been erected. One of these is a mere straight road, with spans of 60 feet, and various forms of rod and rope. The first full-sized train was run on this line with a locomotive which we call the bicycle-wheel loco (Figs. 8 and 9). The line was found inconveniently large and high, and the experiments were continued on a line $\frac{5}{8}$-inch diameter, of round steel rods, with 50 feet span. This line is continuous, that is to say, it re-enters on itself. It is 700 feet long, and we have run a train of more than one ton at a speed of five miles per hour on this line with complete success. The insulation has given no trouble. It need hardly be said that we see our way to great improvements in details. Thus, we can make the road more uniform, and stronger for its weight; we can lessen the quantity of material used, and greatly diminish the amount of skilled labor required in erection. We can improve the design of the posts. We can improve the trucks and locomotives, so that they will go around sharper angles, and so forth, but the main object has been practically carried out. We have had trains on a scale as large as I am prepared to recommend, running at the highest speed I have contemplated.

I trust it will be clear to you, from this description, that what I have contemplated and realized is not an electric railway destined to compete with steam railways in conveying goods and passengers at high speeds, neither is it a new form of communication destined for small parcels and high speeds; it is simply a cheap means of conveying heavy goods

which, like coal or grain can be carried in buckets or sacks, each containing two or three hundredweight. The speed on a telpher line will be that of a cart, and the object we aim at is to cart goods at a cheaper rate and more conveniently than with horses.

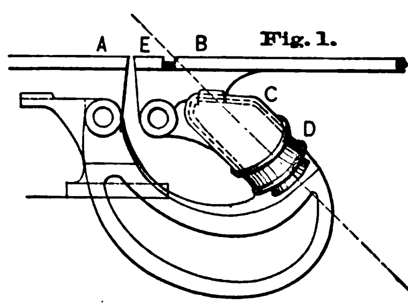
I assume that you all know that an electric motor is a machine which will run so as to exert power whenever an electric current is passed through it. You also know that a machine called a dynamo, driven by a steam-engine or other source of power, will produce an electric current which may be conveyed along a suspended and insulated rod, and used to drive an electric motor.

In describing the details of my system, the first point to be explained is, how the current produced by the dynamo, and conveyed along a single line, is taken from that line and directed round the motor.

In endeavoring to realize this idea, the first thought which occurred to me was that of dividing the line into lengths, equal to the length of the train, so that using the train to bridge over a gap between two sections at different potentials, the current could be conveyed from the leading to the trailing wheels of the train, round the motor. This idea is employed in the model now shown; but, in the first form which suggested itself, the gaps between the sections were opened by a switch worked by the front of the train, and closed by a switch worked by the end of the train. The first model, which may have been seen by some present, working in Fitzroy-street, was made on this plan. Trains driven in that way would all be coupled series. The present model is differently arranged; there are no working parts or switches. Let the successive sides of the polygon be called the odd and even sides; the odd outer sides are connected with the even inner sides, and the even outer sides with the odd inner sides. We thus have two continuous conductors each going right round the model, but not joined to each other; these are connected to the two poles of a battery. So long as no train bridges a gap no current flows, but whenever the train bridges the gap, a current flows from the positive to the negative pole round the motor. This plan is called the cross-over system; all the trains are joined by it in parallel arc, and the current is reversed each time a train passes a gap. This reversal does not affect the working of the motor. This is the plan which has been carried out on a large scale at Weston. Its simplicity leads me to believe that it will be the plan most usually adopted, but several other methods of driving have been devised. A spark passes between the wheels and the line each time the current is stopped, but this spark occurs between large masses of metal, where it appears to be harmless; it has given me no trouble whatever at Weston. Moreover, it has been found very easy to make connection between the line and the train. The ordinary truck wheels answer admirably, so that no complicated brushes are required. There are some absolute advantages in having interruptions at regular intervals, but the discussion of these would lead me too far for my present purpose.

Only one of the two continuous conductors requires to be insulated; this results in alternate insulated and uninsulated sections all along each line. Fig. 1 shows a saddle, as we call it, with an

Fig. 1.



insulated attachment, B, at the one end, and an insulated attachment A at the other, as used for a short sample line which has just been sent to Peru for the Nitrates Railway Company, Limited. The line itself is a three-quarter inch steel rod with forged ends, and Fig. 2 sufficiently shows the mode of attachment. The insulation is given by a vulcanite bell insulator D, carrying a cast-iron cap C. All the parts are designed to stand 2.2 tons strain; the vulcanite is secured between two layers of Siemens' cement. The experiments at Weston have shown that vulcanite answers per-

fectly, but the material is rather expensive. I have here a smaller porcelain insulator, which has been subjected to 2.2 tons' strain. I believe porcelain will answer well in all respects, but it has not yet been subjected to the test of actual traffic day by day. At Weston the vulcanite was used between layers of Portland cement, the only objection to which is, that it takes some time to set. The simple steel rod has been found preferable in all ways to rope. We find that there is less friction and less jar with the rod, and ample flexibility; it is also much easier to secure. Moreover, a solid rod with welded ends can be made so that the ends, where supported, are, to some extent, undercut, as is shown in the corresponding bulb angle-iron (Fig. 3) used for rigid parts

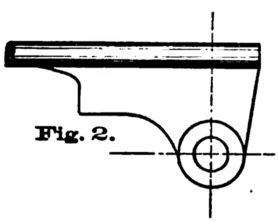


Fig. 2.

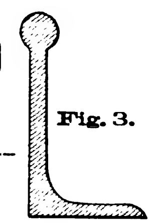


Fig. 3.

of the road; this undercutting allows much greater freedom of rolling than would be compatible with the horizontal gripping wheels, especially when gripping wheels are used which, like those in the model, actually hold on to the line so as to resist being lifted. A short piece E, slightly insulated, prevents the sections from being short-circuited by the wheels.

Fig. 4.

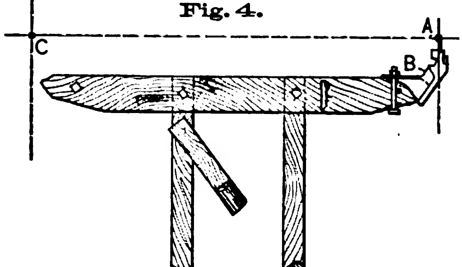
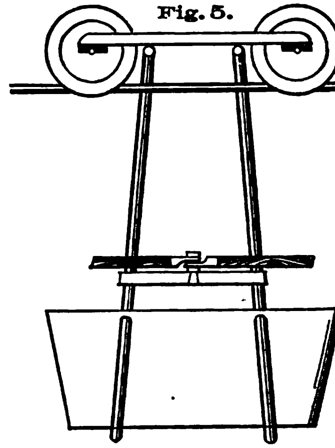


Fig. 4 shows the posts and crosshead supporting the line. In the one-inch example this design was fully carried out, and the posts stood the cross strain due to the overhanging load perfectly. In the five-eighth line an attempt was made to cheapen the construction, but the posts in wet weather work at the foundations; it is well that we are put on our guard against this danger. In the first design a sort of rocking saddle was employed, to allow the strain to be transmitted from one span to the next, but the flexibility of the posts provided amply for this object.

Abutment posts are required at intervals, and these can be made use of to provide compensation for changes of temperature, and to limit the stress on the rods. In straight lines I reckon about four abutment posts per mile.

In the short South American line, curves of 45 degrees at the posts will be employed, as shown in the model. At the stations where goods are to be handled, a rigid rod will be more convenient than the flexible rod. A bulb angle-iron like that shown in Fig. 3, supported every ten feet, answers well at Weston, and a siding, leading the trucks off this line, has been satisfactorily carried out. The siding leads back to the line at a point between two flexible spans. In fine, it may be said to-night that the problem of the continuous line, whether straight, curved, rigid, or flexible, has been completely solved. Drawings and specifications can be put, without further delay or experiment, into the hands of contractors.

Fig. 5.



Trucks used on ordinary rope lines are designed to be pulled by ropes on a road which is necessarily straight. When

trucks of this description, with wheels 8 in. diameter and 22 in. wheel-base (Fig. 5), were tried at Weston, arranged in trains, some new difficulties presented themselves. Any sudden check to the motion was followed by a rearing action, throwing the truck off the line; similar results followed the application of any sudden pull. Moreover, trucks with two rollers on a rigid frame, even with so great a wheel-base as 22 in., require curves of considerable radius if we are to avoid serious binding at the flanges. Notwithstanding these difficulties, the trains at Weston, with a little care, run well and lightly, but the trucks which have gone to South America are on the plan adopted in the model, and run much more safely, and turn much

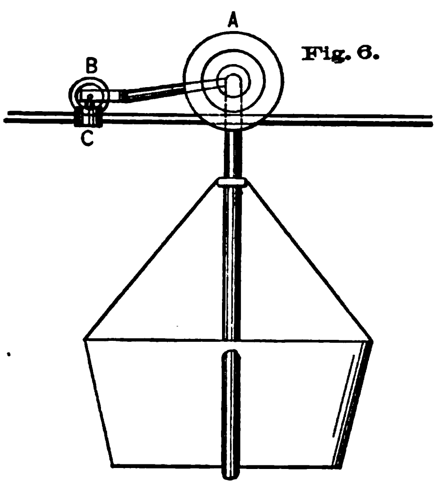


Fig. 6.

sharper curves. They have two peculiarities—first, each wheel 7 in. diameter (Fig. 7), is pivoted on an axis, B, vertically over the center of the wheels, A; this allows the truck to run with the freedom of a bicycle round curves; secondly, the weight carried is hung on a swinging arm, D, pivoted to the frame at a point, P, on a level with the line. The result is that any force applied in a plane containing the line acts as if applied at the line itself, and will neither lift the wheels in front nor behind. In the model, the coupling, as you see, is on one line attached to the top of the swinging arm, where the coupling rods are well out of the way. In the other line the coupling is below the rod. The swinging arm relieves the locomotive from all jerk at stopping or starting.

The truck is completed by a small hook or catch embracing the rod. In case of any accident causing the wheels to leave the line, this hook will prevent the truck from falling. The weight of the two-wheel stiff truck, shown in Fig. 5, with wrought-iron buckets, is 75 lbs. The weight of the two-wheel pivoted trucks, with wooden bucket, is 63 lbs. They are both adapted to carry 2 cwt. Fig. 6 shows a one-wheeled truck tested—the results were not favorable. A special form of bucket must be designed to suit such kind of traffic. Simple iron hooks for sacks will, in many cases, be available, and these hooks can be so contrived that on being struck they will drop the sack.

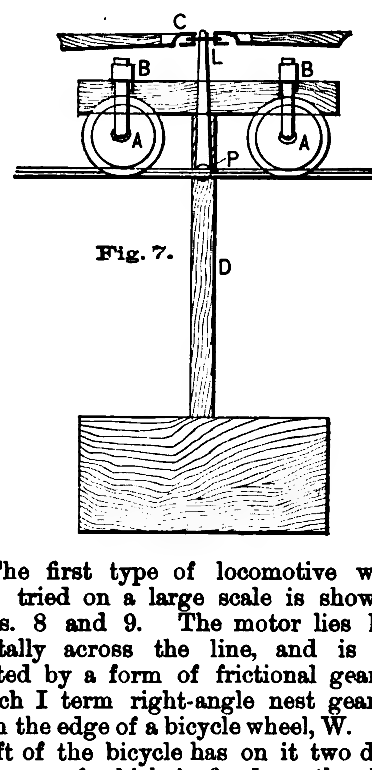


Fig. 7.

The first type of locomotive which was tried on a large scale is shown in Figs. 8 and 9. The motor lies horizontally across the line, and is connected by a form of frictional gearing, which I term right-angle nest gearing, with the edge of a bicycle wheel, W. The shaft of the bicycle has on it two discs, B B, one of which is fixed on the shaft, while the other can slide longitudinally on the shaft. These two discs are pressed together by a spring, D. Their edges bear on the horizontal gripping rollers, A and A, which seize the line. These rollers are supported in such a way as to be free to come together under the pressure of the spring transmitted by the discs, B and B. By tightening the spring, any

required grip can be obtained with no injurious friction, either on the cross shaft or on the spindles of the rollers. This grip is a form of right-angle nest gearing. The weight of the locomotive was taken by wheels, C C, fore and aft. The following defects were observed:—The frictional surfaces both in the upper and lower nests, were too small, and the materials too soft, so that rapid wearing resulted with a consequent increase of friction. Moreover, the grip was so powerful, that the rollers, A A, were capable of supporting the weight, and thus a small

Fig. 8.

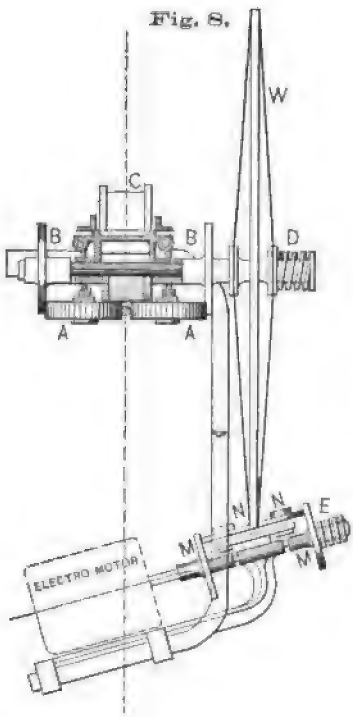
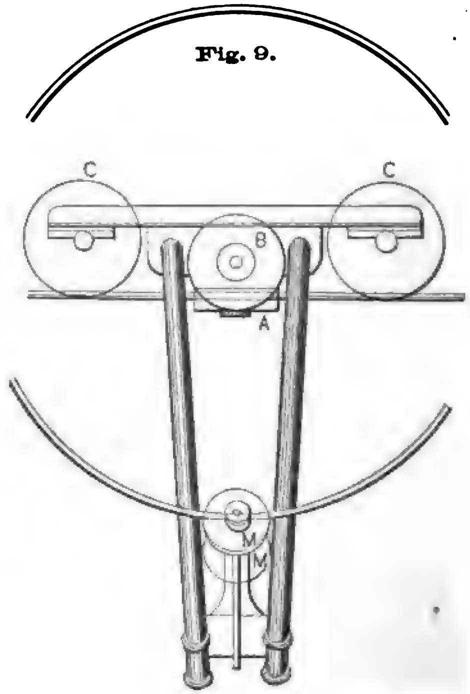


Fig. 9.

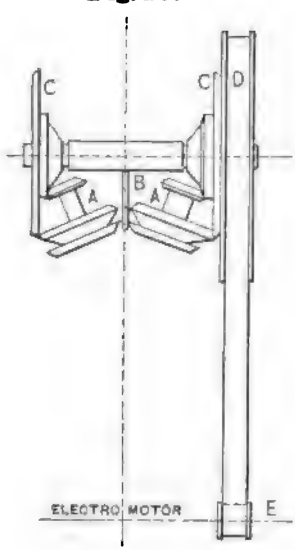


inclination of their vertical axis was enough to cause the locomotive to rise, and even run off the line; moreover, the vertical curvature in the rope, or at the posts, required the rollers, A A, to be deep, thus limiting the extent to which rocking was admissible; moreover, very broad pulleys, fore and aft, would be required even for moderate horizontal curves. Nevertheless, this locomotive ran sufficiently well on the one-inch line during an exhibition to the shareholders last autumn. The weight for a five-eighth line of a somewhat improved form of this type, to exert one-half horse-power, on the average, is 200 lbs., an extra half-hundredweight would give one horse-power. The driving wheels, A A, of this example are $6\frac{1}{2}$ in. diameter. The motor makes 9.23 revolutions for one of the driving wheels. One mile per hour corresponds to 473 revolutions per minute of the motor. 35.21 inch-pounds at the motor spindle are required for a pull 100 lbs. at the rail.

Figs. 10 and 11 show a locomotive designed by Mr. A. C. Jameson, when I was personally unable to attend to work. This locomotive, which is called the belt locomotive, shows a great advance on its predecessor. The general arrangement of the upper nest grip is retained, but a most ingenious modification has been introduced by which the discs, C C, run on one path on the rollers, A A., while the rod runs on another. In this way, the dirt from the line is never conveyed to the driving disc surface between A and C. Moreover, these frictional surfaces, which are points in the first form, have become lines in the second. This head answers admirably. The weight is car-

ried by a roller, B, between the gripping discs, an arrangement contained in one of my first small models, and wrongly rejected in the first large locomotive. With this subdivision of weight, the gripping wheels are much less likely to rise, and can be made very shallow. In the actual locomotive, these gripping wheels are of an open inverted Λ shape, whch has certainly run very well, although I prefer at present the upright V shape, which closes under the rail, as used in the model before you. Both of the gripping rollers drive as in the first type. The cross shaft is driven by a belt on a 20-in. pulley, D; the other end of the belt runs on a 2-in. pulley, E, on the motor spindle. The friction due

Fig. 10.



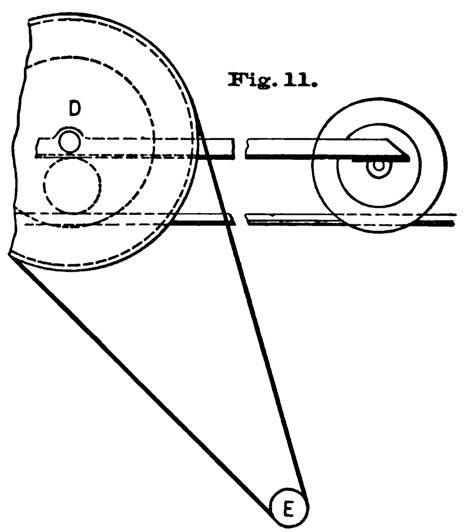
to the pull of this belt on the motor spindle is relieved by friction rollers. This locomotive runs extremely safely and steadily on the line; indeed I am not aware that it has ever been thrown off.

The following are particulars of its construction:—Weights with 96 lbs. one horse-power motor 269 lbs; wheel-base 2 ft. 6 in.; diameters of driving rollers 6 in.; 4.94 revolutions of motor per one revolution of driving wheel. A couple of 60.6-inch lbs. on motor is required for 100 lbs. pull at rail. 276 revolutions of motor correspond to one mile per hour on the rail.

The only improvements I have to suggest in this design are:—1st, the addition of gear which will give a higher speed of motor for the normal speed of four miles per hour, which we contemplate; 2d, the addition of a swivel or bogie arm, such as is used in the model before you; 3d, improvements in the belt connection. Moreover, the machine requires strengthening in some places. It will, however, be seen that none of these points touch the essential features of the design, which might at once be adopted in practice. Worked with motors of the Gramme type, the additional gear would not be required.

Before the belt locomotive had been completed, it was necessary to design a locomotive for the South American line,

Fig. 11.



which I have several times mentioned. I had, meanwhile, constructed the model which is now before you; and this little locomotive, in which the power is transmitted, by ordinary spur wheels, ran so extremely well, that I adopted the general arrangement for the next example on a large scale. This arrangement is shown in Figs. 12 and 13, the grip (C C and B B) is a third variety of the right-angle nest, simpler than that in the belt locomotive. In this form, also, we have line contacts, and two paths for the discs and rod. Where it is desired to drive from both sides, this arrangement is less powerful than that in in the belt locomotive. In the South American locomotive, I drive from one side only, leaving the

off side roller free to revolve as it pleases; this avoids grinding at rapid curves, and the adhesion given by one wheel will be ample in a dry country, such as that where this locomotive is to work. The arrangement of the gearing, E and F, is obvious; it allows the locomotive to lie fore and aft instead of across the line, and this design has some advantages in the adjustment of the weights. The surfaces of the gripping wheels are arranged like an upright V, so as to hold on under the line. This makes it very difficult for the wheels to leave the line, both because of their absolute hold and because the inclination of the V is such as to favor the action of gravitation in overcoming the friction of the grip, instead of opposing it as in the inverted Λ.

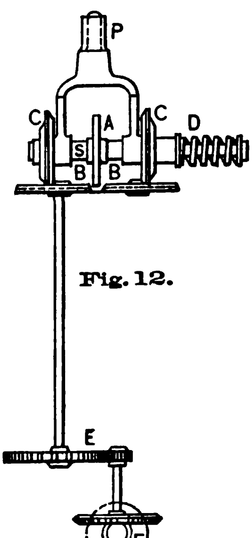


Fig. 12.

Another feature of this machine is the arm pivoted at P, and carrying the leading wheel, which is again pivoted at M in the arm, as in the case of the trucks. This construction allows the locomotive to traverse curves of 6 ft. radius—a very remarkable result.

The full-sized locomotive has only just been completed, and run on three spans at Messrs. Easton and Anderson's. So far as I am able to judge from the trial, it is likely to be a complete success. It will be immediately shipped for its destination, so that its performance cannot be more fully tested in this country. The following particulars will show that it is much more powerful than the belt locomotive, but it is considerably heavier:—Wheel-base, 2 ft. 6 in.; weight, about 3 cwt. 14 lbs.; 15 revolutions of motor per revolution of driving wheels; diameter of driving wheels, 10 in.; 33.3 in. lbs, per 100 lbs. pull at rail; 504 revolutions of motor per minute for one mile per hour.

I am in doubt at this moment whether to adopt the belt locomotive or the spur wheel locomotive for the next example; it is simply a question of cost, weight, and durability. Either will do the work.

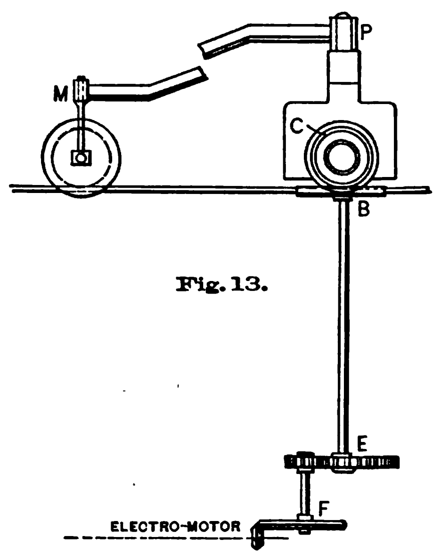


Fig. 13.

In all the arrangements it is essential that the second bearing wheel at M (Fig. 13) should lead, not follow the drivers in regular work. The reverse arrangement lets the rope lead on at an angle with the plane of the roller, causing an injurious grinding action.

Details of couplings have been well worked out, but space fails for their description.

As general features of the train running on the line, I may mention that the deflection of the rod within reasonable limits has very small influence on the resistance. When the deflection on a 50 ft. span was about 2.4 ft., the resistance for a train of trucks, weighing in all 1,260 lbs., was 22 lbs.; and no sensible difference could be detected when the

deflection was materially reduced. This resistance was measured by pulling a train along, span after span, by one end of a rope passing over a pulley on the leading truck, and having a weight hanging vertically from the other end of the rope; the weight thus limited the pull. This pull differs extremely little as the train moves along, for when one part of the train is descending the curve the other part is ascending. It should be noted that during this experiment no special care had been taken to oil the bearings, and I have no doubt this pull can be materially reduced.

I have ventured to dwell at some length on the mechanical problems involved in this form of telpherage, because the experiments made so far have chiefly borne on questions of mechanics. The makers of dynamos can put at our disposal apparatus which will generate day after day, with perfect certainty and regularity, currents of electricity such as will transmit the horse-power generated by powerful steam-engines. These makers have already solved the chief electrical problems which present themselves in connection with telpher lines. They can give us at will constant current or constant electromotive force, high or low, as we may chose. They are now able to arrange their apparatus so that any number of incandescent lamps may be turned off or on, without disturbing the regularity with which other lamps are supplied, and by the same arrangement we are enabled to start or stop any number of telpher trains without disturbing the running of others. The electrical problems of the telpher line and those of electric lighting run in absolutely parallel lines.

The electric motor, although it may be termed a mere inversion of the dynamo, has not as yet been brought to equal perfection, but month by month improved designs, proportions, and materials are being introduced, and the result already attained is sufficient for our purpose. It is all the more encouraging to feel that these results will certainly be surpassed, and far surpassed in the immediate future.

The following short summary of the problem of the transmission of power by means of electricity, may interest those who have not studied the subject. There are three steps in this transmission—1st, we convert mechanical power into electricity by means of a dynamo; in doing so we incur a loss of from 10 to 20 per cent.; 2d, this electricity, in flowing along a conductor, generates heat, representing a further loss, analogous to that resulting from friction in mechanical gearing. This loss, depending on the distance of transmission, the size of the conductor, and the electromotive force employed, is easily computed. 3d, we re-convert the electricity into mechanical power by means of an inverted dynamo, which we term an electric motor. With motors in which large weights of iron and copper are employed, the loss in re-conversion need not exceed 20 per cent., but with light motors, weighing from 70 lbs. to 100 lbs. per horse-power, such as we must employ in the locomotives, I could not undertake with certainty at this moment to effect the re-conversion without a waste of one-half. The effect of all these sources of loss is, that at the stationary engine I must exert about 3 horse-power for every single horse-power which is employed usefully on the line. I look forward confidently to the time when 2 horse-power at the engine will be sufficient to give 1 horse-power to the motor.

To put these conclusions in a more scientific form, I may assume the efficiency of my dynamo as 80 per cent., that of my small motor as 50 per cent. The waste by heat expressed as horse-power is equal to $\frac{C^2R}{745}$ where C is current in amperes, and R the resistance in ohms. The horse-power represented by the current is equal to $\frac{E.C}{745}$ where E is the electromotive force in volts, and C the current in amperes. It follows from the last expression that I may increase the horse-power in three ways, by increasing either E or C, or both. If I increase E, leaving C the same, I do not increase the loss during transmission along the line, no matter what horse-power the given line may transmit. A practical limit is set to the application of this law by the difficulty met with in dealing with electromotive forces above 2,000 volts. Marcel Deprez, taking advantage of this law—first pointed out by Sir William Thomson

—has transmitted seven or eight horse-power over seven or eight miles, through an ordinary telegraph wire, and he obtained a useful duty of 63 per cent., taking into account all the three sources of loss which I have enumerated. With small motors I cannot yet promise a result so good as this, and I merely mention it to let you understand that, in speaking of 3 horse-power for one at the locomotive, I am leaving a very ample margin.

Quitting generalities, I will give some details as to the electrical and other conditions necessary, in two examples, for what may be considered as typical telpher lines:—

First Line.—Length, five miles. Length of circuit, out and in, ten miles. Twenty-five trains running at once, spaced one-fifth of a mile apart; speed, four miles per hour. Let each require 1 horse-power on average; let the motor take on the average two ampères of electric current; let the electromotive force near the stationary engine be 840 volts; the electromotive force at the end of five miles will be about 746 volts. The total current entering the line will be fifty ampères at the near end of the line. Fifty ampères and 840 volts represent 56.5 horse-power; of this, 6.5 horse-power will be wasted in heating the line; the remaining 50 horse-power will do work in the motors equivalent to 25 horse-power. In order to give this current of fifty ampères with 840 volts, the stationary engine will require to exert $\frac{10}{8} \times 56.5$ horse-power, or roughly, 70 indicated horse-power, or somewhat less than three times the useful horse-power. Let us now examine the economical results to be obtained from such a line as this. Mr. Dowson, in an interesting comparison between the cost of horse-power obtained from coal and gas, reckoned the cost per horse-power for a 100 horse-power engine, at the rate of £3 6s. 9d. per annum, to include wages, coal, oil and depreciation. Mr. Dowson would naturally be led to put the cost of steam power obtained from coal rather high than low. I will, however, adopt a very much higher figure, and assume that the power may cost as much as £6 10s. per horse power per annum; this gives £455 as the cost of the 70 horse-power required for my telpher line.

Let the 25 trains each convey a useful load of 15 hundredweight. In a day of 8 hours the line will have conveyed a traffic which we may express as 600 ton-miles—*i. e.*, it will be equivalent to 600 tons conveyed 1 mile, or 60 tons on each line conveyed from end to end daily. If we count 300 working days in the year, the sum of £455 gives £1 10s. 4d. per diem, and the 600th part of this is about 0.604 of a penny, as the cost of the power required to carry a ton one mile.

In Great Britain we ought easily to be able to reduce this below a halfpenny per ton per mile, which proves that the apparent great waste, even of two-thirds of the power in transmission, does not involve prohibitory expense. In calculating the whole cost of transport, we must further take into consideration the cost of the installation. Taking the spans at 70 ft., I estimate this cost as follows:—

Line £500 per mile	£2,500
Engine, boiler, and shed, at £20 per indicated H. P.	1,400
Dynamo and fittings	1,000
Twenty-five trains	2,500
Contingencies	600

Allowing 12½ per cent. for interest and depreciation, this represents an annual cost of £1,000. Allowing £100 as the salary of an electrician or young engineer, and adding £455 the cost of the power, this gives a total annual expenditure of £1,555 for a daily duty of 600 ton-miles. If we continue to assume the year as containing 300 working days, the total cost of conveying one ton one mile will be found equal to 2.07d. If goods are to be transmitted for long distances, the same calculation applies. We should simply have stations ten miles apart, working lines five miles long on each side of them. This, then, is the practical outcome of the general principles stated at the beginning of this paper. We may expect with great confidence that it will pay investors to convey goods for any distance at the rate of 2d. per ton per mile, by the agency of the suspended telpher line.

Matters are somewhat modified when the traffic is smaller. Making similar calculations for a line one mile long instead of five, with only four trains running at once, we might employ an electromotive force as low as 100 volts; the loss by heating would be insignificant; we should require about 12 horse-power; the work done in

8 hours would be 96 ton-miles. I estimate the cost of installation at £1,600, and the annual cost of working £344, without the annual salary of an electrician. This corresponds to 2.875d., or less than 3d. per ton per mile. One very important feature in respect to the cost of telpher lines is the fact that the larger part of that cost is due to plant, such as locomotives, trains, and dynamos. This plant can be increased in proportion to the work required; thus there is a very moderate increase of cost in the rate per ton per mile for a small traffic as compared with a large one, and, on the other hand, a line laid down for a small traffic will accommodate a much larger traffic with no fresh outlay on the line itself.

There are numerous minor electrical problems involved, but time does not permit me to enter into the consideration of these to-night. It will be sufficient for electricians when I say that I see my way to governing, blocking, and breaking the trains, without ever interrupting the current used to work the motor, except between the line and rolling wheels. We already know that the interruption at this point, although accompanied by a spark, does no injury whatever. I have often been asked whether the frequent reversals involved in the cross-over system do not tend either to injure the dynamo or the motor. I made special experiments on this very point lately with a compound-wound Crompton dynamo and Mr. Reckenzaun's motor with thirty-six coils. I was unable at the commutator of the motor to detect the smallest change in the motion due to the most rapid reversal. At the dynamo commutator I could just see when the reversal occurred, but there was no change of a character to cause the smallest alarm. At the same time I may state that, when from any cause reversals may be thought undesirable, we are in possession of apparatus which we call "step-overs," which, without diminishing the simplicity of the permanent way, enable us to send a continuous and unreversed current. These and similar electrical questions, such as the performance of Messrs. Ayrton and Perry's excellent motors, might possibly have had greater interest for electricians than some of the mechanical details discussed to-night; but I have felt that the main point to establish, in bringing this invention before the public, is that we have in telpher lines a means of conveying goods in an economical manner, by lines, locomotives, trucks, dynamos, and motors, which have undergone their preliminary trials with success, and can be at once applied to the more searching test of performing work for the public. If I have established this fact, I think you will have no difficulty in believing that the subsidiary electrical problems have been, or will be, readily solved. I hope that at a future period these will be brought before you in detail on many occasions by many men.

In conclusion I will enumerate some of the uses to which telpher lines may be put. They will convey goods, such as grain, coals, and all kinds of minerals, gravel, sand, meat, fish, salt, manure, fruit, vegetable; in fact, all goods which can be divided conveniently into parcels of two or three hundredweight. If it were necessary, I should feel no hesitation in designing lines to carry weights of 5 or 6 cwt. in each truck. The lines will carry even larger weights, when these, like planks or poles, can be carried by suspension from several coupled trucks. The lines admit of steep inclines; they also admit of very sharp curves. Mere way leaves are required for their establishment, since they do not interfere with the agricultural use of the ground. They could be established instead of piers, leading out to sea, where they would load and unload ships. With special designs, they could even take goods from the hold of a ship and deliver them into any floor of a warehouse miles away. When established in countries where no road exists, the line could bring up its own materials, as a railway does. Moreover, wherever these lines are established, they will be so many sources of power, which can be tapped at any point, for the execution of work by the wayside. Circular saws, or agricultural implements, could be driven by wires connected with the line, and this without stopping the traffic on the line itself. In fine, while I do not believe that the suspended telpher lines will ever compete successfully with railways, where the traffic is sufficient to pay a dividend on a large capital, I do believe that telpher lines will find a very extended use as feeders to railways in old countries, and as the cheapest mode of transport in new countries. In presenting this view to you, I

rest my argument mainly on the cost of different modes of transport, which may, I believe, be stated approximately, as follsws:—

Railway, 1d. per ton per mile; cartage, 1s. per ton per mile; telpher lines, 2d. per ton per mile; and let it be remembered that, in taking the cost of cartage at 1s. per mile, the first cost and maintenance of the road is left wholly out of account; whereas, in my calculations for the telpher line, allowance has been made both for establishment and maintenance.

HARBORS AND DOCKS.*

By E. SHERMAN GOULD, C. E.

Reviewed for VAN NOSTRAND'S MAGAZINE.

Of books of the class to which the volume before us belongs, it may be commonly predicted, off-hand, that the latest is the best. Harbor and dock engineering is so largely built upon observation, is so much an affair of "trial and error," and, as a science, is as yet in such an early period of its development, that that work which, from its late date, can give the most complete record of success and failure in the various operations which it describes, must, in so far, be the most valuable one.

In the present volume Mr. Vernon-Harcourt gives a very thorough and comprehensive review of the subjects enumerated on his title page. His method of treatment is purely descriptive, and rather general, in its avoidance of detail. This was unavoidable if the whole of the wide scope undertaken was to be brought within the limits of 650 pages, and probably the work possesses a wider usefulness from this fact. It is certain that the guiding principles involved are brought into a clearer prominence by not dwelling at length upon merely accessory facts or details of construction.

In the opening chapters the fundamental data of marine engineering are presented, by a statement of the facts concerning the winds, waves and tides. The wind and the tide are the great causes of those violent wave agitations, which it is one of the objects of this branch of engineering to encounter and neutralize. Without the wind there would be no waves; indeed, when we witness the awful onset of the ocean, dealing its staggering blows against the free end of a breakwater, we but behold the physical embodiment of the invisible breath of the tempest, which, were its destructive energy not stored up in the waves, would howl harmlessly around the pier-head. From the chapter which treats of the winds, we transcribe the following valuable and simple formula, verified, it seems, by observation, which gives the pressure of the wind in pounds on the square foot, expressed in terms of the velocity in miles per hour, thus:

$$\frac{V^2}{100}=P.$$

Passing to the subject of waves and wave motion, we find in Chapter II. these matters treated from the point of view of their more striking generalities, and without the detail into which Monsieur Comoy enters in his "*Etude pratique sur les Marées fluviales*" (see VAN NOSTRAND'S MAGAZINE, September, 1884), but in a way which, to the general reader at least, will perhaps be more acceptable than a full mathematical investigation. We notice here a tendency to question Mr. Russell's division of waves into those of oscillation and translation. Mr. Harcourt says: "It is evident, however, that if great wind waves are waves of translation, the same must be true of the smaller wind waves, though in a less degree. As soon as it is admitted that wind may produce some translating effect upon water, the distinction between waves of translation and oscillation cannot be maintained."

As an instance of the marvelous power of the sea, Mr. Harcourt mentions the destruction of the free end of Wick

* Harbors and Docks; their Physical Features, History, Construction, Equipment and Maintenance. By Leveson Francis Vernon-Harcourt, M.A. Two vols. 8vo, text and plans. Oxford, 1885.

breakwater during a storm in December, 1872. A mass of cement masonry of 1,350 tons, about 45 feet wide, 21 feet high, and about 26 feet long, was on this occasion gradually turned around upon its base, and finally tilted bodily off its foundation. After reading this account we are relieved to learn that the Wick breakwater is exceptionally exposed, owing to a combination of tidal and other influences, the waves rising occasionally to the height of 42 feet, although the depth at low water is only 30 feet.

We would call particular attention to the long and very able Chapter III., in which tides, currents, and their effects are admirably treated upon. Indeed, we should say that these subjects alone, of the theoretical ones touched upon in his book, are examined by the author with some considerable degree of close investigation.

These preliminaries being cleared away, the author enters upon a classification of the various types of harbors, their forms and characteristics. This part of the work seems well done, and to be well done must have cost much thought and study, as those will admit who have ever attempted to reduce a great number of facts and instances to a systematic order. When well done, such classification is of itself an immense aid to the study of any subject.

Next comes, in natural sequence, the consideration of the structures destined to improve or create the harbors thus described.

The principles and classification of these structures being established, practical applications are made by citing an exhaustive list of existing harbors—a sort of *catalogue raisonée* of some of the principal seaports of the world, in which the different harbor improvements mentioned have been practiced, with their results.

Rather than attempt even the briefest review of the whole of this most interesting and instructive portion of Mr. Harcourt's book, it will serve a better purpose to dwell for a moment upon some one or two leading types of harbor improvements. We will select the comparison afforded by the two systems of jetties for the improvement of backwater harbors—that is, harbors largely kept open by the scour of the ebb tide, the waters of which had previously entered on the flood. These two systems are, that of parallel and that of converging jetties.

Parallel jetties were the first to suggest themselves for the improvement of tidal harbors by deepening their bars, when a growing commerce caused larger and larger vessels to seek their docks. To a considerable extent the desired effect was accomplished, but, says Mr. Harcourt, "These jetties, however, when projecting from a shore along which there existed a natural drift of sand or shingle, acted like groynes, and arresting the travel of material along the coast, produced an advance of the foreshore, which eventually compromised the depth at the extremity of the jetty channel. This difficulty was met by a periodical extension of the jetties, till at last, when the drift was considerable, the jetties extended a long distance in front of the port, as may be seen at the present day at Dunkirk."

One among the objections to parallel jetties is, that they impede the free entrance of the flood tide into the harbor, by forcing it to travel through a long and narrow channel, and thus diminish the amount of backwater available for ebb-tide scour. To avoid this the upper portions of the jetties have been built of open timber work, with partial success only, because not only was a large portion of the tide water still excluded, but also from the jetties not being tight, they allowed a considerable dispersion of the ebb tide, which, to be fully effective, should be confined to the channel it is wished to deepen. Sluicing basins have also been in some cases resorted to, but of course all such appliances are not only costly, but introduce an element of complication which it is always very desirable to avoid. On the other hand, solid converging jetties "provide a reservoir of tidal water close to the entrance, afford ample room for the reception of vessels or the working of dredgers, reduce the entering waves, also in some cases by being kept low, allow the littoral current to pass over unchecked, and in every case, owing to their sloping course, present less obstacle to the regular flow of the current around the entrance."

Among other differences of the two systems is the width of opening. "In converging jetty harbors, a greater width

of entrance is generally given than in the case of the parallel jetty harbors. These harbors are of more recent origin, and consequently designed for a larger class of vessels; they also commonly extend into deeper water, and are frequently more exposed. In these cases the width has to be a compromise between conflicting conditions. The object of these jetties is to secure a deep entrance; if the entrance is made very wide, a bar is liable to form from the inefficiency of tht scouring current; if the entrance is very narrow it induces a rapid current, which, indeed, maintains the depth, but is prejudicial to the entrance of vessels, and a narrow passage is naturally objected to by seamen. . . . . . Charleston, the finest jetty harbor in the world, is designed to have an entrance 2,000 feet wide."

It is certainly gratifying to an American engineer to record the above tribute to one of the boldest projects ever undertaken in harbor improvements.

One of the most interesting descriptions is that of the creation of a fine harbor at Cherbourg, one of the largest marine works ever undertaken, and which, commenced in 1783, was only completed in 1853. There being no natural harbor of large dimensions suitable for a naval station on the southern coast of the English channel, the French engineers undertook to make one, by simply throwing an immense detached breakwater across the mouth of Cherbourg Bay, leaving an entrance east and west. This, after a long contest with many difficulties, was finally and triumphantly accomplished.

The chapters upon docks are perhaps less interesting to American engineers, from the fact that the small rise and fall of the tide in our harbors renders the use of docks, properly so-called, unnecessary. We extract the following remarks upon the use of concrete in dock-building, as they have a wider interest than this special application: "The experience at Chatham shows that a good concrete wall can be constructed with as small a proportion of cement as 1 in 12; but unless very careful supervision is exercised in the mixing of the concrete, it would be safer to adopt a somewhat larger proportion of cement. . . . . The economy effected by the use of a concrete wall is occasioned not merely by the cheapness of the materials, but also by the facility of construction and the saving of plant."

We also quote the following: "The quay walls at New York, whilst built upon a similar principle to those of Rouen, have a foundation of rubble stone in addition, as the piles in this instance do not reach solid ground, and therefore they have to be consolidated, and the base enlarged by a mound of stones. It is satisfactory to find that a stable quay wall can be built with only a moderate amount of settlement on a bed of silt; the success of the operation being due partly to the adherence of the piles, partly to the consolidation of the silt around the piles by the small cobble-stones, and partly to the broad base of the larger rubble mound."

The author naturally finds the quays of New York deficient in appliances, such as traveling cranes, hydraulic power, and the like, which are always found on the great European docks. He mentions the use of compressed air in the working of dock cranes, but while admitting that it is superior in some special cases, as at Portsmouth, considers that hydraulic power is generally the best.

The remarks made upon dredging seem somewhat meager and scattering. We are rather surprised to find, in so complete and general a *compte rendu*, no mention made of the excellent results obtained by General Gillman in dredging, while under way, with the centrifugal pump at Charleston, Savannah, and elsewhere. We remark also that the author seems to hold the scaphandre, or diving suit, in low estimation, as compared with the diving bell (of which he mentions an improved variety) for submarine work. In his observations upon lighthouses and beacons we fail to find any mention of the advantage of range beacons in entering or leaving ports.

A work based to so great an extent upon facts and observation, leaves but little opening for criticism, its merit or demerit depending almost entirely upon the intelligent classification and presentation of the collected material. In these respects we have nothing but praise to bestow upon Mr. Harcourt's volume. We find in it a vast store of well-selected facts and instances of marine engineering,

carefully ordered and arranged. Every type of harbor and of harbor improvement finds a place and abundant illustration from existing works. The large mass of material necessarily involved, is so condensed and stripped of superfluous matter, that what would seem at first sight to be a superficial review, is found upon examination to contain all the central facts. The accompanying atlas of plates is exceedingly complete; in fact everything mentioned in the text finds its graphic representation in the atlas. Were we disposed to find any objection to the plans of the different harbors, it would be that there was hardly enough of the territory back of the actual sea front given, but such an addition, while it would undoubtedly add to the value of the illustrations, would greatly increase the size of the volume. All the sections of piers, breakwaters, etc., are very good and complete.

THE SEWAGE-DEPOSITING TANKS OF FRANKFORT-ON-THE-MAIN.

By W. H. LINDLEY, M. Inst. C.E.

From Abstracts of the Institution of Civil Engineers.

These works are now in hand, and will be completed in the early part of 1886. The sewerage-works, comprising a network of about 160,000 meters of sewers, have been in execution since the year 1867, and are for the greater part complete. The subsidence-tanks form the completion of the outfall section of these works, and by the plan adopted two outfalls were needed, one on the right, the other on the left bank of the river, discharging into the river main at some distance below the town. While the more costly outfalls were in progress, a temporary outlet was made use of at the gasworks, and this now serves as a storm-overflow. At first the admission of the water-closets was left an open question, as the sewers were constructed to attain a number of important results which could not be delayed till this question, then so largely debated on the Continent, was settled. Professor Pettenkofer, at that time opposed to the water-closet system, however, when called in, reported in this case in its favor, and the prohibition was withdrawn. Owing to the rapid discharging power of the town sewers, the excrementitious matters and paper came down in considerable quantities in a very unchanged condition into the river, and occasioned nuisance, and steps had to be devised to screen out the grosser suspended matters from the sewage water.

After many difficulties, caused by the landed proprietors, and after much pressure had been brought to bear upon the municipal authorities, plans for dealing with the sewage water were prepared so far back as 1876–77, but for various reasons the matter remained in abeyance. In fact, though constant negotiations were in progress, it was not until October, 1882, that the scheme was finally settled. It was decided to bring all the sewage-water to the left bank for treatment, and to effect this the sewage is carried in two wrought-iron pipes, each 0.75 meter (2.46 feet) in diameter, laid as an inverted siphon under the bed of the river. Each of these pipes can deliver 500 liters (110 gallons) of sewage per second. The extra pipe is added to provide for subsequent increase of population, as the one pipe will suffice for the whole of the present flow, including a rainfall equivalent to the sewage in volume. For heavy rainfall, which might exceed the carrying-capacity of the siphon, a storm overflow is provided. The main outfall sewer is capable of discharging 2,700 liters (294.2 gallons) per second, but it was considered unnecessary to arrange for bringing all this volume across the river, as it would not have been feasible to construct tanks for so large a volume of rain-water, and it would have simply been turned into the river as storm-water. For the normal volume of sewage, the one pipe only is employed, the area of cross-section normally in use is thus reduced to one-half what it would have been if one pipe of equivalent carrying power to the

two laid had been adopted; the normal velocities have thus been about doubled, and this will, it is expected, be sufficient to scour out the siphon. Plans are given, showing the arrangement of the tanks and works. The process to be employed will comprise a system of mechanical purification, aided by chemical purification. It is thought expedient to remove first from the sewage such substances as can be separated by simply mechanical appliances. These impurities consist of floating bodies, coarser suspended matters, and the heavier substances which sink to the bottom. The precipitants to be subsequently employed are sulphate of alumina, and lime. As is well known, the acid in the former substance combines with the alkaline matters in the sewage, and sets free the alumina, as a bulky flocculent precipitate, which, by its affinity for organic substances, entangles them and carries them down. As sufficient alkalies are not present in the sewage to liberate the whole of the alumina, a certain proportion of lime has to be added. The advantages of this treatment are set forth. The plan of tanks adopted will not be carried out at present in its entirety, but will provide for future extensions. The whole scheme comprises twelve parallel tanks, of which only four are now in progress of execution.

The chief point to be considered in respect to the construction of the covered tanks was whether they should be above ground or sunk, *i. e.* high-level or low-level. The former plan would involve continuous pumping to a height of from 5 to 6 meters. By adopting the alternative arrangement of low-level reservoirs, it is possible, without any pumping, to pass the sewage into them by gravitation, and to discharge the clarified effluent into the river on an average of three hundred days in the year. Only on forty-seven days in each year does the river rise to such a level as to involve partial pumping of the contents; and only on about one day in three years is the river so much in flood as to render the use of storm overflows necessary for the whole of the sewage. The extra cost of sunk tanks would be compensated for by the avoidance of pumping. The machinery for pumping and treating the sludge, which is the part of the process presenting the most difficulty, has been concentrated at the lower end of the tanks to which the sludge naturally gravitates. The dimension and inclination of the main outfall-sewers, the arrangement of the sand-intercepting chambers, the bye-passes, overflows and storm-outlets, the mode of admitting the water into the tanks, of passing off the clarified effluent, draining the sludge of a maximum amount of its water, and finally drawing off the sludge, are fully described. The four tanks are each 82.4 meters (270.3 feet) long×6.0 meters (19.68 feet) broad. The bottom is formed of an inverted brick arch, and falls 1 meter in the direction of its length; the depth of the water in the tanks being 2 meters at the inlet and 3 meters at the outlet end. Each tank has a normal capacity of 1,100 cubic meters (242.106.7 gallons), and is calculated to receive from 4,000 to 5,000 cubic meters daily, under average conditions of flow. The contents are thus about 25 per cent. of the whole quantity to be treated; the corresponding proportions at the following English towns, by which the Author was guided, being—Leeds, 22½ per cent.; Coventry, 30 per cent.; Aylesbury, 26 per cent.; and Burnley, 16 per cent. The total average daily volume of sewage water to be treated at the present time amounts to 18,000 cubic meters (about 3,961,746 gallons). The average velocity of the sewage-water in passing through the tanks is 4 millimeters per second (about 0.786 foot per minute). It is intended to empty each tank weekly. Full particulars are given concerning the machinery for pumping and preparing the precipitants, the stores (which will contain 200 tons of sulphate of alumina and 180 tons of lime), as also the proposed plan of dealing with the sludge and detritus, and the general method of carrying on the works.

THE 110-TON GUN.—The English government have ordered three 110-ton guns, and of these one is to be delivered in October next, another in January, and the third in April, 1886. The price per gun is £19,500; the weight of the projectile is 1,800 lbs.; the charge is 900 lbs. of cocoa powder; the muzzle velocity is 2,020 ft. per second; the maximum powder pressure is 17 tons per square inch. The velocity and pressure are, of course, only estimated, although they are based on the experience gained with the Italian guns.

EXPLORATION—AND THE BEST OUTFIT FOR SUCH WORK.

BY MAJOR-GENERAL THE HON. W. FIELDING.

From the "Journal of the Society of Arts."

I THINK it best to preface this paper with the Latin expression, *Quot homines tot sententiæ*, which may be very freely translated—a tot of men afford a quart measure of opinions. But, seriously speaking, it would be quite absurd for any one individual at any one period of the world's existence to attempt to lay down the law as to how explorations should be carried out.

The most that I can attempt to do is to speak in general terms on the whole subject, using such knowledge as I have gained during my own travels in various quarters of this globe. In order to treat the subject as exhaustively as the limit of time at our disposal will admit, it will be well to divide it under two headings.

1. On exploration generally, and the manner in which the subject should be considered.

2. On the outfits recommended for use by explorers under varying circumstances.

The first heading we must again subdivide into—(*a.*) Scientific explorations. (*b.*) Commercial and geographical. (*c.*) Military. (*d.*) Explorations arising purely out of a love of adventure.

Now, scientific explorations differ or vary exceedingly in their intention and their scope. Their scope depends again upon their intention, and their duration depends upon both these. For instance, botanical explorations may have for their aim a new genus, a new species, or a new variety only of some species. The scope of such exploration may embrace one or more islands in the Pacific Ocean, or the whole of the interior of some unexplored continent such as New Guinea. The duration must depend upon:—(1.) The means of transport to the primary base of operations. (2.) The means of locomotion over the whole or the various portions of the country to be explored. (3.) The physical difficulties to be encountered from man, and from natural obstacles. (4.) The financial means available in this conflict with the difficulties, foreseen and unforeseen, of exploration.

There is much truth in the old saw "money makes the mare to go," and with plenty of money many of the difficulties of exploration are greatly lessened; yet I would here impress on you that plenty of money may be a source of serious trouble, and of much worry to the unexperienced explorer. He is tempted to buy everything he is likely to want, and so encumbers himself with an amount of baggage which he finds it impossible to transport from his base, and from which he finds it most difficult to make a selection.

It would be useless to mention in detail to a general audience the various instruments, appliances, and chemicals, which should be taken by the explorers in search of botanical, horticultural, geological, mineral, or zoological specimens. Specialists have each their individual special outfit, suitable for the purposes they have in view.

There are, however, certain articles of outfit which are necessary to every explorer of uncivilized, of partially or totally unexplored countries, their quality and quantity must vary with the nature, scope, and duration of the work to be done.

Most of the researches enumerated above necessitate either slow progress through a country, or a lengthened stay in various selected districts best suited for the operations of the specimen hunters. An explorer, bent on commercial or geographical discoveries, naturally contemplates travel over long distances, and, generally speaking, with less physical and fewer natural obstacles to be overcome in proportion to the distance to be traversed. On the other hand, however, he generally has to travel, and indeed to live, in a continual state of preparation for defence.

The military explorer must again work on different lines. His business is to seek information in countries occupied by a hostile population, with whom, however, his nation is not necessarily at war. He must travel unostentatiously, almost

alone, and must avoid all hostile contact with the inhabitants. Such were Colonel Burnaby, when he went to Khiva, and Captain Gill, during his explorations along the Persian frontier, and his subsequent travels in the interior of Northern China. The explorations for purely sporting purposes, or arising from an innate love of adventure, require consideration equally careful, and knowledge seldom acquired otherwise than by personal experience.

For our purpose it will be sufficient for us to divide our inquiries into two different channels. To facilitate and to narrow the question, we will decide that the exploration is to be partly geographical, and so far scientific that the explorer has to report in general terms on the geological and mineral resources of the country to be traversed. There are no roads, but little timber, and that sparsely scattered, except near water, of which the quantity is small, and the quality always questionable and often bad. The rivers in drought do not exist except as chains of muddy ponds, whilst in flood they become impassable for weeks, and overflow their banks often to the extent of from three to fifteen miles on either side. In such a country game would be scarce, and could not be depended upon as the only source of animal food to the explorers. The above data are sufficiently explicit and sufficiently difficult to meet almost every case.

We must now come to consider the manner in which an exploration of such a nature is to be carried out.

1. Would it be possible to establish some one or more subsidiary bases of operations. If the reply be in the affirmative, then comes the questions—(*a*) Where shall they be? (*b*) What shall be stored there? (*c*) How shall these be conveyed thither?

Now the answers to these questions must depend upon the nature of the exploration *i. e.*, if the intention be to return to the place of starting, or to traverse a continent from sea to sea.

2. What is the nature of the transport to be? If wagons, are they to be light (though strong), many in number, and of different sizes, or are they to be few in number, heavy and solid in construction? How are they to be drawn—by oxen, by horses, or by mules? If wheeled transport be out of the question, what are the pack animals to be, camels, horses, or mules, or some of each of these animals?

Each and every one of these questions has to be carefully considered, because on the solution of one question so many others must depend. It may be well here to enumerate some of the chief circumstances which tend to govern the choice of transport.

1. Nature of the soil generally. If the country to be traversed be very broken in character, covered with thick forests, and known to be traversed by sluggish streams with deep slimy banks and bottoms, it is clear that wheeled transport, unless of a very special character, would not be suitable. Neither would such a country be practicable for camel transport; and yet there can be no doubt but that more stores can be easily carried on wheels, and by camels, than any other way by land. There are, however, very few countries in which exploration with wheeled transport may not be carried out, provided time be no object, and plenty of patience and perseverance be available. This brings us to the consideration of the general outfit of an exploring party.

1. As to stores.
2. As to the mode of transporting them.
3. As to the construction of the wagons, the pack-saddles, harness, &c.
4. As to the mode of packing them and storing them.

1. As to the stores. These must be sub-divided under the headings of (*a*), provisions for the mouth ; (*b*), materials for obtaining food, or for offence and defence ; (*c*), materials for facilitating the locomotion.

In the choice of provisions, care must be taken to select such articles as are wholesome, nourishing, small in bulk, and not liable to deteriorate by keeping. There must also be variety, so as to promote health, and a proper proportion of such articles of consumption as would diminish the risk of scurvy.

Of meat the best sort is preserved beef in tins. There is very little to choose between that preserved in Australia and that preserved in America, north and south. The tins should not be too large, and they should be rectangular and not cylindrical in shape. Essence of Beef

(Brand's or Liebig's, in tins or in skins) is a most valuable form of meat. Flour and oatmeal should be packed in block tin boxes, of various sizes, containing from 1 lb. to 4 lbs. each. Sugar should be cane sugar, powdered, and packed in ½ lb. rectangular canisters. Tea—the best for the purpose is Goundry's compressed tea; it is manufactured in tablets of ½ lb. in weight, and subdivided like chocolate tablets, into eight portions, one of which is ample for tea for three or four people. Being wrapped in lead paper, it stands any climate, and I have known it to keep good for five years. Salt should be kept in stone or thick glass jars, with screw or cork-lined stoppers. Lard should always be taken, and should be kept in stone jars, capable of being rendered air-tight. There is an excellent form of compressed and dried vegetable tablet manufactured in France; and there is also a preparation of dried potato, in powder. No expedition should be without these to keep off scurvy, that terrible scourge and bugbear of all explorers. Ginger, peppers, red and black, should be carried in thick glass pickle bottles, with air-tight glass stoppers, edged with cork. Brandy for medicinal purposes should be carried in small wooden kegs, covered with thick felt, and with a locked covering to their bungs. A provision of lime juice should be similarly carried in kegs of different sizes. There should be several spare kegs of the same description, kept constantly filled with fresh water. In addition to this provision of water, each animal should have a canvas water-bag slung by a strap round his neck. These bags keep the water cool, and each should have the neck of an old soda water bottle sewn into the orifice used for filling it; the vessel can thus be easily used, without detaching it from the animal carrying it. Water-bags on the same principle, only much larger, are made of well-seasoned leather, and are slung by straps and iron rings on to a pack-saddle. At first the water has a nasty flavor; but the bags soon cease to affect the taste of the water, and are indispensable on long waterless marches in a hot climate.

Whilst on the subject of water, it may, perhaps, not be out of place to impress upon you the necessity in observing the greatest care in the selection, and, generally speaking, the after treatment of water. As a general rule, the only water which can be drunk with safety, without fear of evil consequences, is that which springs directly out of the ground, from rocks, or which is obtained from a permanent running stream, the bed of which is not muddy, and on the banks of which there is not an exuberant vegetation. Even in the case of water issuing from rocks, care must be taken to avoid water issuing from copper or lead-bearing rocks. In these cases a small quantity of sulphuric acid would at once detect the presence of the mineral in dangerous quantities, as the water would become discolored.

In most countries subject to drought, the water requires special treatment; mechanical filtration is seldom practicable, or even safe. I have come across it as thick as pea soup, and sometimes covered with a growth of green or red weeds. In such cases, the first operation is that of skimming with a skimmer made out of a forked stick, with a pocket handkerchief or other piece of linen stretched tightly between the forks. This done, scatter a pinch of powdered alum into the vessel in which you have collected the skimmed water; this will cause a great deal of the matter in suspension to precipitate. Then pour the water slowly into a filter filled with the charcoal of your last night's camp fire, mixed with any sand or fine gravel which may be obtainable, and which you have previously washed. It must then be boiled, and skimmed whilst simmering, and only when no more scum arises on the water is it really fit or safe to use. It is a good plan always to fill the kettle—or, still better, the cooking pot—with water the last thing at night, and put it at the edge of the camp fire to simmer (not to boil), and always to fill up the water kegs and bottles from what is left over from each morning's cooking. It is also a good rule never to drink plain cold water in the tropics. Each man should carry in his pocket half a handful of oatmeal, and put a pinch into his pannikin of water when he fills it for drinking.

I once traveled 1,400 miles across a portion of the center of Australia, and began my journey after a drought which had then lasted eighteen months, and which only broke up the day I reached

the sea coast. It was only by the strict enforcement of these precautions that (under Providence) I never had a case of illness from fever or from dysentery. Personally, I always carried in my pocket a few "thirst lozenges," which are, I believe, nothing except a compressed form of Lamplough's Pyretic Saline.

Before closing the enumeration of the *provisions de bouche*, it is well to add a list of the medicines and surgical instruments necessary to every expedition:—Rhubarb, essence of ginger; about 100 pills of colocynth and henbane; about double the quantity of quinine pills, made up in small doses of three grains each; some opium pills; a couple of bottles of Dover's powders; four bottles of sweet spirits of nitre; about 100 pills of podophyllin in small doses; camphor, and chlorodyne. Two lancets, two abscess knives, two catheters, two enemas; some surgical needles, and some silver wire thread for sewing wounds; a silver probe, and two vein or artery forceps; a syringe with various nozzles for various uses. Sticking-plaster of various sorts, and some prepared lint and medicated wool; and some vaseline, carbolic acid, and carbolic soap. All the medicines should be in glass-stoppered bottles, the stoppers having been lubricated with pure glycerine previous to insertion. The medicines, &c., should be divided into at least three portions, so that each wagon, or each detached party, should have a complete set of everything. There is no greater mistake than to have everything in one medicine chest. All boxes should be avoided, as in a very damp or a very dry climate boxes are apt to come to pieces with the rough handling that every package gets at the hands of those who often have to do the packing and unpacking of animals two or three times each day.

Clothing.—Take as little as possible when starting from England, as you can get most articles necessary for explorers at the place from which the wagons would make their start. Of personal attire, the following are those which I consider sufficient for most expeditions:—Four shirts made of grey flannel, with two buttons on each wristband, to admit of them being worn loose or tight. Four long merino drawers, double seated and double down the inside of the thighs. Four pair of thick knitted woolen long stockings. Two cholera belts, one of knitted worsted, the other of flannel about a quarter of a yard wide and three yards in length, to be wound round the body or fastened with a safety brooch. Six silk pocket-handkerchiefs (white), and of the thickest and best quality. They are useful sometimes when traveling in the very early morning, to serve as a curtain against the sun's rays, which often at that hour strike with great force on the nape or side of the neck under the hat. A Norfolk jacket of good woolen serge or light tweed, made double-breasted, so as to be worn either open with the lappels buttoned back, or buttoned across double over the chest and stomach. It should be made like a garment known by miners as a jumper, not cut in at the waist, but merely kept in at the waist by a belt. This belt should be made of two pieces of soft leather, about 2½ inches wide, and stitched together at the edges so as to admit of dollars or other coins being kept in the belt and slipped in at either end, and prevented from falling out by a flap and button at each end.

If a sword has to be carried, it is best carried fastened on to the side of the cantle of the saddle by a round strap and button of leather. If a revolver has to be worn, it is best carried in a frog supported by a webbing belt over the right shoulder, which should be kept in its place by the waistbelt.

The best hats are of gray felt, of a helmet shape, with means for ventilation round the edge and at the top. They should be provided with a chin strap, to be worn when riding fast or against a strong wind. The best boots are those known as the Paliser boot. They reach nearly to the knee, and are laced up for about six inches from below the instep, so that the boot can be always easily got on and off, whilst remaining watertight. I prefer those made of porpoise hide to any other, as they are lighter and more supple in wear.

Dogskin driving gloves should always be taken, as their use prevents sun boils, blisters, and many sores arising from thorns, &c., on a journey. Breeches should be made very loose, except just below the knee, where they should be fastened with a buckle and strap, or tied with thongs of porpoise hide. A

hunting whip with a hammer handle and a long brown leather lash is always useful, and is a necessity where there are many spare horses to be driven along with the party.

Camp Necessaries.—India-rubber buckets, two to each wagon, should always be carried, to be used for watering the horses whilst in harness. Palkee hammocks, made of waterproofed canvas, are the best and most portable form of bedstead, and it is always unadvisable to sleep on the level of the ground. The blankets should be loosely sewn together round three sides so as to form bags. This plan saves many a sleepless night. Moreover, it keeps snakes from getting in between the blankets. A waterproof sheet, with eyes round the edges, is most useful, as when thrown over the ridge pole of the hammock it can be lashed to the sides of the hammock, and serve as a complete shelter even in the heaviest storms of rain and wind. A light folding chair, or if this be too large, a beach seat with a back, is a great luxury, and is almost a necessity in wet ground.

We have now to consider the selection of such materials as are necessary to secure supplies of fresh provisions to protect life. First and foremost are guns. These should be breech-loaders of the simplest possible construction, and of 12 bore. Each gun should be provided with 20 steel cartridges. These are really indestructible, and are very easily reloaded and recapped; and having a female screw turned for a distance of an inch inside the cartridge, there is no difficulty in making the wads to keep in position.

For ammunition, shot of all sizes should be taken, the larger slugs for use against man or large animals. Powder should be carried in two small copper magazines, each containing about 7 lbs. of powder in half-pound canisters, fitting into the outer cylindrical copper case. These canisters should have screw tops with leather washers to them. The canisters should always be kept full so long as there is any powder in them. When a canister cannot be filled with powder, it should be filled up with cotton wool, rags, or even crumpled up soft paper. It must be remembered that any expedition is liable to be reduced to pack animals only, and then the attrition is so great that everything which can rub, soon gets rubbed to the finest dust.

When it is known that there are rivers or lakes, it is well worth while to take a casting net, and even a small Seine net of strong tanned twine. A large provision of hooks and fishing lines of all sizes should always be taken, as they are not only useful in the obtaining of a change of diet, but are very valuable as an article of barter with natives.

For personal defence the best weapon is the largest sized Colt's revolver, with a stock which can be used at the shoulder, and is detachable. When on horseback it is best carried in a bucket, like our cavalry carry their carbines. A good one shoots with wonderful accuracy up to 100 yards. A hunting knife, of a pattern of my own, I have found the best, as it is light, and yet strong enough to cut away a fairly large branch. The sheath is of bamboo, and there is room in it for a knife and fork of steel, flat, with wooden handles screwed on each side of the shaft. The blade of the hunting knife is made light by having two deep grooves cut out of the thickness near the center of the blade, so that, whilst the blade is made lighter, it is also thereby rendered much stiffer.

The only other stores which we have to review are those required in reference to locomotion—*i. e.*, spare harness, leather, rivets, and copper wire for repairs, spare nuts, screws, iron clips, splinter bar caps; tools, such as augurs, center bits and braces, saws, files, chisels, screw wrenches, screw-drivers, gimlets, awls, sewing needles, wax and strong thread, felt for saddle cloths, roll of flannel for saddle linings, raw green hide, and skins of dried leather, half-inch iron rods, flat iron hooping for strengthening splicings, adzes, jack planes, spoke shaves, sharpening hones, files, punches, rasps, horse shoes, nails, and shoeing tools, felling and trimming axes, crosscut saws, hand saws of three or four different sizes, from 3 ft. 6 in., to 15 in., clamps, light and heavy hammers, a few pairs of blacksmith's pincers and tongs, a couple of good bellows (hand), and an assortment of nails, screws, copper and steel, D's, buckles of different sizes, and straps of various lengths and widths.

Having enumerated the stores necessary to an expedition, the next thing to

be settled is the means of their transport.

It is rare that the only transport available is man, but yet in some tropical countries, covered with thick jungle, and where the ground is too rocky and broken even for mules, donkeys, or cattle, it is necessary to use men, and men only, for this purpose. Chinese and Japanese coolies will carry great weights balanced on two ends of a long bamboo cane, which rests on the shoulders. Sometimes two men will carry a heavy package for weeks at a stretch, slung on a bamboo cane between them. In Africa and South America, the natives prefer to carry heavy things on the top of the head. When packages are carried slung, the slings should be made of plaited ropes of green hide, kept well greased. Everything else wears out almost immediately. Every expeditionary force should be provided with pack-saddles, and with the means of constructing them. Personally I prefer the sort in use by the Basque population in the Pyrenees. It has the advantage of simplicity and cheapness of construction, and of being easy to use and to repair. The best form of camel pack-saddle is the one used by the Arabs, who contract with pilgrims to and from Mecca. Numnahs of felt should always be used, both with saddles and pack-saddles. If carefully adjusted, they admit of animals being kept in work with sore backs, should it be necessary.

The best form of bridle for all animals are those made entirely of tanned twine webbing. It is quite impossible to break them, and they are comfortable both to the heads of the animals and to the hands of the rider or driver. Besides this, they do not become slippery in wet weather, neither do they require any care to keep them in working order, as leather does in hot or dry climates.

Hitherto, we have treated entirely of man or of animal transport; but there are very many countries where it is not only possible, but very advisable, to adopt wheeled transport.

The class of wheeled transport must depend upon three conditions.—1. The nature of the country to be traversed (I put roads as out of the question). 2. The quantity of stores to be carried. 3. The quantity and quality of the animals available for its traction.

It is now almost an exploded idea that a wagon must of necessity be a heavy, cumbersome vehicle, with thickness and weight as the governing virtues of all its component parts. The Boers and others in South Africa still adhere to the old pattern, from habit and ignorance more than for any known reason. In America and in Australia, where the country is just as difficult to traverse, very much lighter vehicles are used with great success.

It is well to have several sizes and types of vehicles in every expeditionary outfit. Two-wheeled carts, long and broad, with draught from the shafts and outriggers at the sides of the shaft, which would admit of its being drawn, if necessary, by three horses abreast; four-wheeled wagons, light and medium, with pole draught, with side springs, and india-rubber buffers on the axles, these latter being connected by a perch. All wagons and carts should have lever brakes, capable of being worked by hand and foot by the driver. There should also be iron skids, or shoes and chains to be used if required, in addition to, or to replace the brake. The chief things to be borne in mind in the construction of vehicles for expeditionary transport are—

1. Great simplicity of construction.

2. As few parts as possible.

3. Screw clips should be used in preference to bolts and nuts, inasmuch as every bolt weakens the wood traversed by it, in proportion to the diameter of the bolt.

4. All parts should be made of such shapes that they can be readily copied and replaced by an unskilled workman.

5. The wood should be perfectly seasoned, neither so dry as to diminish its toughness, nor too full of natural moisture or sap, and no iron should be used except where absolutely incapable of being broken, or where the use of wood would be incompatible with strength and endurance. It may, however, be used where, in the event of its breaking, it could be easily replaced by wood.

6. The height of the axles from the ground should be the same, and not less than two feet. It is seldom that a wagon has to be turned at a very acute angle, therefore no great amount of "lock" is necessary. When making a track through a forest, much time and

labor are saved by cutting the trees off at about two feet from the ground, as they are not nearly so large in girth at that height, and it is less fatiguing to the men felling them with cross-cut saws or axes.

7. The various parts of each wagon, excepting the bodies, should be interchangeable, so that in the event of a complete breakdown, the unbroken portions of the disabled wagon could be utilized in the repairs of others. This is very essential, as tending greatly to the reduction in the quantity of spare stores.

We may, therefore, proceed to consider the construction of a wagon under the following heads:—

1. The under carriage, including the wheels.
2. The mode of traction.
3. The body (including the tilt where necessary).
4. The means for suspension of the body.

As stated under paragraphs 6 and 7 above, the parts should be interchangeable, and the axles should not be less than 2 ft. from the ground. It follows, therefore, that the wheels should be of the same diameter, and not less than 4 ft. 2 in.

One of the great troubles in all explorations, especially in very hot and dry climates, is the difficulty experienced in keeping the wheels in working order. The spokes shrink, and unless this is immediately found out and remedied, by calking the gaps left in the wheel stock and felloes with white lead and cotton waste, or with oakum, wet gets in, and the end of the spoke soon rots away. The slightest sign of looseness anywhere in the wheel must be at once attended to and remedied; green hide (cut in strips half an inch wide) wound in and out the spokes near the stock, greatly strengthens a wheel of which the parts have begun to shrink. In some very dry climates, no wheel of ordinary construction will stand. On one of my expeditions I had each night to take off all the wheels and lay them in water till daylight, in order to keep them together, and even with this precaution, the wheels eventually turned inside out and fell to pieces. There is, however, a form of wheel which seems to me to be likely to last longer than the sort in ordinary use. It is that known as the Madras pattern, and the invention is claimed by an American named Sarven. The spokes fit round an iron stock, and are kept in position by two circular plates, bolted from outside to inside the wheel. This would admit of a broken or damaged spoke being easily replaced, or they could be wedged up from the center by the insertion of thin bits of iron, tin, or some hard substance, between the inner end of the spoke and the stock. Another difficulty arises from the difficulty of keeping the tires on. In England, it is easy enough to remedy the tendency which all tires have to lengthen. They can be cut, shortened, and reshrunk on the wheel. In exploring work, the tires, as a rule, do not permanently increase in circumference, as they do from use on hard roads here, but the wheels shrink away from them with the heat, and this same heat expands the iron tire, and so causes it to lose its contact with the felloes.

The evil results arising from these causes may be minimized in two ways:—

1. By constructing the tires slightly convex on the inner circumference, and by making a corresponding concavity in the outer circumference of the felloes.

2. Sometimes, however, the shrinkage is so great, that it becomes necessary to cut and shorten the tire. As it is almost impossible to secure a good weld to reclose it, it has to have the two ends filed to a feather edge, brought together, and then firmly clipped to the felloe at either end of the splice. The tire may be wedged tight, and secured with clips.

The axle-trees should be of the best toughened iron, bedded in tough timber, and clipped. In length they should be 3 ft. 8 in. to 3 ft. 10 in.

The greater the breadth (in moderation) the greater the stability of the vehicle when moving across an incline. Moreover, with all the wheels of equal diameter, the lock is increased by leaving a greater space between the wagon body and the front wheels. The axle-trees of the fore wheels and hind wheels should be connected by a straight perch made of tough wood, such as hickory. Inasmuch as there is very little "lock" required, there is no necessity for any complicated or delicate wheel-plate (or fifth-wheel). A stout transom, with an iron

eye through which the king bolt would pass, and the axle-bed connected with the splinter bar by two wheel-irons, and braced at the rear by a stout sway bar, is all that would be required. These should be all straight pieces as far as practicable, and clipped together (not bolted). In very broken and precipitous ground the pole might be taken out, and the movement controlled by ropes held by men.

As regards traction, it would be a great gain if the pole could be dispensed with, inasmuch as in very rough ground it knocks the wheelers about sadly, and it is more frequently broken when working in difficult ground with untrained horses and bad drivers than any other part of a wagon. It is, however, sometimes necessary, and must therefore be provided for. It should be attached firmly to the splinter bar, and the bar allowed to move freely. The attachment should be by means of two iron bars passing through eyes clamped on to the splinter bar at the two ends, ending in two iron stays coming out from the bars at an angle of about 20 degrees, and clamped on to the pole. The pole is thus worked freely up and down, and the pressure would be taken off the jaws of the futchells by the two jointed iron bar stays.

The hanging pole, moreover, necessitates a contrivance to relieve the horses from having constantly to support its weight. This can be done by having a strong hook, fastened by a clamp, at about one-sixth of the length of the pole, from the splinter bar. On to this hook is fastened a chain, or strap of plaited raw hide, which, running through a sheave (firmly fastened by a broad plate bolted on to the footboard), is hooked at its other end on to a hook fastened to the under side of the front of the body of the wagon. These hooks must be strong, and have a broad bearing where fastened to the carriage body. It would even be advisable to introduce a spiral spring at one end of the chain, to take off the sudden strain occasioned during the passage over very rough ground.

When driving four or more half-trained horses on broken ground, it is safer to use no pole piece and bars, but to use long traces made of green hide rope, kept up by loops hanging from the wheeler's trace carriers, the leader's traces being kept apart by means of very light hickory bars, kept from slipping by green hide thongs passing through the ends of the bars, and fastened through loops in the leader's traces. It is well, however, to be able to use the pole and bar draught; with that view, the pole piece should be fastened by clamps, counter sunk round the pole head. The hook should be made on a twist, to avoid the necessity of using a strap, as with wild horses it is necessary to be able to detach the leaders with as little delay as possible.

Germane to the subject of traction is the question of how to bring it into control. The ordinary skid or shoe cannot be depended upon in rough, rocky ground, as the wheel is apt to jump out of the shoe. The ordinary hand brake, acting on the front of the hind wheels, is insufficient. To these two should be added a friction brake working on the hinder circumference of the hind wheels by means of a bar, shod at the two ends, which can be compressed against the wheels by a screw working on the end of the perch, prolonged for this purpose.

If the tires should be secured by clips at any part of the journey, the projections would interfere with the brake blocks, so the brake blocks should then be applied with enough pressure to prevent the wheels revolving.

Suspension.—If a very rough country has to be traversed, it is well to have the body of the wagon suspended on springs, so as to save the damage done to the stores, as well as to the wagon by the jolting.

The best form of spring appears to me to be that adopted by some of the best carriage makers in the construction of gentlemen's omnibuses for station work with heavy loads.

The springs are single, and coupled to the scroll iron on the body by a shackle, inside which is an iron coupling or robin. These are practically unbreakable, as the coupling takes off the strain from any sudden and heavy jolt. There should be, however, india-rubber buffers fastened on to the body to minimise the shock, if it were to be so severely jolted as to come down suddenly on the bed of the spring. I have found it very useful to have a strong swinging tray (made of strong ash planks one inch thick) fastened so as

to hang between the axle-trees. The planks should not be too close together to prevent axes, spades, picks, and such like heavy articles being attached to the planks by means of thongs tied round the planks, and passed through holes in the handles of the implements. The whole tray should be constructed that it can be readily taken to pieces, and the planks utilized in the passage of boggy grounds, or in the sandy beds of rivers, or in running the wagons up very steep inclines in soft ground. I have found them of great use, especially in deep ground, where they can be put under the wheels. Another advantage arising out of the use of this tray is, that as in it are placed heavy articles, the center of gravity is brought lower than if the same weights were carried in the wagon itself. There should also be a small water barrel, covered with felt, hanging under the wagon at the rear.

Covering.—Every explorer's wagon should have a tilt, to serve as a shelter from sun and rain. It should be made of waterproofed canvas, and have a fall-down piece in front to shelter the driver, and a curtain behind, with thongs to enable it to be kept closed when needed. The framework is best made of hickory, fitting into rectangular sockets well outside the framework of the body, so as to allow of ventilation from under the sides, and to give greater head-room space in the interior. There should also be a ridge pole of hickory running through rings clamped on to each rib. This ridge pole can be utilized for slinging a hammock in case of sickness or wounds during the march.

Fittings.—Under the driver's seat should be a movable box, in which to place all the tools and materials necessary for mending the harness, or any part of the wagon. The box should be constructed in trays, so that each thing may have its place, and be readily available. Each wagon should have its camp kettle, which should be slung on hooks under the rear of the body. On the splash-board there should be hung a stout leather bag, in which might be kept strong twine, a sharp knife in a sheath, and a hatchet and hand-axe for ready use. Each wagon should have a strong lantern for use, with good wax candles.

Harness.—The great desideratum is to have as little harness as possible, and that it be strong without being heavy. Headstalls and bridles may be made of stout webbing dipped in tan. The reins should be round, and of plaited green hide. There should be as few buckles as possible, and the ends in the driver's hands should never be buckled, but merely kept together by a loosely made reef knot, which can easily be undone in the event of its being necessary to let the leaders go clear. The traces should either be made of plaited raw (or green) hide, or of the best two-inch rope.

It is well to be provided with both collar and breast draught, so as to be able to change from one to the other form of draught in case of need. Copper rivets and copper wire are most useful for mending harness and saddlery, and plenty of it should be with the stores. There should also be plenty of hobbles to prevent horses from straying too far from camp at night in search of feed. Some horses, however, become so clever in hobbles that they can even gallop in them. In such places the best plan is to attach a cord from the head collar to the hobble of one leg. It is well to have a few cattle bells to attach to the necks of some of the horses most likely to stray. By these means much annoyance and delay in starting are to a great extent avoided.

Horseshoes.—Although in most expeditions the horses are not shod, it is wise to take a small supply of shoes and nails, to be used in the event of it being necessary to cross a tract of stony or rocky country, where horses would soon wear down their feet, and become tender-footed and useless. The class of shoe must depend upon the breed and class of horses used. The Arabs, who ride their horses over very rocky and stony ground, most frequently shoe their horses with plate shoes, covering the whole of the sole; but this form is not suitable to a wet soil or a stiff clayey country. Every party should have a blacksmith amongst its members, and it is well that most of the party should be able to shoe a horse without driving the nails into the quick.

Packing the Wagons or Pack Animals.—There are certain principles in packing, whether it be wagons or pack

nimals, which should never be lost sight of.

1. To make each wagon or group of pack animals complete in itself, *i. e.*, it should contain everything necessary to the existence of those in charge.

2. So to arrange the stores that those most frequently used should be so packed that nothing else need be disarranged in order to get them out.

3. To arrange the stores in such a manner that the heavy packages should be equally distributed over the surface of the wagon, or amongst the beasts of burden, and that the lighter articles should always be so well secured as to prevent the possibility of their becoming loose, and thus spoiling their contents.

I have known hard biscuits reduced to powder by the omission to pack the case with paper, so as to keep the box always full; clothes worn into holes by attrition from their having been placed in contact with hard corners; maps, and even books, destroyed in the same manner.

Now, as regards the packing of animals, it is quite impossible to do more than lay down first principles, viz.:—

1. That the panels of the pack saddle must be well and evenly padded; this should be looked to at every halt and promptly remedied, otherwise sore backs will ensue.

2. That the weights should be quite evenly divided on either side of the saddle, so as to avoid the necessity of having to draw the girths too tightly, or of having to stop frequently to re-arrange and trim the burdens.

3. The weights should be kept low, so as to lower the center of gravity as much as possible. This is especially necessary when any mountainous country has to be traversed.

4. The packages ought not to stick out too much laterally, especially when wooded country or a narrow rocky pass has to be traversed.

5. Where practicable, it is best to put some soft or yielding package outside the others, as the pack animals often run against one another, and damage in such cases might arise both to the animals and to the packages, if the latter were hard and unyielding. Moreover such a plan enables the surcingles to be better arranged.

6. Never attempt to pack an animal alone. The weights having been arranged on the ground, the animal should be led between them, and the packages should be placed on the hooks simultaneously.

7. The same precautions should invariably be taken when unpacking, as at that time it is so very easy to "wring" and to "rick" an animal's back.

8. At every halt of more than an hour the packs and pack saddle should be removed, and, where practicable, the backs should be washed with salt and water, alum and water, or carbolic soap and water, then rubbed dry; and just before repacking, the back should be brushed with a penetrating bush, to remove all grit, sand, or dander, as almost all horses, mules, and asses, roll on the ground as soon as their saddles have been removed.

9. After each day's march, the back of every animal should be examined, and the slightest tenderness or shrinking observed. The smallest sore or abrasion should be carefully washed with carbolic soap, and dressed with vaseline ointment. If there be no spare pack horses, and rest be an impossibility, then a numnah of thick felt should be interpolated between the back and the saddle, and a hollow, or even a hole, cut in the numnah, to prevent any pressure coming over the sore place.

The same treatment should be observed with respect to the shoulders and withers of the harness animals, remembering the old proverb, "a stitch in time saves nine."

There should be an intelligent, capable man in charge of all the wheeled transport, another in charge of the pack animals, and a man in charge of the spare and sick horses. Each driver should be responsible for his wagon and team, and there should always be a mounted man with the wagons, and with each detached wagon. There should be a cook in whose charge all the stores should be, and he should ride, if possible, so as to go forward with the advanced party, to light the fires, collect the wood, and, where necessary, improve the water supply. He should carry a hatchet and a small spade.

The man charged with the supervision of the sick and spare horses should have another man with him, as it is often

necessary, especially at the commencement of a journey, to leave a man behind to search for and bring up horses which have strayed, and sometimes even gone back from the camping ground.

In every exploration where wheeled transport is employed, there should always be a reserve of a class of horses called "emergency" horses, *i. e.*, horses able and willing to give a steady and strong pull. They should be well bred, strong horses, and should not be used except when required in heavy ground.

In countries where the water supply is uncertain, it is the best plan to send two men ahead with a spare horse, to explore for water. When found, one of the men returns on the spare horse.

As a rule, from ten to fifteen miles may be considered an average march in a new country, in which there are no physical difficulties. I have, however, more than once, only been able to progress two miles in the day; whilst in order to reach water I once had to make, in three successive days, marches of forty-one, forty, and forty-three miles. These were, however, made with pack animals, and without wagons. Before concluding this paper, it may be interesting to most of you to hear a few remarks on the manner in which exploration for water is generally conducted. Experience, and even common sense, tells us that in a hot or a dry climate, animals and birds are but very seldom found far from water towards sunset, and that at sunrise they generally leave the vicinity of water on their search after food. Observations as to the direction of the flight of birds, and especially of all the parrot tribe and the carrion birds, will generally lead to the discovery of water.

In almost every country there are some descriptions of shrubs and trees which will not grow except in the vicinity of water; and even where this water may not be obtainable on the surface, it can, under such conditions, be found by sinking in suitable spots in the beds of the streams where those shrubs or trees are found. I once traveled for three days down the bed of a river which was quite dry, and yet by sinking from six to ten feet in the bed, a sufficient supply was obtained. It seems to be a provision of nature that in very hot and dry countries the streams almost invariably run for considerable distances under ground. With a very rudimentary knowledge of geology, and by the observance of the natural signs of water peculiar to each country, travelers may, and do, often find water where an unobservant man might die of thirst. This all-important question is of more interest than usual at the present time, when an expeditionary force, composed of European troops, is about to undertake the opening up of the trade route from Suakim to Berber, on which the last two stages, fifty-three and fifty-two miles respectively, are without any visible supply of water. Personally, I have but little doubt, from the geological formation of the country, and from the conditions of the water supply along the rest of the route, that these two dry stages will be bridged over by the discovery of a subterranean supply of good water Let us hope that capable men may be employed in the exploration of that part of the route, and that our expeditionary forces on the Nile and the Red Sea may be able to join hands at Berber, and thence proceed to re-establish the prestige of British arms in the Soudan.

THE MANUFACTURE OF "COCOA" POWDER.—The War Department have adopted the new brown gunpowder known as the "cocoa" powder, and it is now being manufactured at the Government Factory, Waltham Abbey, on the principle introduced from the United Rhenish Westphalian Powder Mills, Cologne. Some trials of this powder have taken place at the proof grounds in the government marshes, Woolwich, under the direction of Major Hemans, Royal Artillery, the proof officer, and in the presence of Mr. E. Kraftmeier, the representative of the Westphalian works. Two thousand cases of this description of powder have recently been purchased, and samples have been fired in an 11-inch breech-loading gun. Ten rounds were fired to test the pressures and velocities, the charge of powder being 295 lbs., and the weight of the projectile 655 lbs. The pressures were taken at five positions within the gun, and the velocity, as usual, at the muzzle. The mean pressure was 16.5 tons per square inch, which is two tons lower than stipulated, and the whole of the velocities were between 2,002 feet and 2,010 feet per second. The pressures were also exceedingly regular, the highest being 17.6 tons, and the lowest 16.3 tons, while the mean variation in velocity was less than 2 feet. The brown powder creates a very thin smoke, and the committee at the School of Gunnery, Shoeburyness, have reported that it does not obscure the targets.

THE RESILIENCE OF STEEL.

By WILFRED LEWIS.

Proceedings of the Engineers' Club of Philadelphia.

The problem of storing energy in a convenient shape for transportation or domestic use, suggests a wide range of possibilities, and opens up a large field for the exercise of inventive genius.

A good solution of the problem is urgently needed and as earnestly sought, but, as yet, all efforts seem to have failed to accomplish practical and economical results.

That continued labors in this direction will finally be rewarded by success, is certainly within the bounds of possibility, but meanwhile it appears as though everything that will not work must be tried first, and that a vast amount of knowledge must still be gained by sad experience and disappointment. The subject of this paper was suggested by a case in point, the proposition being to utilize the energy stored in a number of steel springs for the propulsion of street cars.

The question which naturally arose, was: how much energy can be stored in a given weight of steel?

The answer to this question was at first sought from the data given in standard works of reference, but these were found to be so meager and indefinite, that the writer was led to make some experiments, to be described. Before going into particulars, however, it will be of interest to note some of the various ways in which energy can be stored, and the comparative position of steel among them.

This has been done by a writer in the "English Mechanic and World of Science," for November 2d, 1883, who compares the methods of storing energy by means of steel springs, India rubber, compressed air, hot water, and electricity, and expresses the results in terms of the weight of material required to store one horse-power per hour.

Of steel, he says that fifty tons of watch-springs, all fresh wound up, would not supply one horse-power for one hour.

Five or six tons of India rubber, or about three hundred pounds of compressed air, including the weight of a steel case to contain it, would yield about the same result, and the weight required to store this energy by means of hot water or electricity, is said to be at present about the same as for compressed air, with the future probabilities in favor of the storage battery. He also goes on to show that the energy stored in the shape of horse flesh, will yield 2,000,000 ft. lbs. per hour for five hours. The weight of the horse being taken at 1,500 lbs., gives for the energy stored about 6,670 ft. lbs. per lb. of the animal, and as this is a more convenient form of comparison, we find for the previous examples, that 18 ft. lbs. can be stored in a pound of steel; 100 ft. lbs. in a pound of rubber; 6,600 ft. lbs. in a pound of air, including its steel case, and about the same in a pound of hot water or storage battery.

According to the same writer, the energy stored in the coal, water and boiler of a locomotive, will yield about 25,000 ft. lbs. per pound of all the materials used in storing.

From these statements it appears that steel has comparatively but little capacity as a reservoir of power, although, as is well known, it has long been used successfully and even preferably, for such light work as the running of clocks and toys, where convenience and availability are the main points in view. If the figures just given were accepted as correct, it would seem hardly credible that any one would attempt the task of propelling a street car, upon the basis of 18 ft. lbs. per pound of material used for driving, but upon investigation it was found that considerable work had already been done upon an experimental car, for the purpose of having a practical test.

The invention is described in *The New York Scientific Times and Mercantile Register*, for December 15th, 1883, as a "wonderful system, by which horses on street car lines will be abolished."

The working parts will be of phosphor-

bronze, polished, and there will be 80 steel springs, each 3 inches wide, by $\frac{3}{32}$ inches thick, and 60 feet long, which, it is said, can be wound in two minutes, by a stationary engine at the depot.

This may be very quick work, but what is more to the point. the figures are given to show that by the use of this system, the car companies can carry passengers for three cents, and make as much money as they now do at the present rates.

In the case of the springs running down before reaching the station, a wise provision has been made by the introduction of a "powerful hand-winding arrangement, so that the engineer can apply the arrangement while the car is in motion, and thus reach the station without delay," but, unfortunately, no provision appears to have been made for winding up the engineer.

"The car is also provided with an ice attachment for winter use, a governor to regulate the speed, and about sixteen hand-levers, for various clutches, all conveniently and easily handled."

Upon inquiry it was not surprising to find, that although considerable work had been done upon the polished bronze portion of the car, no experiments had yet been made to determine the duty of the steel springs in question; and having become interested in this part of the subject, as a matter of scientific importance, I undertook the following investigation to determine the possible resilience of steel:

The elasticity of a steel bar may be developed by extension, compression, torsion or flexure, the latter being a combination of the two former.

All steel springs are brought into action by torsion or flexure, not because any more work can be stored by these methods, for in reality there must be less than by either extension or compression, but because the forces involved are more manageable.

In flexure and torsion, the metal is strained in proportion to its distance from the neutral axis of the section, and therefore, the full amount of elasticity is developed only in the extreme fibers, but in direct tension or compression every particle of metal must yield its full share of duty.

It is impossible to determine what this may be under the most favorable conditions, because it may never be known when those conditions are reached. Everything, of course, depends upon the quality of the steel, and its physical treatment. No satisfactory data upon these points could be found; the effect of hardening was said to increase the elastic limit and ultimate strength, and to diminish the elongation and reduction of area, but the effect upon the modulus of elasticity was not given in connection with these other changes.

This modulus is given by Rankine, at 29,000,000 for soft steel, having a tenacity of 90,000 lbs., and a working strength of 30,000 lbs., and at 42,000,000 for hard steel, having a tenacity of 132,000 lbs., and a working strength of 44,000 lbs.

We have also, upon his authority, that within the elastic limit, the modulus for compression is sensibly equal to the modulus for extension.

Prof. Burr, in his work on the "Elasticity and Resistance of the Materials of Engineering," gives a table showing the effect of tempering upon the elastic limit and ultimate resistance, and the highest values there given for the elastic limit are 58,350 lbs. before tempering, and 107,650 lbs. after tempering. The ultimate strength corresponding, is 110,340 lbs. before, and 169,430 lbs. after tempering.

These results were obtained from a mild grade of French steel, and, although this elastic limit is probably above that of average steel, it is of course below what might be expected of higher grades.

Assuming Rankine's figures for hard, untempered steel, we find that its resilience is 7 ft. lbs. per lb., under direct tension. That is to say, any given quantity of untempered steel is just capable of storing enough energy to raise its own weight, through a distance of 7 ft., and that a car without weight driven by such a spring, without any loss through friction, could not quite manage to ascend a hill eight feet high, without the assistance of the engineer.

Supposing the elastic limit to be 107,650, instead of 44,000, as given by Rankine, we have about 43 ft. lbs. per lb., a much better result, but still insignificant in comparison with horseflesh, or compressed air.

According to Rankine's formulæ for spiral steel springs, the torsional resili-

ence of steel is greater for the same intensity of stress than that of direct tension or compression. This is due to the lower modulus of elasticity for torsion, which prevails against the loss from incomplete straining of the fibers. The modulus for shearing is given at 12,000,000, and assuming 44,000 for an elastic limit, we have 12 ft. lbs. per lb. of metal for the torsional resilience of steel springs.

In order to put these calculations to a practical test, I procured two door-springs, of $\frac{3}{16}$ inch wire, and about $3\frac{1}{2}$ feet long. One end was held in a vise, and, upon a lever attached to the free end, weights were suspended until a deflection of 180° was produced. The first specimen, which was $37\frac{1}{2}$ inches long, deflected 90° with a weight of 4 lbs., at 15 inches rad., without taking set, and 180° with 8 lbs., at $11\frac{1}{4}$ rad., showing a set of about 30°. The second specimen gave better results. It was $40\frac{1}{2}$ inches long, between clamps, and deflected 90° with a weight of 4 lbs., at 13 inches rad., and 180° with a weight of 8 lbs., at the same rad., showing not more than 5° set. Taking the latter case, we find the greatest shearing stress to have been 80,000 lbs. per square inch, and the resilience about 43 ft. lbs. per lb. Substituting these results in the general formula for torsional deflection, we find the modules in this case to have been about 10,000,000, which corresponds closely with the values generally given.

Dividing the general formula for torsional resilience

$$R=\frac{\pi}{16}\frac{f^2 d^2 l}{C} \text{ by the weight } W=\frac{.28\pi d^2 l}{4}$$

and reducing to ft. lbs., letting f= intensity of stress, and C= modulus of elasticity for distortion, we have, for the energy developed per lb. of steel,

$$E=\frac{.075 f^2}{C}$$

The ultimate value of E, from these experiments is, therefore, about 48 ft. lbs., allowing 80,000 lbs. for f and 10,000,000 for C. If the spring is strained to but half its elastic limit, but $\frac{1}{4}$ of this amount, or 12 ft. lbs., per lb., can be obtained, the same as deduced from Rankine's formula for the safe load.

In order to test still further the resilience of spring steel, I procured two clock springs wound in spiral forms. The first, which was $\frac{5}{8}$ inches wide, and .014 inches thick, weighed 605 grs., and was consequently 60 inches in length.

It was mounted upon a mandril and tested for each revolution in winding and unwinding, by a weight sliding upon an arm attached to the mandril. By this means the friction of the coils in pressing against each other could be measured, as well as any set which might occur. At twelve revolutions the spring appeared to be wound up, and it then supported a weight of 1 lb., at 3 inches rad., with a variation of $\frac{1}{4}$ inch either way in the radius. The tension was in all cases proportional to the number of turns, and no set was apparent after unwinding.

It was expected in this test that a much lower result would be found than in the test for torsion, both on account of the higher modulus for bending, and also on account of the character of the stress in developing a smaller proportion of the inherent energy in the steel. But the result is surprising, for we have without doubt, 9.42 ft. lbs. in 605 grs., or 108 ft. lbs. per lb., and assuming the neutral axis to be in the middle of the ribbon, there must have been exerted a transverse resistance of 240,000 lbs. per square inch upon the outside fibers, and this too within the elastic limit.

To satisfy myself on this point, and to be sure that this remarkable strength was not due to the combination of superimposed layers, I fastened a short length of the spring between wooden clamps, one of which was fixed so as to use the spring as a cantilever to sustain weights attached to the other. In this way the spring supported a weight of 1 lb. at a radius of 4 inches, when an apparent set took place.

The transverse strength of the steel was thus found to be over 320,000 lbs., with an elastic limit closely approaching that amount.

The other spring, which was $\frac{1}{2}$ inch wide by .022 inches thick, and weighed 2,040 grs., showed even better results. After making twelve revolutions it sustained a weight of 2 lbs., at 6 inches rad., developing 45 ft. lbs. of work, or 154 ft. lbs. per lb., and showing a transverse elastic resistance of 300,000 lbs. per square inch. The energy developed by

any layer of the spring is proportional to the square of its distance from the neutral axis, and we find by integration that the energy developed by flexure is one-third of that which is possible by direct tension or compression.

We should therefore expect to obtain from this steel, under tension or compression, 462 ft. lbs. of energy per lb. of steel, when strained up to 300,000 lbs. per square inch, and this gives for the modulus of flexure the value of 30,000,000.

This low modulus of elasticity, in connection with such a high degree of transverse strength, seems to give new life to the project which at first appeared so hopeless, but it is still doubtful whether any great success can be anticipated under this more favorable light. Assuming 154 ft. lbs. per lb., which is probably far in excess of what can be obtained on a large scale, and allowing ½ of the total weight for the weight of the springs, and 50 per cent. for the efficiency of the driving mechanism, we have about 20 ft. lbs. of available energy per lb. of load moved. On a level track or down grade this might be sufficient to run a mile, but it would be entirely inadequate to overcome ordinary grades, or to endure many stops, even if a portion of the energy were returned in stopping. The approach to Spring Garden Street Bridge, for instance, would doubtless prove insurmountable.

The experiment, however, will soon be tried, and the result, if unsuccessful, will at least be instructive. It is believed, in conclusion, that the results here given upon the resilience of steel are the highest yet recorded, notwithstanding the fact that they are constantly being realized in practice, and that the demonstration of their truth is within the reach of any one who will take the trouble to make it.

SOME GENERAL CONSIDERATIONS AFFECTING STRUCTURAL DESIGN.

BY WM. H. BURR, C. E.

From Selected Papers of the Rensselaer Society of Engineers.

THE time is even now not far distant in the past when the chief object in the design of a bridge or similar structure was considered to be the proper determination of the area of cross section of the ties, posts, and upper and lower chord members; and what was twenty-five or thirty years ago a problem of no ordinary complication, has not at the present time yielded in all its parts to the most approved analytical and experimental methods. While a clear line of demarcation has long been drawn between that class of structures which are free from ambiguity in stress determination and those that are not, the compression member as placed in a bridge structure still possesses a resistance at least partially indeterminate, in spite of the accuracy with which it may be treated when placed in end conditions identical with those of the testing machine. It has been clearly established by numerous tests that the best forms of cross section will enable the length of a flat or pin-end strut to vary between wide limits without essentially changing the resistance of the column, but the upper chord of a pony truss still defies exact treatment, and refuses to wholly yield even to gusset plates or knee braces. It can be readily conceived that the centers of resistance of the ends of a column may within certain limits keep pace with the center deflection both in direction and rate of motion, and thus preserve the resistance essentially constant, even with a considerable increase of length; but what can be taken as the effective column length in the pony truss upper chord? It cannot be its distance between panel points symmetrical with the center, for gussets and knee braces do give some steadiness, though not complete rigidity, nor can the lateral stability yielded by the tension web members be completely disregarded.

The demands of extraordinary span

lengths have called into existence new types of truss and stiffened structures requiring novel but successful analytical methods; but the resistance of the swing bridge with variable movement of inertia of truss section and ever-changing thermal effects, remains at the present time, at best, in only an advanced conventional state, in spite of many analytical attacks. It is true that much has been done, but the design of a swing bridge is at present a task which the competent and conscientious engineer must approach with some hesitation.

But if these difficulties bear the marks of age, there are a host of others that have been reached by the rapid advance of modern bridge building. Many of these depend solely upon correct conceptions of true functions of details which originally were either not recognized at all or very incompletely; others have arisen from the adaptation of improved or new materials, while perhaps the most complicated questions relating to structural design are those involving the manner of application of the moving load.

The office of that important detail, the pin, which gives the stamp of individuality to American bridge structures and forms the basis of their superior excellence, has been clearly discovered, though original failure to do so caused the early disrepute in Great Britain of a feature of construction which, under more skillful design in this country has been the foundation of the only system permitting exact analytical determination in the truss. The proper development of the eye-bar head has long since produced a member equally strong in all its parts, and by judiciously designing and arranging, an indefinite number may be grouped on the same pin without unduly increasing the size of the latter.

The importance, and even necessity of applying increments of chord stress directly to the center of the metal intended to carry them has long been recognized and accomplished in all first-class design, except in the matters of upper and lower lateral bracing. The problem of a perfect system of lower lateral bracing for a through bridge is one that has not yet been satisfactorily solved, though a degree of no inconsiderable excellence has been attained. Fortunately, however, the chord stresses of the lateral systems are usually so small in comparison with those of the vertical loads that a very material eccentricity, even in the application of the increments, is not of great importance, and is easily provided against by stiffness of details.

Among the more frequent questions confronting the designer, in consequence of combination of the more elementary shapes in the production of proper forms of strut section, is that of the latticing uniting channels or composite shapes of plates and angles. Although this mode of strut construction has long been in use, a rational method of proportioning the latticing is yet to be determined. Conventional rules of more or less crudeness have been followed, but none of them appear to recognize the true office of this important portion of the column. It certainly does not perform the part of a compression member to the extent of the difference between the resistance of the two halves of the strut in laterally unsupported columns and that of the complete post itself. It impresses upon each half a transverse load which produces a bending in direction opposite to that caused by the longitudinal thrust, and its office is, therefore, simply to hold the two parts in fixed positions relatively to each other. The amount of the tension or compression in a direction normal to the axis of the column exerted by this latticing measures the least allowable longitudinal section (as well as rivet area) of the stay plates at the strut ends; for they must exert an amount of force equal, but opposite in kind, to that of the latticing. The amount of this force and its distribution is as yet unknown, and this part of the strut problem remains unsolved.

It is probable that there is some extravagance of material in the best of present latticing, but it is a judicious practice under circumstances of such deficient knowledge.

These are a few examples only of the many that might be cited, of the difficulties of design that confront the engineer at the present time, and while they do not admit of precise solution, it is absolutely essential that resort be made to some experimental or conventional method which, though not accurate, shall neither be extravagant on the one hand,

nor permit the margin of safety to fall below the proper limit, on the other.

The rapid improvement in processes for the production of structural materials during the past few years has led to the application of the lower grades of steel yielded by the open hearth and Bessemer processes, to the construction of bridges and roofs. The nature of this material and its behavior in certain constructive manipulations is not even yet completely understood, but its superior capacity to wrought iron in many operations of the rolling-mill, its increased ductility and perfect homogeneity, no less than its greater tensile and compression resistance, but chiefly its successful and satisfactory application to a number of existing bridges, renders the question of its ultimate general use merely a matter of a comparatively short time, and gives to all questions connected with it an unusual demand on the attention of the engineer. Although we have as yet only begun to traverse the problem connected with this material, much has already been done not only in solving some preliminary questions, but also in indicating the line along which the most productive results will probably be found.

The lower structural grades of steel, being little else than "melted wrought iron," can be welded with almost, or quite as much, facility as wrought iron, while its superior ductility and perfect homogeneity make it especially adapted to such processes as upsetting end, hence to the production of eye-bars. It is very true that these observations now apply only to those low grades of steel that exceed wrought iron in ultimate tensile resistance by at most 25 per cent., and that exercise of increased care and skill are required, but the latter circumstances are always necessary concomitants of any advance, and there is no reason to doubt that an enlarged experience will result in the employment of a metal of constantly increasing ultimate tensile resistance. Any process or operation which may obviate the final step of annealing steel eye bars will remove the last sensible (though not serious) obstacle to their general use.

The employment of operations less disturbing to the molecular arrangement, in the production of steel compression members, facilitates the use of a grade of metal giving a much higher resistance than that of which the eye bars are made. It is yet a question just how much higher steel may be used in compression than in tension members; but it is already certain that a higher grade may be used, and hence that the advantage in steel columns is correspondingly greater.

Many tests have already been made on steel angle struts, though few on composite columns; enough has been done, however, to show the superiority of steel struts over those of iron. The freedom of steel from slag and its perfect homogeneity give a greater range to the shapes in which it can be produced, and render it capable of resisting severer duties with less fatigue. The possession by steel of a greater number of better structural qualities gives certainty to the early solution of the more fundamental and important problems involved in the general introduction of that metal.

It is evident, therefore, that in some respects, at least, the approaching use of steel forces bridge building into somewhat of a transition state, in which will arise, or rather have arisen, questions affecting not only the proportions of cross-sectional areas of main truss members, but others depending on the design of details.

While, however, the preceding questions embrace difficulties of no ordinary character, they are subject to a relatively easy treatment, since the effect of a variation in attending circumstances may be determined both in quantity and quality, there are, on the other hand, numerous questions arising from the manner of application and distribution of the moving load which, on account of the varying conditions of action, are essentially indeterminate. The general question of moving loads was originally decided in a very simple and summary manner by considering it equivalent to double the amount of static load. Such a method is certainly simple enough, but does not more than very loosely represent the true action of the moving load; it would only accurately represent it if the load were applied instantaneously over the *whole* bridge without the slightest shock. But even under the most rapidly moving train this can scarcely be considered approximately true, though such an assumption would

be more reasonable in connection with the floor system.

Again the principle just given is based on the supposition that the load is not only applied instantaneously, but allowed to remain applied in one position long enough to allow the structure to take a deflection double that due to a static load just equal in amount to the moving. As a matter of fact, the moving load remains in one position an indefinitely short time, and, in addition to that fact, the members of any bridge possess an amount of inertia that would materially delay (in time) the deflection, even if all other circumstances were favorable. If the bridge is of long span so that the fixed weight (*i. e.*, of structure) becomes proportionately large, the inertia will militate with increased intensity against the conditions requisite for the support of the old hypothesis. The definite duration of time in application of load at any one point, which is the essential basis of that hypothesis, seems to have been strangely overlooked by confusing rapidity of motion with suddenness of application; conditions neither alike in direction nor similar in nature. It may, indeed, be shown that rapidity in movement, *per se*, may relieve stress rather than increase it, for the simple reason that when the load is "suddenly" applied at any one point, it is just as "suddenly" removed.

These considerations will gain force in connection with some simple computations based on the movement of rapidly passing loads. If a train moves at the rate of 60 miles per hour, it will pass over 88 feet in one second, or about 10½ inches in one one-hundredth of a second. During the same length of time a heavy body will fall a little more than one-sixty-fourth of an inch vertically. In other words, if there happens, from any cause, to be a descending slope in the track of 1 in 670 for a distance of 10½ inches, the approach thereto being level, a train moving at the rate of sixty miles an hour would produce no pressure on the track over that space. Now, if the hypothesis of a "suddenly applied load" has any application whatever to one in rapid horizontal motion, the accuracy of its application ought to increase with the rapidity of the motion. But if, in sixty miles an hour, the load moves 10½ inches in less time than a deflection of one-sixty-fourth of an inch can take place, it is readily seen how thoroughly erroneous must be the hypothesis which assumes no motion in a horizontal direction, while a deflection of one to six inches, according to length of span, may take place. The engineer, therefore, must look for another origin than that of "suddenly applied loads" for the known destructive effects of a rapidly moving load.

In all the observations on this question it has been assumed (as indeed was assumed in the old hypothesis) that the track is absolutely smooth and that no shock takes place. But let it be considered what occurs at the post of the 10½-inch incline. The load has fallen one-sixty-fourth of an inch, and if the track is again level, the moving load has become also a falling load, and shock will take place. These effects will be aggravated if the slope continues, or is steeper, or, again, if at its foot it joins an ascending slope. Even if a track is laid with the greatest skill, and maintained with the most watchful care, these slight elevations and depressions, inappreciable as they may be to the unaided eye, must exist to a greater or less extent. If to these we add the shocks and hammerings which arise at rail joints, it will be difficult neither to discover the origin of shocks and vibrations, nor to appreciate the wisdom of the ample margins of safety employed in structural design. Those shocks will not coincide in time and position for each rail, and the result will be violent movements both lateral and vertical, originating vibrations transverse and longitudinal in direction as well as vertical. Any longitudinal effects will be very materially aggravated by all train movements, save those under just enough steam to produce uniform motion.

A heavy consolidation locomotive may exert a tractive force on the rails of twenty-five to thirty thousand pounds, the greater part or all of which will be resisted by the floor system of a bridge if train motion is begun on such a structure, and a still greater force may be called into action by the air brakes of a quickly stopping train. It is erroneous to imagine, as is frequently done, that a train passing a bridge without steam exerts no tractive force. The rolling and axle friction always exist as a force opposed to the actual motion, and if the

locomotive takes no steam on a level, this force acts to drive the rails ahead, and is only neutralized by just enough steam to keep the train in uniform motion. This effect may easily be observed by measuring the movement of a marked point on a rail under a train moving by its inertia only.

All these effects are produced by the external action of the moving load, but there are others of a different character, possibly little or no less severe in their action. These result from the rapid rotation of the counterweight of a locomotive driving wheel combined with the vertical component of the thrust or pull on the main connecting rod. The first of these is most active in express locomotives, and the latter in those engines engaged in the heaviest traffic. On account of the low speed of the latter class, both effects will act most destructively under locomotives designed for high speed.

Mr. J. W. Cloud, of Altoona, Pa., has shown that the counterweight (weighing 300 lbs.) on the driving wheel of a "Class B" locomotive on the Pennsylvania R. R., running at the rate of fifty miles per hour, will exert a "blow" every fourteen seconds of 6,260 pounds above the regular moving load for a single rail, and, of course, an equal amount below it, making the total blow 12,520 pounds. Under the same locomotive the vertical component of the thrust of the main connecting rod, with an initial steam pressure of 110 pounds, and cut off at one-half, inflicts a "blow" of nearly half the magnitude of that of the counterweight, and with the same frequency. The resultant of these two effects amounts to a "blow" of about 12,500 pounds on each track for each front and rear driver, every fourteen seconds. The variation of vertical thrust in the main connecting rod involves a correspondingly periodical redistribution of the track loads, and as none of these effects coincide on the two sides of the locomotive there is generated a tendency to "roll" both transversely and longitudinally, the latter of which frequently produces very considerable motion in the engine.

At first sight it would appear that the life of a structure must be short under such fatiguing duty. It is, however, scarcely accurate to call these effects "blows"; in reality they are not "blows," but rapid variations of force taking place with the locomotive only, and never repeated at the same point.

Even at 60 miles per hour it would require eleven seconds to produce one of those effects in the engine assumed, which is certainly not identical with a "blow" consuming perhaps one-ten-thousandth part of that time; nor is it a "shock."

While, therefore, it is absolutely necessary to recognize the fatigue of the resulting vibrations, particularly in the floor system, where it is more severe than in the trusses, in the determination of the proper working stresses, it is probable that a slight relaxation in the maintenance of a high degree of track excellence would originate shocks far more destructive in character.

The qualitative analysis of any of these influences, whether of moving load, shocks, counterweight, or main rod, when once recognized, presents no particular difficulty. Unfortunately, however, the engineer needs not only the quality but the quantity of the destructive forces, and the presence of the uncertain element of time renders the exact solution of these questions impossible, and a resort to the judgment, tempered by experience, the only possible method of treatment. The *raison d'etre* of our ample margins of safety for moving loads, is then sufficiently clear, though we are unable to express it in mechanical units.

The recognition of wind pressure as an agent at times actively aiding the destruction of bridges is a late matter in structural design, but the derailed locomotive no less than the railway bridge destroyed by the unaided wind demonstrate that the recognition is most timely. These extremely violent pressures are happily of rare occurrence, and the ordinary intensities are easily provided against in all usual designs.

A matter of the greatest practical importance is the distribution of the moving load, and that importance is intensified by the constantly increasing weight of locomotives with their tenders. The old method of a certain amount of uniform load per lineal foot of track served a good purpose in its day, but the requirements of the present time make it necessary that the imaginary uniform locomotive load should give way to the ac-

tual loads applied at wheel points. This observation bears with particular force on the design of the floor system and those web members near the center of the span, especially when it is borne in mind that these are the members subject to the greatest fatigue. It is now clearly recognized that in order to find a uniform load equivalent to a given number of concentrations, so far as chord or flange stress is concerned, the greatest moment due to the concentration loads must first be found, and then a uniform load which will produce the same moment determined; this latter will be the equivalent uniform load. The table on next column shows the results of this process applied to a consolidation engine and tender weighing 171,000 pounds, with 96,000 pounds on a driving-wheel base of 14 feet 9 inches.

Above 55 feet the equivalent uniform load per lineal foot will slowly decrease until it reaches a value of about 3,200 lbs. for 100 feet and over, *i. e.*, supposing the moving load to consist of a train of such locomotives.

Span in feet.	Equiv. uniform load in lbs. per lin. foot.	Span in feet.	Equiv. uniform load in lbs. per lin. foot.
55	3750	25	4838
50	3866	20	5137
45	4004	15	5760
40	4242	12	6000
35	4336	10	5766
30	4572	5	9600

It is very clear, therefore, why the intensity of the moving load should increase as the length of span decreases, and it is equally clear that in consequence of the greater relative value of the moving load, stresses as compared with those of the fixed load, no less than the intensified effects of shocks, vibrations, etc., the fatigue of the metal in short spans will be much greater than in long ones, and consequently that the working stresses per unit of area should be correspondingly less in the former.

ON THE FRITTS SELENIUM CELLS AND BATTERIES.

By C. E. FRITTS, New York, N. Y.

From the Proceedings of the American Association for the Advancement of Science.

In all previous cells, so far as I am aware, the two portions or parts of the selenium, at which the current enters and leaves it, have been in substantially the same electrical state or condition. Furthermore, the paths of the current and of the light have been transverse to each other, so that the two forces partially neutralize each other in their action upon the selenium. Lastly, the current flows through not only the surface layer which is acted upon by the light, but also through the portion which is underneath and not affected thereby, and which therefore detracts from the actual effect of the light upon the selenium at the surface.

My form of cell is a radical departure from all previous methods of employing selenium, in all of these respects. In the first place, I form the selenium in very thin plates, and polarize them, so that the opposite faces have different electrical states or properties. This I do by melting it upon a plate of metal with which it will form a sort of chemical combination, sufficient, at least, to cause the selenium to adhere and make a good electrical connection with it. The other surface of the selenium is not so united or combined, but is left in a free state, and a conductor is subsequently applied over it by simple contact or pressure.

During the process of melting and crystallizing, the selenium is compressed between the metal plate upon which it is melted and another plate of steel or other substance with which it will not combine. Thus, by the simultaneous application and action of heat, pressure, chemical affinity and crystallization, it is formed into a sheet of granular selenium, uniformly polarized throughout, and having its two surfaces in opposite phases as regards its molecular arrangement. The non-adherent plate being removed after the cell has become cool, I then cover that surface with a *transparent conductor of electricity*, which may be a thin film of gold leaf. Platinum, silver, or

other suitable material may also be employed. The whole surface of the selenium is therefore covered with a good electrical conductor, yet is practically bare to the light, which passes through the conductor to the selenium underneath. My standard size of cell has about two by two and a-half inches of surface, with a thickness of $\frac{1}{1000}$ to $\frac{5}{1000}$ inch of selenium, but the cells can, of course, be made of any size or form. A great advantage of this arrangement consists in the fact that it enables me to apply the current and the light to the selenium in the same plane or general direction, instead of transversely to each other, as heretofore done, so that I can cause the two influences to either coincide in direction and action, or to act upon opposite faces of the selenium and oppose each other, according to the effect desired.

By virtue of the process and arrangement described, my cells have a number of remarkable properties, among which are the following:

1. *Their sensitiveness to light* is much greater than ever before known. The most sensitive cell ever produced, previous to my investigations, was one made by Dr. Werner Siemens, which was 14.8 times as conductive in sunlight as in dark. In Table A, I give results obtained from a number of my cells.

TABLE A.

SENSITIVENESS TO LIGHT.

Selenium cell.	Battery power.	Resistance in dark.	Resistance in sunlight.	Ratio.
No. 22	5 elements	39,000 ohms	340 ohms	114 to 1
" 23*	5 "	14,000 "	170 "	82.8 " 1
" 24†	5 "	648,000 "	2,400 "	270 " 1
" 25	5 "	180,000 "	930 "	196.5 " 1
" 26	5 "	135,000 "	710 "	190 " 1
" 107	5 "	118,000 "	740 "	159 " 1
" 108	5 "	200,000 "	900 "	222 " 1
" 122	5 "	56,000 "	220 "	254.5 " 1
" 129*	5 "	200,000 "	940 "	212 " 1
" 137	5 "	108,000 "	320 "	337.5 " 1

* Cells No. 23 and No. 129 are now in possession of Prof. W. Grylls Adams, of King's College, London; Dr Werner Siemens has No. 25, and Prof. George F. Barker, of Philadelphia, has No. 26.
† No. 24 was measured with a bridge multiplier of 6 to 1.

It will be observed that I have produced one cell which was 337.5 times as conductive in hazy sunlight as in dark. The tremendous change of resistance involved in the expression "337.5 times" may perhaps be more fully realized by saying that 99.704 *per cent.* of the resistance had disappeared temporarily, under the joint action of light and electricity, so that there remained *less than* $\frac{3}{10}$ *of* 1 *per cent.* of the original resistance of the selenium in dark.

In order to obtain these high results, the cells must be protected from light when not in use. The resistance is first measured while the cell is still in total darkness. It is then exposed to sunlight and again measured. It is also necessary to send the current in at the gold electrode or face, as the cell is much less sensitive to light when the light acts upon one surface of the selenium, and the current enters at the opposite surface. When the two influences, the light and the current, act through the gold in conjunction, their forces are united; and as every atom of the selenium is affected by the light, owing to the extreme thinness of the plate, we have the full effect shown in the measurements.

Cells which are sensitive to light improve by being used daily, and their sensitiveness becomes less if they are laid aside and not used for a considerable length of time, especially if allowed to become overheated. They should be kept cool, and exposed to light frequently, whether they are used or not.

Mode of measuring cells.—So great is the sensitiveness of these cells to external influences, that it is necessary to adopt some particular system in measur-

ing their resistance, and to adhere strictly to that system, as every change in the method of measurement produces a difference in the result, and the different measurements would not be comparable with each other. The reason for this will be explained presently.

The system I have adopted is the Wheatstone's bridge arrangement, with equal sides, never using multipliers, except for some experimental purpose. In each multiplier wire I have 500 ohms resistance. When the bridge is balanced, one-half of the current flows through the cell and acts upon the selenium. Between the bridge and the cell is a reversing switch so that the current can be reversed through the cell without changing its course through the bridge. A Bradley tangent galvanometer is used, employing the coil of 160 ohms resistance. The Leclanché battery is exclusively used in measurements for comparison.

2. *The kind of battery employed* has a marked effect upon the sensitiveness to light, which is largely reduced or entirely destroyed when the bichromate battery is used. The same cells again become extremely sensitive with the Leclanché battery. We might naturally expect that a change in the current employed would cause a change in the *resistance* of a cell, but it is not clear how or why it should affect the *sensitiveness of selenium to light.*

"If one kind of battery current destroys its sensitiveness, may we not suppose that another kind might increase its sensitiveness? Although the Leclanché has operated well, some other may operate still better, and by its special fitness for use on selenium cells, may intensify their actions, and so bring to light other properties yet unthought of. Is not here a promising field for experiment, in testing the various forms of battery already known, or even devising some new form especially adapted to the needs and peculiarities of selenium cells?

One year ago I made the foregoing suggestion in a paper on "A new Form of Selenium Cell," presented before this Association at Minneapolis. I am now at liberty to state that my photo-electric battery, presently to be described, marks an advance in the direction indicated. The current from this battery increases the sensitiveness of the cells to light, and also to reversal of current. One cell, whose highest ratio in light was about 83 to 1, with the Leclanché battery, when measured with my battery, gave a ratio of 120 to 1. It seems to make the resistance of the cell both higher in dark and lower in sunlight than with the Leclanché battery. But the field is yet open to others, for the discovery of a battery which may be still better for use with selenium cells.

3. *The two surfaces of the selenium act differently towards currents* sent into them from the contiguous conductors. One surface offers a higher resistance to the current than the other. The former I utilize as the anode surface, as I have found that the cell is more sensitive to light when the current enters at that surface, which is ordinarily the one covered by the gold or other transparent conductor. Some cells have this property but feebly developed; but in one instance the resistance offered to the current by the anode surface was 256 times as high as that offered by the cathode surface to the same current. In the majority of cases, however, the ratio

TABLE B.

SENSITIVENESS TO REVERSAL OF DIRECTION OF CURRENT.

No. of cell.	Battery.	Resistance. "gold anode"	Resistance, "gold cathode."	Ratio.
$\frac{3}{8}$ inch sq., No. 4	5 elements.	20,000 ohms.	1,000 ohms.	20 to 1
$\frac{3}{8}$ " " " 3	*Se.* cell.	6,500 "	400 "	16.2 " 1
Full size, No. 13	1 element.	9,000 "	800 "	11.2 " 1
" " 14	5 elements	2,440 "	130 "	18 " 1
" " 15	5 "	4,640 "	210 "	22 " 1
" " 27	5 "	6,900 "	440 "	16 " 1
" " 126	1 element.	5,000 "	330 "	15 " 1

does not exceed ten times. Table B gives some recent results.

The direction of the current is always indicated by stating the position of the gold electrode—by the terms "gold anode" and "gold cathode." The above measurements were made in dark.

4. *Sensitiveness to change of battery power.*—My cells are extremely sensitive to any change in the strength or character of the current flowing through them, which is shown by a corresponding change in the resistance of the cell. I can, therefore, vary the resistance of one of my cells in many ways, and the following may be specified:

(*a*) By changing the potential or electromotive force of the current through the cell.

(*b*) By changing the "quantity" of the battery or current.

(*c*) By putting more or less resistance in the circuit.

(*d*) By dividing the current by one or more branch circuits or shunts around the cell.

(*e*) By varying the resistance in any or all of said circuits.

A cell whose resistance becomes greater as the battery power becomes greater, and *vice versa*, I call an "L. B." cell, signifying *Like the Battery power*. A "U. B. cell" is one whose resistance becomes greater as the battery power (or strength of current) becomes less, and *vice versa*, being *Unlike the Battery power* or current strength.

These changes of resistance are not due to heating of the conductor or the selenium, and the following instance will illustrate this. I have one cell in which the selenium had about one-fourth inch square of surface melted on a brass block one inch thick. This cell measured, with 25 elements of Leclanché, 40,000 ohms. On changing the battery to 5 elements, the resistance fell instantly to 30 ohms, and there remained. On again using the current from 25 elements, the resistance instantly returned to 40,000 ohms. Had these results been due in any degree to heating, the resistance would have changed gradually as the heat became communicated to the brass—whereas no such change occurred, the resistances being absolutely steady. Moreover, even the fusion of the selenium would not produce any such change.

The "U B" property does not ordinarily change the resistance of the cell to exceed ten times, *i.e.*, the resistance with a weak current will not be over ten times as high as with a strong one. But I have developed the "L B" property to a far higher degree. Table C gives some recent results obtained with L B cells, including one whose resistance, with 25 elements Leclanché, was 11,381 times as high as with 8 elements, and which, after standing steadily at 123 ohms (and then at 325 ohms with 1 element), on receiving the current from 25 elements, again returned to its previous figure of 1,400,000 ohms.

TABLE C.

SENSITIVENESS TO CHANGE OF BATTERY POWER.

No. of cell.	Resistance with 25 elements.	Resistance with 5 elements.	Ratio of change.
$\frac{3}{8}$ inch sq., No. 1	40,000 ohms	30 ohms	1,333 to 1
$\frac{3}{8}$ " " 2	13,000 "	40 "	325 " 1
$\frac{1}{4}$ " " 1	1,400,000 "	123* "	11,381 " 1
$\frac{3}{8}$ " " 2	500,000 "	62 "	8,064 " 1
$\frac{1}{4}$ " " 5	3,500 "	21 "	167 " 1
Full size, No. 81	68,000 "	121 "	561 " 1
" " 82	9,000 "	64 "	140 " 1
" " 83	17,300 "	74 "	233 " 1
" " 119	35,600 "	19 "	1,894 " 1

* This measurement was obtained with 8 elements.

The results in the table were obtained by changing the strength of current by throwing in more or less of the battery. Like results can be obtained by varying

the current through the cell by any of the other methods before specified. The above measurements were in dark.

5. *Dual state of selenium.*—My cells, when first made, seem to have two states or conditions. In one, their resistance is very low, in the other it is high. When in the low state they are usually not very sensitive, in any respect. I therefore raise the resistance by sending an intermittent or an alternating current through the cells, and in their new condition they at once become extremely sensitive to light, currents, and other influences. In some cases they drop to the low state again, and require to be again brought up until, after a number of such treatments, they remain in the sensitive state. Occasionally a cell will persist in remaining in the insensitive state. The before-mentioned treatment raises it up for a moment, but, before the bridge can be balanced and the resistance measured, it again drops into the low or insensitive state. Some cells have been thus stimulated into the high or sensitive state repeatedly, and every means used to make them stay there, but without avail; and they have had to be laid aside as intractable.

In the earlier stages of my investigations, before the discovery of this dual state and the method of changing a cell from the insensitive to the sensitive condition, hundreds of cells were made, finished, and tested, only to be then ruthlessly destroyed and melted over, under the impression that they were worthless. Now, I consider nothing worthless, but expect sooner or later to make every cell useful for one purpose or another.

The most singular part of this phenomenon is the wide difference in the resistance of the cells in the two states. In the low state it may be a few ohms, or even a few hundredths of an ohm. In the high state, it is the normal working resistance of the cell, usually between 5,000 and 200,000 ohms, but is often up among the millions. The spectacle of a little selenium being stimulated, by a few interruptions of the current through it, into changing its resistance from a fraction of an ohm up to a million or several millions of ohms, and repeatedly and instantly changing back and forth, up and down, through such a wide range—we might almost say, changing from zero to infinity, and the reverse, instantly—is one which suggests some very far-reaching inquiries to the electrician and the physicist. What is the nature of electrical conductivity or resistance, and how is it so greatly and so suddenly changed?

6. *Radio-electric current generators.*—My cells can be so treated that they will generate a current by simple exposure to light or heat. The light, for instance, passes through the gold and acts upon its junction with the selenium, developing an electromotive force which results in a current proceeding from the metal back, through the external circuit, to the gold in front, thus forming a photo-electric dry pile or battery. It should preferably be protected from overheating by an alum-water cell or other well-known means.

The current thus produced is radiant energy converted into electrical energy directly and without chemical action, and flowing in the same direction as the original radiant energy, which thus continues its course, but through a new conducting medium suited to its present form. This current is continuous, constant, and of considerable electromotive force. A number of cells can be arranged in multiple arc or in series, like any other battery. The current appears instantly when the light is thrown upon the cell and ceases instantly when the light is shut off. If the light is varied properly, by any suitable means, a telephonic or other corresponding current is produced, which can be utilized by any suitable apparatus, thus requiring no battery but the selenium cell itself. The strength of the current varies with the amount of light on the cell, and with the extent of the surface which is lighted.

I produce current not only by exposure to sunlight, but also to dim diffused daylight, and even to lamplight. I have used this current for actual working purposes—among others, for measuring the resistance of other selenium cells, with the usual Wheatstone's bridge arrangement, and for telephonic and similar purposes. Its use for photometric purposes and in current-regulators, will be mentioned farther on. It is undoubtedly available for all uses for which other battery currents are em-

ployed, and I regard it as the most constant, convenient, lasting, readily used and easily managed pile or battery of which I have any knowledge. On the commercial scale, it could be produced very cheaply, and its use is attended by no expense, inasmuch as no liquids or chemicals are used, the whole cell being of solid metal with a glass in front, for protection against moisture and dust. It can be transported or carried around as easily and safely as an electro-magnet, and as easily connected in a circuit for use wherever required. The current, if not wanted immediately, can either be "stored" where produced, in storage batteries of improved construction devised by me, or transmitted over suitable conductors to a distance, and there used, or stored as usual till required.

7. *Singing and Speaking Cells.*—When a current of electricity flowing through one of my selenium cells is rapidly interrupted, a sound is given out by the cell, and that sound is the tone having the same number of air vibrations per second as the number of interruptions in the current. The strength of the sound appears to be independent of the direction of the current through the cell. It is produced on the face of the cell, no sound being audible from the back of the cell. An alternating current also produces a sound corresponding to the number of changes of direction. Experiments also show that, if a telephonically undulating current is passed through the cell, it will give out the speech or other sound corresponding to the undulations of the current—and, furthermore, that cell will sing or speak in like manner, without the use of a current, if a suitably varied light is thrown upon it while in closed circuit.

My experiments having been devoted especially to those branches of the subject which promised to be more immediately practically valuable, I have not pursued this inquiry very far, and offer it for your consideration as being not only interesting, but possibly worthy of full investigation.

GENERAL OBSERVATIONS ON THE PROPERTIES OF CELLS.

From the number of different properties possessed by my cells, it might be anticipated that the different combinations of those properties would result in cells having every variety of action. This is found to be the case. As a general rule, the cells are noteworthy in one respect only. Thus, if a cell is extremely sensitive to light, it may not be be specially remarkable in other respects. As a matter of fact, however, the cells most sensitive to light are also "U B cells."

The property of sensitiveness to light is independent of the power to generate current by exposure to light—the best current-generating cells being only very moderately sensitive to light, and some of the most sensitive cells generate scarcely any current at all. Current-generating cells are, almost without exception, "U B cells;" and the best current-generating cells are strongly polarized, showing a considerable change of resistance by reversing the direction of a current through them; and they are also strong "anode cells," *i. e.*, the surface next to the gold offers a higher resistance to a battery current than the other surface of the selenium does. The power to generate a current is temporarily weakened by sending a battery current through the cell while exposed to light, in either direction. The current generated by exposure to light is also weakened by warming the cell, unless the cell is arranged for producing current by exposure to heat.

The properties of sensitiveness to light and to change of battery power are independent of each other, as I have cells which are sensitive to change of current but absolutely insensitive to light—their resistance remaining exactly the same whether the cells are in darkness or in sunlight. I also have cells which are sensitive to light, but are unaffected by change of battery power, or by reversing the direction of the current through them.

The sensitiveness to change of battery power is also independent of the sensitiveness to reversal of direction of the current. Among the best "L B cells," some are "anode cells" and others are "cathode cells," while still others are absolutely insensitive to reversal of current, or to the action of light.

Constancy of the Resistance.—A noticeable point in my cells is the remarkable constancy of the resistance in sun-

light. Allowing for differences in the temperature, the currents, and the light, at different times, the resistance of a cell in sunlight will remain practically constant during months of use and experiments, although during that time the treatments received may have varied the resistance in dark hundreds of thousands of ohms—sometimes carrying it up, and at others carrying it down again, perhaps scores of times, until it is "matured" or reaches the condition in which its resistance becomes constant.

As has already been stated, the sensitiveness of a cell to light is increased by proper usage. This increased sensitiveness is shown, not by a lowered resistance in light, but by an increased resistance in dark. This change in the cell goes on, more or less rapidly, according as it is retarded or favored by the treatment it receives, until a maximum is reached, after which the resistance remains practically constant in both light and dark, and the cell is then "matured" or finished. The resistance in dark may now be 50 or even 100 times as high as when the cell was first made, yet, whenever exposed to sunlight it promptly shows the same resistance that it did in the beginning. The various treatments, and even accidents, through which it has passed in the meantime, seem not to have stirred its molecular arrangement under the action of light, but to have expended their forces in modifying the positions which the molecules must normally assume in darkness.

Practical Applications.—There are many peculiarities of action occasionally found, and the causes of such actions are not always discernible. In practice, I have been accustomed to find the peculiarities and weaknesses of each cell by trial, developing its strongest properties and avoiding its weaknesses, until, when the cell is finished, it has a definite and known character, and is fitted for certain uses and a certain line of treatment, which should not be departed from, as it will be at the risk of temporarily disabling it. In consequence of the time and labor expended in making cells, in the small way, testing, repairing damages done during experiments, etc., the cost of the cells now is unavoidably rather high. But if made in a commercial way, all this would be reduced to a system, and the cost would be small. I may say here that I do not make cells for sale.

The applications or uses for these cells are almost innumerable, embracing every branch of electrical science, especially telegraphy, telephony and electric lighting, but I refrain from naming them. I may be permitted, however, to lay before you two applications, because they are of such general scientific interest. The first is my

Photometer.—The light to be measured is caused to shine upon a photo-electric current-generating cell, and the current thus produced flows through a galvanometric coil in circuit, whose index indicates upon its scale the intensity of the light. The scale may be calibrated by means of standard candles, and the deflections of the index will then give absolute readings showing the candle-power of the light being tested. Or, the current produced by that light and that produced by the standard candle may be compared, according to any of the known ways of arranging and comparing different lights—the cell being lastly exposed alternately to the two lights, to see if the index gives exactly the same deflection with each light.

This arrangement leaves untouched the old difficulty in photometry, that arising from the different *colors* of different lights. I propose to obviate that difficulty in the following manner. As is well known, gold transmits the green rays, silver the blue rays, and so on; therefore, a cell faced with gold will be acted upon by the green rays, one faced with silver by the blue rays, etc. Now if we construct three cells (or any other number) so faced that the three, collectively, will be acted upon by all the colors, and arrange them around the light to be tested, at equal distances therefrom, each cell will produce a current corresponding to the colored rays suited to it, and all together will produce a current corresponding to all the rays omitted by the light, no matter what the proportions of the different colors may be. The three currents may act upon the same index, but each should have its own coil, not only for the sake of being able to join or to isolate their influences upon the index, but also to avoid the resistances of the other cells. If a solid transparent conductor of electricity could be found

which could be thick enough for practical use and yet would transmit all the rays perfectly, *i. e.*, transmit white light unchanged, that would be still better. I have not yet found a satisfactory conductor of that kind, but I think the plan stated will answer the same purpose. This portion of my system I have not practically tested, but it appears to me to give good promise of removing the color stumbling-block which has so long defied all efforts to remove it, and I therefore offer it for your consideration.

Photo-Electric Regulator.—My regulator consists of a current-generating cell arranged in front of a light, say, an electric lamp whose light represents the varying strength of the current which supports it. The current produced in the cell by this light flows through an electro-magnetic apparatus by means of which mechanical movement is produced, and this motion is utilized for changing resistances, actuating a valve, rotating brushes, moving switches, levers, or other devices. This has been constructed on a small scale and operates well, and I think it is destined to be largely used, as a most sensitive, simple and perfect regulator for currents, lights, dynamos, motors, etc., etc., whether large or small.

In Conclusion, I would say that the investigation of the physical properties of selenium still offers a rare opportunity for making very important discoveries. But candor compels me to add that whoever undertakes the work will find it neither an easy nor a short one. My own experience would enable me to describe to you scores of curious experiments and still more curious and suggestive results, but lack of time prevents my giving more than this very incomplete outline of my discoveries.

HYDRAULIC PROPULSION.

By SYDNEY WALKER BARNABY, Assoc. M. Inst. C. E.

Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Sir J. W. Bazalgette, C. B., President, said, in proposing a vote of thanks to Mr. Barnaby for his interesting paper, that it was just thirty years since the subject was discussed at the Institution. The record of that discussion was very meager, and perhaps it was well that it was so, for report stated that the discussion was exceedingly animated, and somewhat personal, and that the language used on that occasion was not altogether parliamentary. He was sure that on the present occasion the discussion would be animated; he was equally sure that it would be conducted without any personal feeling, in a perfectly "parliamentary" manner; and he could also promise that a true and complete record of the discussion would be given.

Mr. Sydney W. Barnaby asked permission to call attention to a diagram which had not been alluded to in the paper, but was illustrated by a model on the table. It represented a small steam launch driven by what he had called a screw-turbine propeller. It had been already described at the Institution and elsewhere, and he only alluded to it because it was really the outcome of the hydraulic-boat. It had been pointed out in the paper that there were four features in the Ruthven form of pump which militated against its success as a propeller. In the first place, there was the difficulty of getting the water through the bottom of the boat and into the pump without checking the velocity it already had relative to the vessel. In the second place, there was the disadvantage of the extra weight of machinery involved by the necessity of carrying all the water acted upon. Thirdly and fourthly, there was the loss by friction and by bends in the passages. In working out the hydraulic-boat, it occurred to Mr. Thornycroft that if the turbine could be put outside the boat and under the bottom, the first two causes of loss would be avoided. It was also clear that if the water

could be made to flow axially through the turbine instead of centrifugally, as in the Ruthven pump, no pipes would be needed, and there would be no loss by friction or by bends. This was what had really been done in the boat, illustrated by the diagram and model, to which he had referred. It might be said that it was not a turbine, but it would certainly be an abuse of language to call it a screw. He thought it might be fairly described as a screw-turbine propeller. Another point in its favor was that at least as light a draught (and in some cases a lighter) could be obtained as with an actual centrifugal pump inside the boat. In the second class torpedo-boat, if a screw-turbine propeller had been used, its diameter would have required to have been 21 inches only, and the draught could have been reduced from 2 feet 6 inches, which it was when fitted with the turbine, to 2 feet 2 inches. Therefore, the principal point which Lord Dufferin's Committee had mentioned as a reason for a further trial of the turbine propeller had been secured by a simpler method avoiding a great many of its disadvantages. The steering power also was very superior with the turbine underneath the bottom and near the end to what it was with the nozzles in the ordinary hydraulic-boat, the reason being that almost the full power of the engines was used in both cases; but in the steam-launch (Plate 1) it acted upon a leverage of half the length of the boat instead of a leverage of half the breadth, as in the case of the hydraulic-boat. He thought it might therefore be taken that that was at any rate one solution of the problem of hydraulic propulsion.

Mr. M. W. Ruthven said that the author had taken some trouble to show that great losses were sustained by the hydraulic mode of propulsion; the question, however, was not how much loss was due to the hydraulic method, but was the loss less or greater than by the screw? The safest way to get at that was by a comparison of the results. It

VIPER CLASS.

9th May, 1866........	274.8
18th " "	297.2
31st " "	328.0
5th Nov., "	317.7
4th June, "	321.0
5th " "	335.9
2d Aug., 1867........	379.9
5th " "	437.6
11th Oct., "	424.8
	9)3116.9
	346.3
	3.2

WATERWITCH.

Coefficient from.....	$\frac{v^3 \times M S}{I.HP.}$
1st Jan., 1867........	342.7
17th " "	327.7
9th Aug., "	351.9
28th " "	328 7
3d Sept., "	303.3
12th Oct., "	357.8
	6)2012.1
	335.3
	12.2

PHILOMEL CLASS.

14th March, 1868.......	302.0
8th May, "	316.0
20th March, "	299.0
16th April, "	274.0
14th July, "	329.0
20th March, "	279.0
20th May, "	311.0
19th Nov., "	294.0
12th May, "	311.0
27th Oct., "	272 0
	10)2987.0
	298.7

was for this purpose that the trials of the Waterwitch, Vixen and Viper, had been undertaken, though a comparison might also be made with the screw in other ships. The author had chosen to compare the Waterwitch only with the Viper, and with a particular trial; but as the results of the trials of the Viper, and Vixen varied nearly 60 per cent., the only approach to accuracy would be the mean of all the trials of both ships, compared with the mean of all the trials of the Waterwitch. When that was done it would be found that the efficiency of the hydraulic propeller was within 3 per cent. of the screw propeller in the Viper and Vixen; but as the results of the trials of those vessels varied so much they did not form a satisfactory comparison. There was, however, a class of vessels called the Philomel class, with similar engine and boiler-power to that of the Viper and Vixen, whose performance varied less. Now comparing the coefficients of this superior class of ships with the coefficients of the Waterwitch, it would be found that the hydraulic-propeller in the Waterwitch was more than 12 per cent. better than the screw-propeller in the Philomel class. In Table 1 the coefficient of the hydraulic-propeller in the Waterwitch was put as 116.9,

while that of the Swedish boat was 52.5, showing that the former was 122 per cent. better. The Thornycroft was stated to be 72, giving the hydraulic-propeller in the Waterwitch a superiority of 62 per cent. In Table II., however, this seemed reversed, for the total efficiency of the hydraulic-propeller in the Waterwitch was put as 0.180, in the Swedish at 0.214, and in the Thornycroft boat at 0.250. Now the use of the column of coefficients was to afford a comparison of efficiency, and this being so, that column in Table 2, showing a total efficiency, must keep the same relations as in Table 1. Mr. Froude, he believed, found that a good screw had a total efficiency of 0.40. Taking this to be correct, and seeing that the hydraulic-propeller in the Waterwitch was superior to the screw by 12 per cent., its total efficiency was thus proved to be 0.44; and unless the author could show that the total efficiency of the screw-propeller was less than 0.18, he must be wrong in putting the total efficiency of the hydraulic-propeller in the Waterwitch at that figure.

Captain Heathorn observed that five years ago Mr. Ruthven had given him copies of the tables of the various trials to which he had referred, and he had himself given copies to many members of the Institution. He only hoped that every possible experiment would be tried to see if greater velocities could not be obtained. Mr. Thornycroft had invented something like a Giffard's injector, and had obtained a good result. The force was a flow of water instead of steam; surface friction was at work and produced the required result. About eleven months before his recognition of surface friction as a means of propulsive effort, Captain Heathorn had tried experiments with many kinds of orifices, and found that the force of a stream of water, from a pipe under water, in the still water surrounding was, when once the flow began, not so much dependent on its sectional area, as on the surface contacts; and that if the area of contact was larger in proportion to the area of the section of the flow from a round pipe in one case and one of another form in the other, greater surfaces set up greater friction, greater speed resulted and less water was used. The Admiralty had put him in communication with the late Mr. Froude, who, in answer to a letter of the 20th of March, 1878, said that the proposition which he had laid down as the basis of his view was one with which Mr. Froude was familiar, and he went on to ask in what way Captain Heathorn proposed to apply it to the propulsion of ships. His answer was: Contract the orifices or jets of H. M. S. Waterwitch to a certain form, and do the same to the exit ends of the screw channels of H. M. S. Bruiser. Screw channels, he believed, were invented by the late Mr. Griffith. Now that the Admiralty had a boat he hoped that they would persevere, and find out what hydraulic power really was.

Admiral Selwyn considered the paper an important contribution to the history of the subject. If any means could be adopted, even at the sacrifice of speed, to prevent ships and their crews going suddenly to the bottom by the impact of a torpedo or a ram, he was sure Englishmen would advocate spending a little more money for the extra power required to give the necessary velocity, and at the same time secure the other advantages. He doubted the accuracy of the statement in the paper that the principles underlying the subject were perfectly understood; he thought that, otherwise, none of the experiments would have been made on the basis on which they had been carried out in the Waterwitch and the other similar vessels. The hydraulic arrangement seemed to present advantages which were apart from its use as a propeller; but wherever it was necessary to put a scoop into the bottom of a ship it would be found that there is a strong objection to it, because the scoop would be the first thing to encounter the ground on getting into shallow water. It would not, therefore, be liked for that purpose, however applicable it might be to a torpedo boat. He had lost all faith in coefficients. He had lately seen a statement that the large and fast steamer, the Phæton, sister ship to the Iris, had had about 600,000 lbs. weight of engine taken out, the engine-power being reduced from 7,000 H. P. to 5,000, and had been driven faster (more than 18 knots, with the same displacement and the same shape), and he confessed that coefficients would require a little more explanation before he could recognize their applicability to the sub-

ject. Engineers were apt to make a formula to fit certain cases, and to lay it down that those cases were the rule, and that all abnormal cases, as they were called, were the exception. It was strange that with the addition of 600,000 lbs. to the armor-plated deck, one vessel with 5,000 H. P. went 18.3 knots an hour, while the other vessel with 7,000 H. P. went 18 knots. An experiment mentioned to him some years ago by Mr. Gwynne, of Hammersmith, gave the following results: Diameter of disk, 30½ inches; orifice, 8 inches; revolutions, 800 per minute; indicated H. P., 100; head attained 90 feet, with a discharge of 800 gallons per minute; thus, there was a pressure of 45 lbs. per square inch. The effective thrust was 2 tons. Of course, to the 45 lbs. per square inch would have to be added the 800 gallons per minute discharged at the top of the 90 feet. There was an important anomaly in all the records of the Waterwitch. Engineers would readily admit that there was no departing from the law of action and reaction being equal and contrary, and when any one told him that a pump, whose peripheral velocity was 29.3 feet per second, gave a velocity of discharge of 29 feet per second, he thought some other explanation should be sought than that which was found in the pump itself. Unless the velocity of the feed was there utilized, he did not see how such an effect could be produced. If it were produced, the pump would be undoubtedly an absolutely efficient machine. It might be expected that as the disk-pumps were enlarged there would be an increase of efficiency. All experience pointed to that. While in the smaller pumps the velocity of the issuing water was not more than about 50 per cent. of the peripheral velocity, in the larger pumps 60, and even 70, per cent. had been obtained. Distrusting formulas, he took the percentage by calculating the revolutions into the circumference, finding the peripheral velocity of the disk from that, and getting the discharge as a percentage of that peripheral velocity. When 37.2 feet per second had been obtained as the velocity of discharge, and the velocity of feed was 20 feet per second, he presumed that only 17.2 feet per second had been communicated by the engine to the water. If that was the case, might not the anomaly in the Waterwitch be attributed to the velocity of feed from those inlets? A set of mouths, immediately under the pump, scooped downwards, as was the case in the later trials of the Waterwitch would give a certain velocity of feed into the pump, and that might be added to the peripheral velocity, so explaining what otherwise seemed to be utterly inexplicable? He had found that various sources were accessible for an account of the revolutions of the Waterwitch engines, and while he had found them sometimes stated at forty-two, they occasionally descended to forty per minute. Engineers, he thought, would be thoroughly aware of the vice of taking revolutions either through another observer or at a different time from that at which the indicator diagrams were accorded. Any possible H. P. might be indicated in such a way, and give rise to a very false basis of coefficients. It appeared that in the Thornycroft propeller a velocity of 56 feet per second had been obtained—he presumed a peripheral velocity—and the velocity of discharge was 37.25 feet. That velocity was much more important than quantity of discharge. The volume did not matter in the least in regard to the velocity obtained. The whole power of the engines might be thrown away in lifting a mass of water, but if the necessary velocity to produce the speed sought was not communicated to the water, most engineers would say it was vain to seek it at all. It would be interesting to try an experiment first by towing the boat with her inlet so arranged and her scoop so arranged at the velocity which she had obtained with the engines at rest. It would then be seen how much the velocity of the inlet-water was diminished by passing through the pump at rest. Next, to tow the boat with such revolutions of her engines as would give a peripheral velocity equal to the speed of the boat. And, lastly, at the calculated velocity of feed. Those three experiments would bring out exactly the efficiency produced by the feed as distinguished from that produced by the pump. Now, supposing that the revolutions of the screw-engines had not been changed, but that the diameter of the nozzles had been diminished from 9 inches to 6 inches, and the disk increased to 2 feet 9 inches, what might be

expected? He thought it would be found, taking the basis obtained by the author's experiments of 56 feet peripheral velocity, and 37 feet velocity of discharge, that 60 ft. per second velocity of discharge should be obtained with 91 ft. per second velocity of periphery. That would correspond, provided the water thrown was sufficient to bring out a proportionate pressure. He was not aware whether the pressure per square foot of midship section to drive the boat 17.3 knots per hour had been calculated, but, whatever it was, it could no doubt be made up. The velocity of 91 ft. per second peripheral, and 60 ft. discharge, would give about 17.7 knots per hour. He was sure that would be impracticable with a less speed of discharge water. If the number of revolutions per minute were diminished, the speed of the boat could be arranged without any possibility of change. Every experiment he had made brought out the fact that the speed of revolutions was indissolubly connected with the speed of the boat. If, therefore, the principles underlying the subject had been thoroughly understood, the first question asked would have been, "What speed is it wished to get from the boat?" That would govern the revolutions, or the revolutions multiplied into the periphery of the disk. The disk might be made a little larger and then the revolutions might be diminished, or the revolutions might be increased and the disk diminished. But, as Mr. Gwynne's experiments showed eight hundred revolutions producing a thoroughly good effect, with one 8-inch issue, there need be no fear. He had himself driven a small 1-foot pump at one thousand revolutions, and on the basis to which he had referred he had obtained exactly the calculated speed of the boat. The pressure per square foot midship section was, as nearly as he could make out, at the velocity and size of nozzle stated, 120 lbs. He presumed that that would be scarcely sufficient for a speed of 17.7 knots per hour, but it could be easily increased. In regard to the possibility of pumping the ship out with the full power of the engines, if any issue was opened from a drowned compartment (where the water must be on a higher level) to the turbine, he failed to see why the turbine should choose between the bilge-water and the other water. He thought it would certainly take a larger quantity from the easier path of the inlet, but that it would take a portion from the bilge that would probably be sufficient to keep the boat from sinking, or result in so far prolonging its floating above water as to save human life. That result would be sufficient to justify any trials. It could scarcely be supposed that in the event of another war there would not be a lesson to learn as to the effects of rams and torpedoes. Mr. Thornycroft had done so much and so well in providing the means of destroying the British and other navies that it was only fair to ask him to throw a little of his energy, skill, and genius into the question of how to save human life and the expenditure of ships.

Sir Edward J. Reed, K. C. B., M. P., thought the paper was a very important contribution to the knowledge of the subject of hydraulic propulsion. The last bearing on the subject was the comparison, referred to by Mr. Ruthven and other speakers, between the Viper and the Waterwitch. Admiral Selwyn had expressed his distrust of coefficients and of formulas, and Sir Edward Reed supposed that all engineers distrusted them when they only took account of a fractional part of the circumstances they wished to consider. There were very peculiar circumstances in the case of the Viper and Waterwitch trials. In the first place he might say, as the designer of the boat, that the Viper was about the very worst screw-propelled vessel that had ever been built, and that for a very good and sufficient reason. She had to be designed to pass through certain locks; her length and her breadth, therefore, were arbitrarily determined, and her depth, also, had been similarly determined. Having thus a parallelopipedon which was to embrace the Viper, there had to be carried by it certain armor and guns, and particularly as much as possible of the armor. The speed was of very minor importance, and it had been deliberately subordinated to the armor-carrying and gun-carrying power. It would therefore be seen that the Viper and Vixen were rectangular boxes, slightly improved, with small regard to propulsive advantages, and he thought they were about as bad vessels as could

have been designed. Admiral Sir George Elliot, impressed, like Admiral Selwyn and many others, with the great advantages of hydraulic propulsion to Her Majesty's ships at sea, put a strong pressure upon the Admiralty to get the hydraulic propeller tried, and holding at the time a high official position, and being an officer of great experience at sea, and of much deserved influence, he succeeded in inducing the authorities to try a hydraulic propeller in a vessel of the class referred to, or a vessel of about the same dimensions. There were reasons, however, tending to show (the figures given by the author did not fully demonstrate it, but he had no doubt Mr. Ruthven would readily concur) that the form of the Waterwitch was superior to that of the Viper and the Vixen, because, when she was designed, some limitations imposed upon the other vessels had been removed. A considerable improvement in form was realized, and the trials were made, and, as far as his memory went, the figures given by the author fairly represented the state of the case.

Taking Mr. Ruthven's figures, it would be seen that the bad screw-propelled vessel was a little better than the hydraulic-propelled vessel, even at the low speed of 9½ knots per hour. The trial certainly made a good show for the hydraulic propeller by comparison with a bad screw-vessel—a better result than had been expected by many persons; but what would happen when higher speeds were attempted? The author's paper contained a case in point. The Government had sought to give effect to the recommendation of some of the members of Lord Dufferin's committee, or of the committee itself. They had gone to a gentleman who had distinguished himself in the design and production of vessels of the torpedo class, Mr. Thornycroft, to whose genius the introduction of these light and extremely fast vessels was due, and they had asked him to do the best he could in establishing a comparison between the hydraulic and the screw propeller designed for high speed. The result was, that with a given H. P. in two vessels, made as nearly alike as the circumstances would admit, the hydraulic vessel steamed at a little over 12½ knots per hour, and the screw-propelled vessel at a little over 17¼ knots. Admiral Selwyn had referred to the statement in the paper that the jet as a propeller might be taken as a little better than a screw, but he had forgotten to complete the sentence: "But the loss in the pump is a dead loss, and represents about half of the power." He did not know what the experience of others might be, but he could say for himself that, if he had to devise a fast or any other steam vessel, to lose half the power in order to secure an efficient propeller, would be a very sad imposition to have placed upon him. He was unable, as he always had been, to anticipate any satisfactory result from the hydraulic system of propulsion as applied by means of internal pumps in the ship. He sympathized with the objects that Admiral Selwyn and other naval officers had in view, and he should be delighted to see growing out of the discussion some relief from the loss which the system always involved when so applied. He confessed that his sympathies went very strongly in favor of the apparatus described by the author after the reading of his paper, viz., the application outside the vessel of an arrangement by which the good effects of hydraulic propulsion might be obtained in a large degree without its serious disadvantages. With regard to Mr. Ruthven's reference to the mode of conducting the trials, he thought he ought in fairness to say that everything had been done at the time that could, in reason, be done to satisfy Admiral Elliot, who was known to be more or less in communication with Mr. Ruthven. He had also to state that he believed that the consideration of the subject had been brought to an end because the discussions between Admiral Elliot and the Admiralty had passed away from the efficiency of the jet system to questions about the indications of power in the engines of the Waterwitch, and other collateral matters, so that the essential part of the subject had in some way fallen out of discussion. He was sure it would be the desire of the council and of the members that every possible opportunity should be given to gentlemen capable of discussing the subject, to direct the minds of the members to some methods, if methods there were, by which to get rid of the enormous loss sustained with-

in a ship by the application of the hydraulic system for its propulsion.

Admiral Sir George Elliot, K. C. B., said he would not venture to enter the domain of science; all that he knew of hydraulics he had learned from personal observation of the practical results which had been hitherto obtained. He was greatly pleased when he heard that the subject was to be discussed by the Institution, on account of the great advantages to navigation which would accrue even from a partial application of hydraulic propulsion to ships. He did not think that hydraulic propulsion had ever yet had fair play; it had never yet been tried on its merits. Certain conditions had been attached to the experiments which he thought had marred the best efforts of engineers. The Nautilus had not been designed with the sole view of developing hydraulic motive power in a ship; the owner had a commercial object in view. He looked to a commercial enterprise, inasmuch as the vessel was designed to run up the Thames to Richmond; consequently, with a light draught, and having to carry a large number of passengers, she had full lines; still the speed realized by that vessel was 9½ knots per hour. He thought the author had made a mistake when he said 8.32 knots. She was raced on the river, against a vessel called the Volunteer, down to Gravesend, and she distanced her. The Volunteer was a vessel of 24 H. P., whilst the Nautilus was 20 H. P. She afterwards raced constantly with the Citizen boats, and to his knowlege she beat them. Then a challenge was held out to any paddle-boat on the river, but it was not accepted, although an offer was made to pay the expenses. No one could say that the Waterwitch was a good specimen of naval architecture in which to try an experiment. The constructors had a very difficult task in making armor-clad gunboats, and the designer looked upon them as very bad specimens of naval architecture. However, the Waterwitch, in her first trial at the Maplin Sands realized a speed of 10 knots per hour.

At that time in a contest between the Viper and the Vixen, her two sister vessels, the latter was the slower of the two, therefore the trials afterwards took place between the Viper and the Waterwitch; the former vessel at the Maplin Sands obtained the same speed as the Waterwitch. They were afterwards taken to Portsmouth where the trials went on. The builder's trim of the Viper was 1 foot at the stern, at which she was run at the Maplin Sands; but at Portsmouth she was brought down to 2 feet at the stern, and the bow lines were thus lengthened, whilst the Waterwitch was compelled to remain on an even keel. The trials gave the advantage to the Viper of ½ of a knot speed. In the Waterwitch the water came in to the turbine from forward through a canal, with holes perforated in the bottom. The turbine was raised unnecessarily high, and the discharge pipes were raised still higher, in order to test the differences between discharging the water above water when she was at her light trim, and in the water when she came down to her load draught. He considered both these features to have been adverse to the success of the hydraulic method of propulsion. Those vessels went to sea, but they were found to roll so heavily as to be unseaworthy, and they were afterwards laid up and lightened and taken out to Bermuda; the Waterwitch was laid up in ordinary. Some years afterwards, when he was appointed Dockyard Superintendent at Portsmouth, the Admiralty allowed him to have the Waterwitch to make some experiments. He had an idea that leading the water in through the bottom of the vessel in a canal, by which a large quantity of water was carried about, necessitating increased displacement, was a mistake, and he was determined to ascertain how far it would be advantageous to bring the water in from forward or from directly underneath the turbine. The trials were carried out very carefully by the dockyard officers. He merely asked them to close the canal and to open underneath the turbine a hole and to insert a tube, and let the water enter directly underneath the turbine. The vessel was taken into dock and cleaned, put to a certain trim, run at the mile, and her speed ascertained. She was then docked. The alteration was made; she was again cleaned, put to the same trim, taken to the mile, and the result was ½ knot additional speed per hour. He then thought that the turbine to be at its best should be brought as low in the vessel as possible, the water

admitted from underneath, and the discharge-pipes horizontal. In the Waterwitch they were raised, as he had said, which was certainly an objection. He still believed that, in order to try the hydraulic-propeller at its best and give it fair play, a vessel should be designed for the purpose, a double-ended vessel with a rudder at both ends, on an even keel, and the hydraulic-propeller as low as possible, in point of fact to work in the water. He had reasons for believing that bringing the water up to the turbine from directly underneath was an advantage. His idea was that directly the turbine was turned, a hole could not be made in the water, and a column of water at a certain speed was immediately set in motion, which was greater than the speed at which the ship was going through the water; consequently the greater velocity overcame the lesser velocity due to the momentum of the vessel; in fact there was no loss of power. If the turbine was very low down the bottom of the turbine might almost be brought to the sea itself. Having a rudder at both ends the vessel would go equally fast both ways; in fact, in the trials in the Waterwitch, although the water was brought in forward it did not make much difference in her speed one way or the other. That told very much against the attempts that had been made to draw the water in from forward by pipes from the bows. He could understand the object in view; and theoretically he could see that it appeared to be a wise one; but as far as that trial in the Waterwitch went, it proved not to be so. Then he made an experiment to ascertain what the size of the discharge pipes should be. The Admiralty were again good enough to carry out his experiment, by putting a flap on a hinge inside the nozzle, which could be pushed out, so as to close the aperture or leave it wide open. The speed of the water at the discharge was measured, and it was found when the nozzle was half closed the water was going out at double the speed; therefore the same quantity of water was discharged as when it was wide open—the speed of the vessel in both instances being the same. That showed that the nozzles of the Waterwitch were unnecessarily large, because he believed that the best result was obtained when the pipes were completely filled, but trials were made at different revolutions of the turbine, and it did not appear to signify much. He did not agree that the turbine would not work unless it was full. The author had described all the advantages (and they were very great) of the turbine as a propeller for ocean purposes, but he had not specially named one benefit which he thought the most important. If a vessel could be stopped, without any reference to the engine-room, in her own length, and if two vessels meeting could do the same, he believed there would be very few collisions. Another feature was the advantage that it afforded in foul weather. The accidents in screw-boats that then occurred were generally caused from the racing of the screw; but the greatest amount of motion a ship could be put into would not in the slightest degree bring a greater strain upon the engines of the turbine. After a considerable time, the subject was again mooted at the Admiralty, and then came the trial of the Thornycroft torpedo-boat, respecting which he was greatly pleased to think that the experimental trial had been placed in the hands of that firm, who, he had no doubt, had taken pains to make it a success. But as a competitive trial it was inadequate. The author had stated that the object of the Admiralty was to have vessels of very light draught of water in waters so shallow as scarcely to afford sufficient immersion even for twin screws. In a fair trial both vessels should have been put to the same test, which was the draught of water. But in the case of the turbine the draught of water was 2 feet 6 inches, while that of her opponent was 3 feet 8¼ inches; and in the screw-boat the shaft was on the keel, and nearly half the blades revolved below the keel, giving a large diameter to the screw, added to the extra draught of water. In this competitive trial the screw torpedo-boat would require a depth of 6 feet of water to work the screw, whilst the turbine boat would only need a depth of 2 feet 6 inches to move about in. That was unfair to the turbine which had not those advantages. The two trial vessels should have had the same draught of water, similar displacement, and similar power.

If the draught had been 2 feet 6

inches, and the screw had been limited to that diameter, he thought that a very different result would have been obtained. He should be much pleased if Messrs. Thornycroft and Co. would build a boat exactly as he had described, with her turbine low down, with the entrance of water from below, with the discharge-pipes horizontal ejecting the water under water, and with the valve like that in the Waterwitch, and then see what speed they could get out of the screw with a diameter of 2 feet 6 inches, with the same power. Then again one boat was brought by the stern, and the other was on an even keel; but both boats ought to be confined to a given draught of water. He would give the turbine boat rather a full body, so as to lower the turbine and get as much benefit from the full diameter as possible. In the vessel in question, the diameter of the turbine was small, and the discharge-pipes were long. It appeared to him that the importance of centrifugal force had been lost sight of, and this he had always understood to be an advantage in the turbine, by not having a large diameter so as to discharge the water at a rapid rate through the pipe, and reduce the revolutions of the turbine. Another advantage of admitting the water from below was that it was not necessary to carry the same volume of water in the canal as had to be carried in the Waterwitch; it was only necessary to carry the water which the turbine itself contained. When he spoke of the partial success of the turbine, he referred to its use in sea-going vessels as an auxiliary power. With power sufficient to give a speed of 8 knots per hour, to get across calms and in and out of harbor, an enormous advantage would be gained. Unfortunately the hydraulic-propeller had not had many friends. Whenever he had approached the ship-builder on the subject he had been told that it would cost a great deal of money being a novelty, and that at once had put an end to the idea. On one occasion at Portsmouth a lady, in delicate health and unable to stand the vibration of a screw, was towed about in her yacht by a tug. He endeavored to get a turbine put into a vessel that the lady was having built, but it was stated that it would have added £6,000 to the cost. He should be glad to see hydraulic propulsion tried still further, and felt sure that better results would be obtained if it were tried fairly on its merits.

Admiral Ryder observed that he had paid some attention to the subject from the commencement. He had taken a run in the Nautilus, and two or three trips in the Waterwitch. A few days ago he went out from Portsmouth to Spithead in Torpedo Boat No. 87, which was irreverently called The Squirt. It was a moderately fine day. The vessel was a remarkably good shape forward for riding over the sea, but the two tubes for ejecting the torpedoes were unfortunately conduits for seas that came over forward and which dashed through the tubes against any one on deck with great force. The tubes should have wooden plugs. When going at 12 knots an hour, he said to the lieutenant in charge, "I think you may ease her," and his reply was, "We must either stop or go full speed." If that was the fact, it was certainly an unfortunate position for one of Her Majesty's vessels to be in. The sensation was peculiar when the boat stopped short. She stopped very dead, which, of course, was what was wanted in the case of a torpedo-boat. But he could not make out why the ejection orifices were above water. The noise was like soda-water bottles going off continuously. The tremendous noise would, he should think, on a calm day inform an enemy 10 miles off of the approach of the boat. He could not believe that it was in the specification that the orifices should be above water. Perhaps there was some good reason for it, but from his point of view as a sailor this feature was a most objectionable one. The nuisance of the noise was intolerable, and he thought that any Admiral would send the boat home by the first ship; he hoped that in future designs the orifices would be under water. One of the great advantages of that class of torpedo-boat was the freedom from danger of fouling. Seafaring men and engineers could easily picture what would happen on going into a harbor protected by nets with the ordinary form of torpedo-boat having one or two enormous screws; these would be sure to come in contact with any obstruction of that kind. It was all very well for the open sea, but he did not believe that such a boat would

et inside an enemy's harbor at all. The turbine principle was a capital one for that purpose, because there could be no fouling by nets or anything of the kind. Another valuable feature was the power of changing the course to the opposite direction suddenly. A torpedo-boat fitted with any system of screws could work end on, bow foremost, but could do nothing against the enemy stern foremost; whereas if the turbine principle were further developed, and applied to larger vessels, an enormous advantage would be gained; indeed, the utility of the ship might be almost doubled. After having gone through the enemy's fleet, and struck or missed a ship, a turbine vessel might at once reverse her engines and retrace her course, without exposing her side, as the screw-vessel would do when turning. Of course the great question was whether the numerous advantages named by the author, not omitting that invaluable feature that the turbine-propeller could not be "pitched" out of the water, as the screw frequently was, were worth paying for, and to what amount. They might be paid for in reduced speed.

Would it be worth while sacrificing 2 knots, or how many knots, an hour to obtain the advantages that had been described? A committee of naval officers would soon come to a determination on that point. If the authorities would not pay in speed, would they pay in money? It might be necessary to get a larger or a narrower boat, or to give up some other feature to which importance was attached. He agreed with Admiral Sir George Elliot and Admiral Selwyn that the turbine question was not yet settled. The system had been tried by different nations. Swedish naval officers attached great importance to it. When he was commander-in-chief at Portsmouth, whenever a Swedish man-of-war came in, he always asked the officers how the turbine system was getting on. Their reply was that it was so unpopular with the Construction Department that they were not at all sanguine that it would be favorably received. At length, however, a turbine vessel was tried, and, on enquiring the result, the reply he received was, "They have managed to make it fail." A round sum in thousands of pounds should be offered for the best design of a turbine torpedo boat, capable of a speed of 20 knots per hour, and with orifices under water.

Mr. J. Wright, C. B., Engineer-in-Chief of the Navy, said that the subject had been so fully and fairly discussed in the paper that he had very few remarks to make; but there was one point which had not been much referred to during the discussion, namely, the great advantage supposed to accrue to a vessel with a hydraulic propeller in turning power. It had been tried in the case of the Waterwitch and the Viper, and the advantage was certainly very much in favor of the Viper. It took the Waterwitch twice the time to turn a circle that the Viper took when one screw was worked ahead and the other astern. Another point of importance was the great advantage claimed for the hydraulic propeller in clearing the ship of water in the event of her getting a hole in the bottom. The author had referred to the practical difficulties which would have to be overcome in opening and closing large sluice valves in order to carry out such an arrangement. From some of the preceding remarks it might be supposed that the naval architects of the present day had given no attention to the important matter of saving life on board ship, in the event of a large hole being made in the bottom; but any one acquainted with modern ships of war would know that an extensive subdivision was carried out for that very purpose, so that if two, or even more, compartments were penetrated, a large power of flotation would still be left. If a hydraulic propeller were fitted for ejecting water from the ship, it was just possible that if a hole were made by a torpedo in the compartment containing the propeller, the propeller would be disabled. If, again, a hole were made in another part of the ship, there would be a difficulty in getting the water under the propeller in anything like sufficient quantity to keep it charged. No doubt Admiral Sir George Elliot had made an improvement by closing up the holes in the Waterwitch's channel forward, and opening a direct hole under the bottom. The coefficients of the performances (though one gentleman had declared his disbelief in coefficients) showed an improvement of something like 15 per cent. But, with regard to other experiments

made by Admiral Sir George Elliot, in curtailing the size of the discharge nozzle, there was a decided falling off to the extent of over 7 per cent. There was another point with reference to the Viper, namely, that the screw propeller acted on ten times the amount of water that passed through the turbine. If any one would examine the theory of the subject, he would find that that was a very important matter in regard to the superior efficiency of the screw. Comparing the two torpedo boats in the same manner, it would be found that the screw boat acted upon five times the amount of water that the turbine had acted upon.

Mr. R. Sennett observed that there could be no doubt that the torpedo boat referred to was the outcome of engineering skill of the highest order, and of workmanship of the greatest perfection; and with what result? Practically this, that the speed attained was no greater than could be obtained from a screw-boat of equal size working at about one-half the power. The causes of this defective performance were not difficult to determine, and were, in the main, inherent to the system. All propellers worked on the same principle, namely, the projection of a volume of water sternward, and the thrust of the propeller was measured by the momentum or change of momentum produced. The more directly sternward the water was projected the greater the efficiency, all transverse motion representing wasted energy. In the jet every particle was sent directly astern, and therefore, theoretically, neglecting friction, the jet was the most perfect of propellers. It was from practical difficulties that the pump failed as a propeller; and though engineers were loth to apply the word impossible, yet in this case many of the obstacles were apparently insurmountable, and in his opinion the pump was never likely to become a general and efficient propelling agent, though it might, perhaps, be useful in some special cases. This arose mainly, as had just been pointed out, from the large volume of water that had to be dealt with, and the large orifices that would be necessary in the ship's sides to obtain high efficiency of propulsion. The thrust of a propeller depended on the mass of water operated on, and consequently, other things being equal, the propeller that could operate on the greatest volume of water would be the most efficient. Perhaps the custom of speaking of this propeller as a jet propeller might unintentionally convey the idea that it was different in principle from other propellers. This, however, was not so. The jet was no more the propeller in this case than was the race of the screw or paddle in ordinary ships. As a matter of fact, the pump was the propeller, for it was the agent that drove the water astern from the momentum of which propulsion resulted, just as the screw and the paddle were the machines for driving the water astern in ordinary cases. In the hydraulic boat the propeller was placed inside, in the paddle and screw outside; the action in each case was the same, only the machine was different. Until pumps were fitted that could cope with as large volumes of water as the feathering wheel, and at about the same speed, the hydraulic propeller would not be able to approach in practical efficiency either the screw or the paddle, irrespective of the greater losses that might be expected from the friction of the water in the passages. If the pump were made to satisfy these conditions, the large orifices necessary would, in most cases, be inadmissible, whether above or below the water. Then, also, the question would arise, would it be desirable to place such a propeller in what would practically be a trunk in the ship, occupying valuable space, and involving the carrying of a considerable weight of water? Or, would it not be preferable to place the propeller outside the ship, and make proper provision for a sufficient flow of water to it, without compelling the water to pass through and be carried by the ship? He had little doubt as to the answer that would be given by the vast majority of naval architects to this practical question.

Mr. John I. Thornycroft said that Captain Heathorn appeared to convey the idea that the friction of the external surface of the stream of issuing water had some advantageous influence in propelling a vessel, and he applied his remark to an experiment which he had made where an induced current of water was used to augment the propelling effect. He desired to go into some detail in order to explain the matter. A stream of water issuing from a vessel had a re-

action depending simply on the weight discharged in a given time and the velocity of discharge; the effect was unaltered by the form of aperture at which the stream found exit. The stream being submerged made no change in the reaction of the initial stream, but it then was in a position to induce other streams which might act favorably or unfavorably in propelling—favorably when the pressure was reduced on a surface attached to the vessel which had a motion towards the loss of pressure, and the contrary, if motion of the surface was in the opposite direction. One way of taking advantage of this principle was to surround the stream of water from an ordinary screw propeller by a conical tube of such dimensions as to allow an annular stream to pass through it around the stream from the propeller. When this was so placed that the initial stream entered at the larger end of a portion of a cone, and by the friction of its external surface and intermingling with the surrounding water, induced a stream through the coned tube, the pressure within this tube would be reduced, and the forward component of the difference of internal and external pressure would be available for propulsion. But, in order to estimate the useful effect, the friction of the surface of the coned tube must be taken from the forward component of the pressure already alluded to. Experimentally he found that if a vessel was fitted to utilize the currents so generated, although it would be able to exert a greater pull at a fixed object, in still water this advantage would diminish as speed was attained, and it appeared that at a useful velocity of vessel the apparatus was a failure; but he thought the result obtained by Captain Heathorn must be explained in another way. He thought that Captain Heathorn had fallen into an error. He had described an experiment that he had made, and he understood him to say that from water issuing from a pipe he got a certain reaction, and by contracting the pipe so that less water came out he got a greater reaction. He thought it was evident that in the first case most of the work was done by the friction of the water in the pipe, and in the second case, although less water came out, it came out at a higher velocity, and so the result was obtained. Captain Heathorn's allusion, therefore, to the apparatus to which he had referred was not correct. Admiral Selwyn had alluded to a pump, which he said gave a certain duty, and he spoke of 100 H. P. working a disk of 30½ inches in diameter at eight hundred revolutions, and raising 800 gallons a minute 90 feet. On looking at it, Mr. Thornycroft expected to find a good result; but he had computed the efficiency to be only a little above 0.22. Admiral Selwyn further stated that there was a pressure of 45 lbs. per square inch, and that to that would have to be added 800 gallons per minute discharged on the top of the 90 feet. At first he was puzzled to know how that was to be added; but in taking into consideration the 8-inch pipe, which he described, he found that the water flowed through it at 6 feet per second, and what was really to be added to the 90 feet was, therefore, the head due to the velocity of 6 feet per second. On working that out he found that it came to about ½ foot, and instead of lifting water 90 feet it was lifted 90½ feet. He did not know what the percentage was, but it was perhaps 22 or 22½, so that he was afraid that the pump mentioned by Admiral Selwyn did not compare favorably with his own. The discharge of the pump described by Admiral Selwyn was immensely less than the discharge from the pump of the hydraulic boat, although the disk was the same size and the revolutions were nearly double. Admiral Selwyn had stated that in what he considered a perfect pump the water discharged had the same velocity as the periphery of the disk. That, he thought, was entirely illusory. In one case it might be nearly correct. If only a little water was allowed to flow through a large pump, so that the water was a long while in the disk, no matter of what shape the vanes were, the water in the disk attained practically the same velocity as the disk of the pump. He believed that when the flow was limited in that way, all pumps gave about the same lift with the same speed. He had exhibited some diagrams in which the form of the blade had been considered. In the case of the hydraulic boat that he had constructed, the discharge was large, and the motion of the water through the disk was rapid; therefore, the effect of the blade form was very

manifest. In considering the effect of the shape of the blade, it was necessary to take into account the speed of the water from the center to the edge of the disk. The result of the diagram was to show that, taking into account the form of the blade, the water flowed through the pump at just the velocity it ought to do. In the pump of the Waterwitch, the sectional area of the channel was so large that the water made a turn, and half another turn, around the disk before leaving the pump disk.

The result came out that the velocity of the periphery was 29.6 feet per second, and the velocity as measured in an equal interval of time was 29 feet. It was exactly 29 in the author's figures, which was perhaps too close a coincidence. In the case of the Swedish pump the result was not so coincident, the velocity being only 21 out of 23 feet per second. Admiral Selwyn, therefore, was incorrect in saying that a perfect pump was a pump in which the velocity of discharge equaled the velocity of the periphery. He might say further, that if instead of using the radial blade a scoop blade throwing the water forwards were used, under favorable conditions it would possibly make the discharge equal to double the velocity of the periphery, it being assumed that friction might be neglected. It would, therefore, be more than a perfect pump, because by Admiral Selwyn's estimate it would be a pump with an efficiency of 2. Captain Heathorn had stated that the channel screw was invented by the late Mr. Robert Griffiths. He believed that Mr. Griffiths did put a screw in a tube; but the idea of putting a screw in a tube with vanes to alter the direction of the water from the spiral to a direct run out was due to Mr. Arthur Rigg. He put curved vanes behind the screw, and so got some advantage; but he did not contract the channel as the velocity was increased, and in that way secure the full benefit from the arrangement. Admiral Selwyn had calculated the reaction of Mr. Gwynne's pump at about 2 tons. That was surely too high, because he found that a cylindrical column of water 90 feet high and 8 inches in diameter would weigh about 1 ton. In a jet-propeller double that, or about 2 tons, would be obtained if it issued at its own speed, but in the case in question it only issued at 6 feet per second. If the water would issue as slowly as that, and give all the reaction, it would be correct; but, as it would escape at a much greater speed than that, the water would not supply the nozzle. Admiral Sir George Elliot had referred to the Waterwitch, and to the way in which the water was led to the propeller. He believed his statement was correct, that the arrangement for leading the water to the pump through a canal was inefficient. Where the water entered through parallel plates the velocity was not converted into pressure. In order to convert the speed of a stream into pressure it was necessary that it should enter the small end of a tapering canal. He was not surprised that the channel was found from its surface friction to retard the effect of the propeller. Admiral Sir George Elliot had stated that the discharge seemed to be independent of the size of the nozzle. But Mr. Thornycroft thought there must have been some inaccuracy in the measurement of the speed with which the water had been ejected. With reference to the question of stopping a ship in its own length, it should be remembered that was quite a different thing from stopping a boat in one length. It appeared to be intended to put very large powers in ships; but in the case of a merchant steamer steaming at 14 knots per hour it was admitted that in starting the vessel the propeller was acting efficiently. A large steamer that would go at 14 knots per hour took a long time to attain full velocity, and if the propeller acted efficiently he supposed it would take the same time to stop. Admiral Sir George Elliot considered that the trial to which reference had been made had not been a fair one, and that the boats could not be fairly compared. Nevertheless, the best had been done, and even looking at it at the present time, after seeing where a slight failure might have happened, he did not know that if the work had to be done again it could be done much better. He thought it must be admitted that the discharge being above water-mark was a very great inconvenience. Whether the water could be discharged without loss below water-mark he was not sure. It would involve carrying a great deal

more water within the boat, and it would have to be taken obliquely out of the skin. If the present nozzles were projected into the water so that they were forced through the sea he was sure the loss of speed would be very great.

Mr. G. B. Rennie remarked that the extraordinary speed of 17½ knots per hour in the case of the last torpedo-boat had been arrived at after a great many trials; the speed of the first boats of the same size had been only about 14 knots, so that the comparison was hardly a fair one. In trying various forms of pumps several years ago, in experiments for some large pumping machinery,* he found that the efficiency of the "Swedish" pump far exceeded that of the Thornycroft, which was very much like the pump described by Professor Rankine.† The total efficiency of the pump and engine varied from 0.30 to 0.50. He did not think it got so low as 0.25, as in the cases alluded to in the paper. The way in which the calculation was made was this: the dock was full, and it was pumped out; the level was taken every ten minutes, and the H.P., and the total quantity delivered was divided by the H.P., the result being from 0.30 to 0.50 of efficiency. In the case of a propelling-ship he could not see why that should be very much varied if the proportions were suitable. It had been stated that one of the problems to be solved in the hydraulic-propeller was to get sufficient water to propel the vessel. In the case of the Viper it appeared that about five times the volume of water was discharged or pushed aft as compared with that in the Waterwitch. That had always seemed to him a great difficulty with the hydraulic system. About three years ago his firm was asked to supply a floating-dock at a certain distant port. There was a great difficulty in re-erecting the dock abroad, and towing it was considered to be an impossibility. The question was how it was to be propelled. A screw-propeller seemed to be inadmissible from the draught of water, and from the inconvenience of a screw-propeller when docking a vessel. They therefore turned their attention to the question of hydraulic propulsion, taking the Waterwitch's experiments as a basis. He had a model showing how hydraulic propulsion might in such a case be advantageously employed. There were a great many compartments in the dock, and a great many pumps for emptying them. It was proposed to increase the pumps and power of the engines, and to make six apertures on each side adapted for propulsion. By that means a fair proportion of propelling area was obtained by the water discharged. The total area of all the openings was about the same as the combined area of one or two or three screw-propellers which would be necessary to propel a vessel, having a given section and capable of carrying a given tonnage, through the water. He thought that in the case of vessels of very light draught of water, and short broad vessels carrying heavy weights, the application of a screw-propeller was almost an impossibility. But while in the one case it might be impossible to apply the screw-propeller, in the other case, namely by hydraulic arrangements, it was possible to propel a vessel at a certain speed. It would be interesting if the author would explain somewhat more fully the instrument to which he had referred for taking the pressure of the discharge at the nozzles.

Mr. Edwin N. Henwood considered the hydraulic-propeller so superior to the screw, that it would be unwise to think any money wasted that would enable its advantages to be brought out, and its efficiency to be developed. If a like amount of money that had been expended on the screw-propeller were to be expended on the turbine, he maintained that the success of the latter would be secured. With reference to the comparison of the Viper and the Waterwitch, the former was a very objectionable structure; the Waterwitch was nearly as bad, but even in the latter the turbine did not have its advantages developed; it was fitted horizontally, and hence one of the streams from the discharge pipes had to be turned by a curved pipe into its proper path. The mode adopted of measuring the velocity of discharge was thoroughly unsatisfactory, as had been remarked in the paper, since it was well known that a patent log must be carefully kept out of disturbed water in

* Transactions of the Institution of Naval Architects, vol. xxiv., 1883, p. 126.
† "The Steam-Engine," p. 190.

order to get a true result. Nor did he admit that the arrangement described of a pressure-plate $1\frac{5}{16}$ inch square, supported by a more or less knife-edged lever, could be considered entirely trustworthy. With regard to the efficiency of a pump or other machine, it was needless to introduce midship section or displacement coefficient into the equation. When the designers fixed the diameter of the disk, and the number of revolutions of the engines, they had fixed the maximum velocity of discharge possible, and therefore the speed of the vessel; and no reduction of the nozzle, with the object of increasing the velocity of discharge, would materially influence the effective thrust. It appeared to him that the experiments were so devised—in error, no doubt—as to ensure no greater result than that obtained; and the paper really seemed to lead to a double dilemma. Either the Waterwitch pump was effective within $\frac{3}{10}$ of unity, or the feed was an important element in the total velocity of discharge. If the former proposition were true, then there was, under circumstances easy of repetition, no loss whatever in the pump, while in the screw there was always a large percentage of loss, without taking into account pitching, &c. If the latter were the correct explanation, not only was the result from the velocity of feed quite as great as in the Thornycroft, but the velocity of discharge was much better for the revolutions and the disk diameter. The revolutions in the Waterwitch were forty per minute, and in the Thornycroft four hundred and twenty-six; the diameter of the disk in the Waterwitch was 14 feet, and in the Thornycroft $2\frac{1}{2}$ feet. The velocity of the periphery in the Waterwitch was 29.3 feet per second, and in the Thornycroft 56 feet. The velocity of discharge in the Waterwitch was 29 feet, and in the Thornycroft 37.2 feet per second. If the velocity of discharge was more than had been observed, as seemed probable from her speed of about 10 knots per hour, those figures would have greater prominence. He was unable to find any reason for neglecting to drive the turbine at a higher velocity. The propeller in the Thornycroft boat was driven at six hundred and thirty-six revolutions per minute, and the revolutions of the turbine had been reduced to four hundred and twenty-six. He could not imagine why there should be any objection to driving the turbine at a higher velocity. He should like to ask if there was any objection, and, if so, what, to working the turbine in a vertical direction so as to discharge the water underneath the bottom of the vessel? Allusion had been made to the great difficulty in utilizing the turbine for keeping down any leak. He thought the difficulties were of a very slight character, and could easily be overcome. He wished also to ask whether the speed of the circulating water from the condenser was as great as that of the pump discharge, and whether the discharge was directed aft. He thought the superiority of the turbine-propeller over the screw-propeller in the case of fouling was very great. Everyone who had gone to sea would know what would happen if the propeller broke by meeting a floating substance. In a torpedo-boat propeller driven at six hundred revolutions a minute this would often happen. In the case of the turbine there was no such source of damage.

Captain J. D. Curtis, R. N., understood from Captain Crozier, who had charge of the experiments, that the performance of the Waterwitch was very good except when she got into a double-reef-topsail breeze. He presumed that this was on account of the pitching of the vessel. He looked upon a turbine as a kind of endless rope. Admiral Sir George Elliot had referred to the Viper being put 2 feet by the stern and going $\frac{1}{4}$ knot per hour faster. The Serapis, when put 2 feet to the stern, attained the same speed, 10 knots, with an expenditure of 50 tons of coal per twenty-four hours, as she did with 80 tons on an even keel. Reference had been made to the benefit to be derived from having an arrangement by which the screw should not foul. About two years ago the late Mr. Robert Griffiths took out a patent which he called the shield patent. This shield consisted of two plates, so arranged that they prevented the propeller acting on the deadwater, and increased the speed of a ship from 6 to 8 per cent. In addition, they prevented vibration, improved the steering, and reduced the racing when the ship was pitching. The following results had been obtained from a steam-launch to which it was applied, the own-

er, Mr. C. Bourrot, and Captain Curtis, R. N., being present at the trial:

	Without Shield.	With Shield.
Mean speed in knots per hour	6.417	6.636
Mean revolutions per minute	289	265
Mean pressure of steam in lbs. per square inch......	45.1	42.5

showing a gain of 3½ per cent. in speed, with a saving of 15 per cent. of power.

The shield was inexpensive, the estimated cost for a propeller 12 feet in diameter being only £12, and it could be put on without docking the ship. Taking the cost of marine engines at £40 per nominal H.P., and allowing a similar sum for coal, also for the space occupied in the ship by the coal, and for repairs, &c., the increase of 6 per cent. in speed, to obtain which 19 per cent. more power would be required, was equivalent to an increase in the value of the ship of £1,520 for every 100 indicated H.P. of the engines. In this shield the upper segment went home to the stern of the vessel; the lower segment over one-half the length of the screw from aft forward. Four trials were made with a small vessel above Teddington Lock, two with the shield and two without, and Mr. Robert Griffiths stated that the shield effected a saving of 15 per cent. out of the 45 per cent. which Mr. Froude asserted to be lost by the screw. How was that? When the vessel went through the water, the water must follow the ship, or the ship could not make any progress. The use of the screw detracted from the power of the water to follow the ship, and that had not been sufficiently thought of and acted upon. The shield patent must not be confounded with the cylinder, as they acted differently, the former allowing the escape of water aft. Directly the screw went down, the water came through between the apertures of the shield, and the following water flowed in to occupy the place of the water displaced by the screw. The screw did not carry the load of water round, and did not curtail the amount of inflowing water. As the screw came up to the segment of the shield, it cut the volume of water carried around in the ordinary screw, and there was no vibration. Mr. Robert Griffiths used to say that he considered the screw a hydraulic pump, and that for efficiency it was placed too near the ship's run. By another patent he placed the propeller two-thirds of its diameter away from the foremost sternpost, and in this way allowed the following water to act upon the ship, and did not detract from the speed of the vessel. In conclusion, he might observe that aquatic birds struck out alternately with their feet when swimming, economizing power, the thrust not interfering with or obstructing the following water; and that he had heard a Yarmouth fisherman, when giving directions to the builder of his smack, say. "Give her a cod's head, and a duck's tail for a stern."

Mr. N. Barnaby, C. B., Director of Naval Construction, said the discussion on the paper was remarkable, for two or three reasons, one of which was that amongst those who took part in it was the gentleman who, forty-five years ago, had patented the mode of propulsion in question. Another was that for the last twenty years the Admiralty had been endeavoring to get some sort of success with the jet propeller, and that they had not been able to point to any success. A third reason was that he believed there did not exist at the present moment any vessel in the mercantile marine employed on a passage of any kind which was propelled in that manner. Accordingly he expected that when the paper was read he should have been called upon to explain how it came about that the Admiralty still considered it right to go on trying the jet propeller; but he presumed, after the speeches which had been made by the gentlemen who had been influential during the last twenty years in urging upon the Admiralty the various trials of that mode of propulsion, that it would not be necessary for him to explain further why the Admiralty were desirous, after the Waterwitch trial, to experiment on the jet propeller again in the boat which had been described. The arguments advanced by the gentlemen to whom he had alluded were to the effect that in ships of commerce many accidents to which the screw propeller was liable would be altogether avoided; that many of the perils now incidental to navi-

gation would be removed; and that navigators would have, in the possession of great pumping power, a means of preserving ships and lives which they did not now possess. And they not only considered that that held good with regard to ships of commerce, but that there were special advantages to ships of war which made it still more desirable that the Admiralty should encourage the use of the new method. For his own part, he did not believe in pumping power. He believed that ships would be saved from perils of the sea by proper building, by proper division into compartments, and not by pumps; and even if they passed from the pumps with which they were familiar to the pumps which were supposed to be possible under the new system of propulsion, he thought they would still find that they were going in a wrong direction. But under any circumstances, whatever the advantages might be in the directions that had been pointed out, it was clear that if 2 lbs. of coal were to be burnt instead of 1 lb., for getting a certain result, it was, for the purposes of commerce, entirely out of the question to expect the jet propeller to supplant the screw propeller, or in any way to come into the field for practical adoption.

With reference to the contract into which the Admiralty entered with Messrs. Thornycroft & Co., for the object that had been described, the Board of Admiralty knew that they might expect a loss of about one-half the power, but they were prepared for that, for the sake of the merits which it seemed to them it was possible that such a boat might have. The contract was to give the same indicated H. P. as was given in the boat propelled by the screw, namely, 100; they were also to guarantee that the machinery worked well; they did not guarantee any speed. As a matter of fact, the machinery gave about 160 instead of 100 H. P.; and they had secured a boat which he believed would be found by the trials at Portsmouth to possess so many good qualities that it was very unlikely that they would be called upon to alter the machinery back again to the screw propeller. It was possible that some considerable improvement might be made in the pump, perhaps in the size of the nozzles, and it was possible—he hoped it might be proved to be the case—that there would be a sensibly better result than that which had been already obtained. The boat had the remarkable property of stopping very quickly, but she went astern badly and would not steer—two grave defects. The author had clearly shown the reason for the losses which had to be sustained in that mode of propulsion; not merely had he put before the members in a plain manner the whole of the facts in the case, but he had demonstrated this remarkable circumstance—that there was a screw turbine, a turbine boat it might be called, the turbine being put outside the boat, giving, with the light draught of water which it was supposed was only suitable for the hydraulic propeller, the same speed as could be got with the screw propeller in the ordinary way. The result of the Admiralty experiment, therefore, was a fair boat, capable, possibly, of being considerably improved; an excellent explanation of the causes of the loss in the jet propeller as compared with the screw, and an entirely new propeller, having admirable qualities. It might happen during the time he remained as an adviser of the Board of Admiralty that that department might again be pressed, as they had been during the last twenty years, to build a ship propelled by the hydraulic jet. He could only say, and he said it without the least hesitation, that such a proposal would have his uncompromising opposition.

Mr. Thomas A. Hearson said that in the hydraulic experiments under discussion the short-coming of the propeller was due to losses in the flow of the water through the pump and through the passages, and the loss was much more than could be submitted to with a continued use of the propeller. But the system had its advantages, and its advocates said that if experiments were continued the efficiency would be improved. It was therefore advisable, before continuing the experiments or condemning the system, to examine into the loss quantitatively and see how much was inherent in the system and unavoidable, and how much might be got rid of by improving the apparatus. The author had shown, and it might be accepted without doubt, that 1 ton of water was delivered per second by means of an engine of 167 H. P., with

an efficiency of 0.77, which amounted to this—that every pound of water delivered received an amount of energy of 31½ foot-pounds, in other words, received a head of 31½ feet. Of this head it might be easily calculated that only 10.4 was usefully employed in propulsion, and that 4 feet more was wasted in the energy with which the water left the nozzle. This left 17.1 feet of head wasted in some other way. If the water were imagined to be taken up gradually by the action of the pump, to be endowed with energy given to it by the wheel from the engine, and to be delivered without any abrupt change of velocity into the casing, and then delivered through the nozzles; in other words, if the pumps were assumed to be acting perfectly, how much loss would there be? The loss in such a case—that due to surface friction—was calculable. It might be expressed as a fraction or a multiple of the energy of motion at some point in the line of motion, say at the nozzle. He had calculated that in the Thornycroft boat the fraction of the energy of motion relatively to the nozzle which represented the loss by surface friction was approximately 0.4; in other words, the coefficient of resistance as employed in hydraulics to express the loss of head was approximately 0.4, not less than 0.3 nor more than 0.5. Mr. Thornycroft had succeeded in keeping it down almost to the lowest amount, the boat employed lending itself to a comparatively small coefficient of resistance, for the dimensions were small relatively to the power, and the length of the passages were short relatively to their section. Taking 0.4 of the energy of motion as an accurate estimate, that would amount to 8½ feet of head lost by surface friction, and there remained yet 8½ feet to account for. It would be instructive to work out the loss and efficiency if a value of 0.3 and also 0.5 were assumed instead of 0.4; and further, to work it out for a different velocity of flow. In the Thornycroft boat the velocity of flow was 1¾ that of the boat; what the author called the acceleration was ¾ of the velocity of the boat. If the area of the nozzle was increased 17 per cent., so that the acceleration was reduced to ½, the efficiency fraction was but little altered, being increased from 0.458 to 0.465, calculated from

$$\text{Efficiency} = \frac{2\frac{s}{v}}{(1+\mathrm{F})\left(1+\frac{s}{v}\right)^2 - 1};$$

which was derived from—

$$\text{Efficiency} = \frac{vs}{vs + \frac{s^2}{2} + \mathrm{F} \cdot \frac{(v+s)^2}{2}}$$

F being the coefficient of resistance, showing that very little, indeed, could be gained by increasing the size of the nozzle. The best efficiency worked out to 0.52, which was about the limit attainable by this method of propulsion, leaving out of account the efficiency of the mechanism of the engine. If that was good enough as compared with 0.65 of the screw, it might be continued in use; but he thought it would be admitted that it was not good enough. He wished to speak a little more of the loss of 8½ feet, to which he had referred, and which had not yet been accounted for, the additional loss over and above that due to surface friction. From the principle of momentum—the turning moment being equal to the change of moment of momentum imparted to the water—the energy delivered to the water by the engine per lb. of water might be otherwise calculated. It was equal to the product of the velocity of the periphery, and the tangential velocity of delivery divided by G, and equating that to the otherwise estimated amount of 31½ feet of head, it resulted that the water was delivered from the pump wheel with a tangential velocity of but little more than 18 feet per second, from a wheel going at a velocity of 56 feet, into a casing where the water had a much higher speed of motion, since it had, on the average, a velocity of 37½ feet. It was the eddying due to the difference in the velocities of the streams in the casing that produced the additional loss of 8½ feet—a loss which it was hoped might be got rid of by a better pump. The water came from the wheel into the casing with energy of motion given to it by the engine equivalent only to 5 feet of head. The rest of the 31½ feet was pressure head which took effect in increasing the flow, accelerating the speed of the water in the casing, and the remainder was dissipated in fluid eddying. If it were im-

agined that the streams were kept separate, and the loss for a moment suspended, then the additional 8½ feet would be employed in increasing the flow of the water. Increasing the flow would mean retarding the tangential delivery into a casing where the water would be going faster, which would increase the tendency to eddying. The pump gorged itself. It was easier to find fault than to suggest a remedy; but he would mention that to enlarge the dimensions of the casing might be an improvement, so that the water might flow round the casing with a smaller velocity and not be subject to the eddying described. To fix guiding blades in the passages leading to the wheel might be another improvement, so that the water would come to the wheel with a considerable moment of momentum, and less would be required to be given to it by the pump-wheel itself; or perhaps by making the blades more radial, less curved back, the loss would be less.

Captain H. E. Crozier, R. N., observed that as the officer who commanded H.M.S. Viper in the experimental trials against the Waterwitch, he begged to take exception to some of Admiral Sir George Elliot's remarks. He had never imagined that any comparison could be made between the ships. He had watched the Waterwitch, and it appeared to him that unless she used enormous power, she had very low velocity. When off Portland on her passage to Plymouth in a single-reef topsail breeze, and in the trough of the sea, her propelling force nearly failed. Admiral Sir George Elliot had stated that the vessels Viper, Vixen, and Waterwitch, on account of their rolling, were condemned as unseaworthy; and that the two former had been taken out to Bermuda and laid up there. The Waterwitch was laid up in ordinary at Plymouth, because she was condemned as unseaworthy after failing so signally off Portland, when she just managed to get in "by the aid of her sails" under the breakwater, was taken to Plymouth when the weather moderated, and was paid off. The Viper, on the contrary, was employed on the coast of Ireland and at Liverpool. She steamed about 2,000 miles, exposed to uncertain weather, and then made a passage to Burmuda by sail and steam averaging 6 knots per hour. If she had been condemned as unseaworthy it was scarcely possible that the Admiralty would have allowed her to make such a voyage. At the trials between the Viper and the Waterwitch at Portsmouth, the vessels started with all their weights on board. His orders were that the mean draught of the Viper should be about 12 feet 4 inches. He had an idea that by altering the trim, which he was entitled to do, he should get more speed—a more solid body for the screws to work in; and, at the same time, that by taking the weights from the extremities the ship would be more lively and seaworthy. The builders' trial was 9 knots, but he obtained, as was expected, 9.4 knots per hour. Mr. Ruthven, who was on board, complained that the trial was unfair. The Admiralty decided that another trip should be taken, and he then got 9½ knots nearly. He had stated that at the trials the Viper exerted H.P. up to 650, while the Waterwitch exerted 750. The Waterwitch had then part of her weights taken out, and she ran, not in such a state of efficiency as she should be in when going to sea, at 9.2 knots per hour. He thought that if a vessel so built managed to attain the speed he had mentioned, the same power would be far better exerted by the screw, which was more simple. Again, what would be the use of the Waterwitch in shallow water? If she went into 12 feet 4 inches depth of water, drawing 12 feet 2 inches, she would suck up mud and stones, and remain as if at anchor. As far as her propelling power was concerned, a vessel should go into action under command. Imagine the Waterwitch at full speed, at which alone she would be serviceable, struck by a shot, and some of her pumps getting out of order! It had been mentioned that the Vixen was condemned when she first went outside the breakwater. That was easily accounted for. It was because her hatches were not battened or screwed down. She nearly sank as the water got into her. Of course, when the bottom of a vessel was flat, and she had 2 feet depth of water in her, as she rolled the fires would be put out. When he took the Viper from Plymouth to Bermuda he was thirty days at sea, and there was very little rolling. It was not even necessary to have "fiddles" on the table to prevent the china and glass falling off. In all the 6,000 miles that

he had steamed in the ship he had never seen her roll heavily.

Mr. J. R. Ruthven observed that the author gave the efficiency of the hydraulic in the Waterwitch at 18 per cent., which was a force of only 4,800 lbs. to propel the ship; but to propel the ship at 9.3 knots per hour the hydraulic-propeller must have given out a force of 13,000 lbs., and so have had a total efficiency of nearly 50 per cent. of the indicated H.P. A considerable difference existed in the proportions of the boats whose trials were under comparison. The Viper, Waterwitch, and Swedish boats were all about five times the beam in length; while the Thornycroft was nearly nine times. The author had taken the coefficients from the displacements, but when the proportions differed so much the coefficients from the cross-sections furnished a better comparison of the propellers. They were as follows:

H.M.S. Viper....................	424
H.M.S. Waterwitch..............	357
Swedish screw	277
Swedish hydraulic...	171
Thornycroft screw...............	362
Thornycroft hydraulic...........	160

By these coefficients the Thornycroft screw was shown to be inferior to the screw in the Viper, still more so to the Swedish hydraulic propeller; and the hydraulic propeller in the Waterwitch was 123 per cent. better than the Thornycroft.

Mr. Joseph Bernays stated that he had had considerable experience with centrifugal pumps, but he had not tried to adapt them to the hydraulic propulsion of vessels, because he considered the principle on which they acted was not suitable for that purpose. He did not say that they could not be used; the examples under discussion showed the contrary; but in matters of that kind the question was one of final efficiency or economy. It was understood with regard to the propulsion of vessels that, in order to obtain the best effect, as much water as possible must be driven towards the stern of the vessel, and with as little speed as possible, otherwise a great deal would be lost in energy. The centrifugal pump propelled a small quantity of water with great speed towards the stern. Comparing the two classes of vessels illustrated in Plate 1 the matter would be rendered clear by merely taking the area of the screw and the area of the inlet of the propeller. Judging from the load-line, he imagined that the diameter of the screw was about 3 feet; the area of the circle would be about 7 square feet; the diameter of the inlet of the hydraulic-propeller was probably 1 foot, and the proportion of 0.785 square foot to 7 square feet represented roughly the difference in the volumes of water acted upon, and showed the necessity of greater speed being imparted to that issuing from the hydraulic-propeller than to that which issued from the screw-propeller. Another disadvantage would necessarily be the position of the pump at the bottom of the vessel, where if any damage occurred it would be very difficult to remedy it. It was stated in the paper that floating bodies were not liable to damage the internal propeller, but in reality there was a great sucking in of water into the propeller with a certain amount of speed, which made it liable to catch floating bodies, such as wood shod with iron and things of that kind. If these were to enter the propeller it would be liable to be damaged, and although it was in sight of everybody it could not be got at. If in that way the casing were damaged, he imagined that without bulkheads the vessel would be lost. Something had been said about the great power for stopping leaks, but he did not see where it was to come from, or how it was to be applied, any more than with the ordinary screw propulsion. But assuming that hydraulic-propellers were suitable for the propulsion of vessels, he did not think they had had a fair chance in the arrangements that had been exhibited. Perhaps the preference should be given to the shape of the Ruthven propeller, because even in the paper it was credited with 0.50 efficiency, whereas the more modern arrangement was only credited with 0.47 or 0.46. He did not know whether with regard to the latter any allowance had been made for the improved inlet, because if so the efficiency of the propeller proper would be still less. Many years ago he had written some letters, which were published in some of the engineering papers, with reference to the proper shape of pump-arms. He would not repeat what he had written, but would merely say that the shape of the path which a drop of water should pursue in the pump was

a curve like that stated to be followed in the Ruthven propeller, but not so long; and in order to make the water take that course, the arm must wedge itself under the water near the inlet, and must be more radial as it got out, to let the water issue from the pump-disk in a more tangential direction. He spoke of the matter without any prejudice in favor of one pump more than another. The arms forming the real propelling part of the pump, their shape was of great importance. The position of the pump, as shown in the drawing of the torpedo boat (Plate 1), appeared to him to be altogether wrong. He thought that there were many places where the power of the engine given out was wasted. First, the water was raised vertically, or on an inclined shoot, then it was bent at right angles and round the casing in all directions, whereas it was only wanted to move in one direction. Next it had to go through several small pipes, each bent, and finally it issued through a small jet. At every such change the water lost some of the power that had been put into it. An endeavor should be made to deliver the water out of the pump in as broad a stream as possible, so as to issue with the least possible speed and with the least possible loss of effect; and that, he believed, if other difficulties were not realized, could be done by placing the pump as far astern as possible. How that might affect going astern or steering he would not say, but he thought there might be methods of getting over the difculty.

Mr. W. Atkinson observed with reference to the intake of water by the inclined shoot, that the author stated that in all previous hydraulic boats the water had been taken through a hole in the bottom, in such a way that all its velocity relatively to the ship was destroyed before it entered the pump, and that that velocity had to be restored by the pump, which involved a large waste of power. It appeared to him that there was a misconception upon that point. In the first place it must be assumed that the water in the river or the sea was stationary, and that the boat only was in motion. As the water was not in motion, it was evident that no energy had been put into it, and that there was no loss to avoid. If it was thought necessary to adopt a scoop in order to bring the water up, then it would be merely a question whether that scoop had better be used in that particular position, having already employed the power of the engine or of the machine to propel the boat, or whether it was better only that the pump itself should draw it up without the scoop. Credit was taken for the fact that in the arrangement described the relative velocity of the water and of the boat was not lost. It was, however, he thought, manifest that if the scoop were taken a little further up, and brought to the vertical line, the water would have acquired the velocity of the vessel before it entered the turbine. It had arrived nearly at that position, and it was evident that the greater portion of the velocity of the vessel would have been given to the water, and if it could not be utilized afterwards by changing its motion, all that energy would be lost. But it was not lost; it was merely the portion due to friction which was lost. If the turbine was lowered almost to the level of the bottom, the scoop done away with, and the hole for taking up the water elongated, then the lifting power of the pump would virtually do all that the scoop did, and this advantage would follow, that the motion of the vessel would not have been communicated to the water; part of the water would not have motion communicated to it, and a certain amount of loss would be avoided. It appeared to him, therefore, that the scoop was a mistake. With reference to the chamber for the distribution of the water into the vanes of the propellers, it would be seen that in the model of a parallel flow turbine exhibited there were guide blades, the object of which was to transfer the pressure of the water into rotary motion, so that when it entered into the chamber of the pump, the inner portion of which was rotating perhaps at about thirty feet per second, it should enter without shock and loss of power. He should like to know why the guide chamber had been omitted, because he had never known any turbine arrangement without one. He thought if it was desired to perfect the pump, an efficient guide-blade chamber should be introduced, and at the top an inverted cone, as was shown in the diagram of the Waterwitch pump, where, however, it was used for a different pur

pose. There would then be a second guiding power. The water which was forced in a state of great commotion into the chamber would be let off gradually in each direction. It had been stated in the discussion that the object was to drive as much water as possible astern. He thought that the object was not to send the water astern, but to propel the vessel ahead. In driving water astern, the energy due to the motion greater than the motion of the water in the sea was lost. The paper stated that the velocity of the vessel was 22 feet per second, and the velocity of the jet 37 feet. He supposed that the difference between those two velocities was equivalent to 16 H. P. lost. He believed it had been admitted by Mr. Thornycroft that it would have been better not to have had the nozzles at so great an elevation. If the water by that arrangement was raised only 2 feet it was equivalent to a loss of 8 H. P.

That, he thought, would indicate the direction in which there were great and unnecessary losses. If it were really necessary to have that particular form of hydraulic propulsion, he thought it would be much better to place the axis horizontally, which would no doubt get rid of a great deal of choking up the passages, and there would be a direct flow of water through the propeller, and economy would be the result. The axial or parallel propeller appeared to be the best. He wished to direct attention to a turbine with an inward flow, that had, he believed, yielded the highest efficiency of any turbine ever constructed, and no doubt it could be appropriately placed in a vessel of the kind described, and would give much greater efficiency than the one under discussion. Probably one such turbine on each side of the boat would be the best arrangement. It was an American invention, and called the Victor, being a modification of the Hercules. The guide-blade chamber formed the outside casing, inside which was the register rate and the vanes, as in an inward flow, but with the addition of another set of vanes as in a parallel flow turbine. Thus, within a small space there was a large vane surface, with the result that for the same weight a greater power could be developed than in any other turbine. Drawings of it had been given in Emerson's "Hydro-dynamics," 1882, p. 124.

Mr. W. E. Rich observed that the author had quoted the statement of Lord Dufferin's Committee, "We are of opinion that the system is deserving of a more thorough trial than it has yet received," and he thought if the Institution of Civil Engineers were called upon to advise upon such a subject at the present moment, they would have to endorse those remarks made thirteen years ago. He believed that Mr. Thornycroft, who had done a great deal in his first attempt, had not attempted to remodel the vessel or its machinery in any essential particular since his trials were made; and probably if he had to construct another vessel on the same system, many points brought out by the experiments would lead him further on in the right direction. The late Professor Rankine laid down the principle which was now generally recognized, that the chief reason why very high efficiencies could never be looked for from the hydraulic system of propulsion was that another machine was introduced whose efficiency must be allowed for. Mr. Thornycroft had already obtained a speed of 12 6 knots per hour, and from a general review of the calculations given in the paper, Mr. Rich was of opinion that 15 knots was a possible speed with with the same boat and engine power. The great point in which improved results might be obtained was, he thought, in the efficiency of the fan. The author showed an efficiency of fan and engine combined of 0.35, and then estimating that the engines gave 0.77 efficiency in themselves, he credited the pump with the remaining 0.46. But the Thornycroft engines gave a much higher efficiency than 0.77. The traction engines tested by the Royal Agricultural Society at Wolverhampton in 1871 gave an average of 0.84, and the portable engines tried at Cardiff in the following year gave a mean efficiency of 0.825. He did not think that the engines of the Thornycroft boat were inferior to these, for probably none were made with greater care and more frictionless. Taking the Thornycroft engines, therefore, as capable of doing 0.83, the efficiency of the fans would be 0.42. As long ago as 1851 the late Mr. Appold, with the assistance of other gentlemen, determined that one of

his earliest pump-fans, 12 inches in diameter and 6 inches wide, pumping 1,250 gallons a minute, with a 10-feet lift, gave an efficiency of 0.68. A centrifugal pumping engine, for draining Whittlesea Mere, was erected in the following year under Mr. Appold's supervision, and twenty years afterwards, when Mr. Rich assisted in testing it, the engine and pump were giving a combined efficiency of 0.45. A larger engine and pump, which were substituted for the originals shortly after these trials, gave 0.53 of efficiency. The Witham drainage engines and pumps, which he tested in 1873, were much more powerful, and gave a combined efficiency of 0.63, which he believed was about the highest result on record. They were throwing from 350 to 400 gallons per minute, as measured over a gauge weir. Of course, with a centrifugal pump, as with a turbine, there was for every fall or lift one speed, and one volume of discharge at which it would give its maximum efficiency; and if the proper relations between fall speed and discharge were not strictly regulated the efficiency decreased very much. He had not examined the details of the Thornycroft fan analytically, nor was he in a position at the moment to offer any advice as to the best way of improving it; but from the experiments of Mr. Appold and others, he was inclined to think that a broader fan with a smaller diameter, and driven faster, would probably give greater efficiency than the fan exhibited. He also thought that if the skin of the fan could be kept further from the skin of the casing, better results would be obtained. In the construction of fans working centrifugal pumps skin friction was an important element. In the Witham pumps, with fans 7 feet in diameter and 2 feet 2 inches wide, skin friction absorbed a material part of the whole power. There was not only skin friction on the circular surfaces, but there was the skin friction of the water passing through the passages. The result was that a small fan often gave a higher efficiency than a large one. The fan of the Swedish pump resembled that of the late Mr. Appold, but its blades curved more. He had substituted a fan with blades shaped like those in the Thornycroft propeller, about nine years ago, for an Appold fan at a polder pumping station near Amsterdam. The object in putting it in was to get the same quantity of water lifted with a lower engine speed, and this had been attained; but there was no evidence of improved efficiency in consequence of the change.

Mr. W. Schönheyder said it was important, in arranging the propulsion of a boat by hydraulic power, to have a pump of the best shape for efficiency. It appeared, from what had been done with centrifugal pumps by various engineers, that there was a great diversity of opinion as to the best form of pump both in the cross-section and in the shape of the blades. He was certain that if engineers would pay attention to what had been already accomplished with turbines, and would remember that a centrifugal pump was only an inverted turbine, they would not have such poor results as had been from time to time recorded. The best information with regard to the centrifugal pump was, he believed, to be found in Mr. David Thomson's paper, read on the the 14th of February, 1871,* which contained an account of what had been done by the late Mr. Appold. Mr. Appold had experimented upon fans with arms of three shapes, radial straight arms, straight arms placed at an angle with the radius, and curved arms, such as those in the Swedish pump. Naturally he obtained the best results with arms like those in the Swedish pump, but that was no proof that that was the best possible section. The object, of course, in constructing arms should be to give the water a gradual rotary motion, cutting through it like a knife through a piece of cheese on entering, and then gradually getting the rotary motion, and finally giving it the proper direction in which it was wanted to run. That, of course, was the reverse of what was found in turbines. In the blades experimented with by Mr. Appold, the curve was that of a screw of uniform pitch. The water being moved around by those blades at once acquired a certain rotary motion, and no increase of rotary motion was given by them afterwards. The effect was a blow to the water, which must very much diminish the efficiency, in accordance with what had been laid down

* Minutes of Proceedings Inst. C. E., vol. xxxii., p. 26.

by the best authorities on turbines. The Thornycroft wheel was a better pump than the Swedish pump or the Waterwitch. Mr. Thornycroft had shown the path which the water took in passing through the wheel, but unfortunately he partly condemned his own work. It would be seen that the water entering the vanes took first of all a radial direction, or rather a radial direction partly backwards, and when it had passed halfway through the vanes a rotary motion was given to it gradually. From the diagram it appeared as if the vanes were made, he scarcely knew how, and afterwards the path which the water took was found; instead of which, first of all, the path of water should be determined, and then the vanes made of a proper section to give it that path. It appeared as if the first half of the length of the blades was useless, or worse than useless, because the water passed through them, and lost a certain amount of efficiency in friction. If half the inner length of the blades was cut away, he believed that better results would be obtained, motion would be imparted to the water in the same way, and there would be less frictional resistance. He did not know how far the diagram of the Waterwitch pump was correct—no doubt it was correct from the data possessed by Mr. Thornycroft, but probably the delivery had been very much underrated. It would be seen that the water received a sudden blow immediately it entered the arms, which it ought not to have, and which would necessarily occasion loss. Mr. Thornycroft was correct in stating that Mr. Rigg was the first to use guide-blades for a propeller, but not in stating that he used curved guide-blades, for they were perfectly flat. The water was assumed to strike against them like a billiard ball against the cushion, and fly off at the same angle. A paper by Mr. Rigg on the subject was read before the Society of Engineers in 1868,* and the honor of first using that method was certainly due to him. As to the possibility of a water-propelled boat ever being efficient, he thought there could be no two opinions. Instead of putting a propeller into the water to act upon the whole sea, it appeared to him to be necessary to take the sea into the boat in order to get a sufficient volume to act with efficiency.

Mr. J. P. Symes said that reference had been made in the paper to a floating fire-engine in which, by the advice of Mr. Brunel, one of the hydraulic propellers was used. Some of the previous speakers had expressed an opinion that if the axle were placed horizontally it would give better results than if it were vertical; and one speaker had said it was a pity that the hydraulic propeller had not been tried on a boat of exactly the same dimensions. Messrs. Thornycroft in their experiments had varied the dimensions of the boat somewhat, for the purpose of giving an advantage to the hydraulic propeller. The floating engine referred to in the paper was still in existence. It was fitted with a centrifugal pump with the axle horizontal, and was driven by means of gear, but did not exist at the present as a hydraulic-propelled boat. This vessel had been built by Messrs. Ditchburn and Mair in 1855, and the machinery, including the centrifugal pump for propelling the vessel, the steam fire-engine and the gearing for driving the centrifugal pump, were fitted by Messrs. Shand, Mason and Co., the engines being high pressure, but the results obtained were not very satisfactory as to speed. The boat was 100 feet long, 14 feet 9 inches beam, and had a mean draught of 4 feet 4 inches. The design of the boat was very favorable for a moderate speed—a much higher speed than had ever been obtained by the hydraulic propeller. Water was taken in at the flat bottom through a square hole without any particular guide. Originally the water was discharged below the water-line, which some persons seemed to think an advantage; but some time after the jets were removed and placed a short distance above, as described in the paper, but the difference in speed was scarcely perceptible. He was not able to say whether the jet protruded outside the vessel; if it did, that no doubt would account for there being no difference in the speed, because the friction of the jet outside would be sufficient to re-act against the advantage of placing it below the water. The speed obtained by the boat, as far as he had been able to ascertain, was never higher than 6 knots per hour. There was always great difficulty in keeping the gear

* Society of Engineers. Transactions for 1868, p. 202.

in order, so much so that at last the vessel was towed to fires instead of using the hydraulic propeller. In the year 1878 the Metropolitan Board of Works, which had charge of the fire brigade, decided to have the vessel converted into a twin-screw boat. Here was a vessel of the same dimensions, with the same displacement and draught of water forward and aft, and by converting it into a twin-screw boat an opportunity would be afforded of seeing if any advantage would be gained. His opinion was that Mr. Thornycroft in altering his design had done so in favor of the hydraulic propeller. He could not give any indicated H.P., but the basis might be taken as boiler power. It was proved that the engines driving the centrifugal pump used all the steam that could be generated by the boilers; and as the boilers were left in, they had the same basis of power. Two pairs of high-pressure engines were fitted in by Messrs. A. Wilson and Co., of Vauxhall, with cylinders 8 inches in diameter and 9 inches stroke, the propellers were 3 feet in diameter and 5 feet 6 inches pitch, from two hundred and sixty to two hundred and eighty revolutions per minute were attained. On a trial with the twin-screws a speed of 10 knots per hour was obtained against 6 knots for the hydraulic-propeller with the same boat, the same displacement, and the conditions in fact almost the same. He thought this confirmed the author's opinion and experiments that even with exactly the same boat the results would be much in favor of the screw-propellers for the power developed by the engines, so much so that the hydraulic-propeller had no chance in the competition.

Mr. Sydney W. Barnaby, in reply, said he thought the institution as a body had reason to congratulate itself upon not being responsible for some of the facts and opinions that had been advanced during the discussion. So many criticisms had been made upon the paper, that he should not reply to them all. Of course, with a large hole in the bottom of the boat, it would be likely that floating material would get in, but to prevent that a grating had been supplied, and although it checked the admission of the water, it had the advantage of keeping anything out except a certain amount of sand and gravel, which the pump threw out in very shallow water. He could not approve of the suggestion that the pump should be at the stern of the boat. The reason for putting it in the center was plain; the object was to get as big a pump as possible, and as the boat was small, the center was the only place for it. There was a further benefit from the extra displacement obtained by the increased section of the boat abaft the inlet. The further forward the pump was taken the greater the advantage of that extra displacement. It had also been said that the pump would be more efficient if it had guide-blades. The fans of the pump were so adapted as to take the water without shock, and to gradually accelerate it, and he thought when the pump was so devised the guide-blades were not necessary. With respect to his coefficient of efficiency of the engines, he had taken some pains to get at a fair estimate. He had written to Mr. Rich, knowing that he had made many experiments, and he gave him a probable efficiency of about 0.80. Mr. Lilliehöök, the designer of the Swedish boat, had taken his efficiency as low as 0.72; and Mr. Brin, the Italian constructor, about the same. Mr. Wright gave 0.77 as a more probable efficiency for a marine steam-engine. It should be observed that although the Thornycroft engine was no doubt as efficient as any other, still in the trials the engines were quite new in the boat (after a little running no doubt they would go more freely) while those of Mr. Rich were running in their most efficient condition. It was, in fact, a trial for economy. With regard to the use of the scoop for preserving the velocity of the feed, the water had a velocity relative to the boat, equal to the speed of the boat. If the water was admitted to the pump by a hole at right angles to the direction of its motion, all that velocity was instantly destroyed. In other words, the water had a forward velocity imparted to it by the boat equal to the speed of the boat, out of which velocity no advantage could be obtained. When the entrance was a curved one, as in the Thornycroft boat, the water had its direction gradually changed, and it might be made to rise into a tank at a height depending upon its velocity. True it

would have a forward velocity equal to the speed of the boat if the pipe reached the vertical; but it would be the original velocity with a changed direction. It might be carried any distance in the tank, but when it was allowed to fall through a pipe curving aft it would issue with the same velocity, neglecting friction, with which it entered.

In reference to what had been said about the arms of the pump appearing to be ill-adapted to the path of the water as calculated, it should be remembered that a very slight variation in the quantity of water altered that path considerably. The pump was discharging rather more water than was intended, and it illustrated the fact that a turbine could only work at maximum efficiency in one particular set of conditions. The speakers in favor of the system of hydraulic propulsion might be roughly divided into two classes: there were first those who said they admitted that there was a considerable loss of power by the system, but looking at the great advantages which were to be obtained from it (advantages which had been described in the paper and which he thought might be easily over-estimated), the question was what would they pay for them—would they sacrifice something in speed, or would they keep the speed and make sacrifices in armor, or guns, or cargo, as the case might be? That was a question for the naval architect and the shipowner to determine, and he had nothing to say upon it. But the other class said not only that there need be no loss from the adoption of hydraulic propulsion, but that there should be a gain. Most of those speakers had commenced by saying that they did not approach the subject from a scientific point of view, and they proceeded to lay down a set of novel and astonishing principles as a basis from which to attack the results that had been arrived at. He could only say that he had endeavored to approach the subject from a scientific standpoint. He did not know whether Admiral Selwyn was one of those who made that modest disclaimer, but one of the most important, but certainly the most misleading of those novel principles was that enunciated by him when he said that the velocity of discharge was a much more important thing than the quantity; that the quantity did not matter in the least in regard to the velocity obtained. Taking this principle as a basis, he endeavored to show that by fixing the velocity of discharge at 37 feet per second, Mr. Thornycroft had thereby limited the speed of the boat to that obtained. He stated he was sure he could not obtain a speed of 17.7 knots per hour without a velocity of discharge of 91 feet per second. Mr. Barnaby submitted some figures in order to invite attention to that point, and to show the fallacy of the principle thus laid down.

$$\text{Efficiency of jet} = \frac{\dfrac{wvs}{g}}{\dfrac{wvs}{g} + \dfrac{ws^2}{2g} + \dfrac{fwv^3}{2g}};$$

$w =$ weight of water discharged in lbs. per second;

$v =$ speed of vessel in feet per second;

$s =$ slip or acceleration;

$g =$ 32.2 feet per second.

	Thornycroft Boat.	Hypothesis I.	Hypothesis II.
w.........	2210	221	22100
v.........	21.4	21.4	21.4
s.........	15.85	158.5	1.585
$v+s$......	37.25	179.9	22.985
Efficiency.	0.71	0.23	0.77

At the top was the formula given by Prof. Rankine for the efficiency of the jet. He would therefore only repeat that the first term in the denominator was useful work, the other two terms were lost work. It would be seen that in the second term w appeared in the first power only, while s the acceleration was in the second power. Therefore, if the velocity of discharge was increased the loss was increased as the square. If the quantity discharged was increased the loss increased at the same rate only. The first column showed the quantity of water acted on and the rate of discharge in the Thornycroft boat. The efficiency was 0.71. In the second column one-tenth the quantity was assumed, and ten times the acceleration, the useful work, and therefore the speed of vessel, remaining the same. The efficiency was 0.23. In this case the indicated H.P. would require to have been increased as 23 to 71. In the third column there was ten times the quantity and one-tenth the acceleration,

giving an efficiency of 0.77 with the same propelling effect. In these hypotheses no account had been taken of friction, which added seriously to the loss as the velocity of discharge was increased. Therefore the rate of discharge was not so important as the quantity discharged, and if the whole Thames had been pumped through the boat it would only have required a velocity of discharge infinitesimally greater than the speed desired. He had heard that Admiral Selwyn had built a hydraulic boat of his own. It would have been interesting to the Institution if he had explained how he had applied his principles, and with what success. He regretted that the discussion had been confined to the old battle-field of the Waterwitch and the Viper. He did not care to enter into that battle-field. If the results had proved conclusively that jet propulsion was wrong, there would have been no need for a Thornycroft hydraulic-boat or any other. They did not prove anything conclusively, and it was not until he came to collect all the information available that he found out why that was so. No experiments of any value had been made as to the quantity of water pumped, or as to the rate of discharge; it was therefore impossible to get accurately at what the duty of the pump was. He did not pretend that the efficiency of 0.18 was absolutely correct. As he had previously explained, he had taken that from the calculations made by Mr. Brin in a paper read before the Institution of Naval Architects (vol. xii.). From considerations of the probable resistance of a vessel of the size and shape of the Waterwitch, he calculated that 0.18 of the H.P. which was indicated would be sufficient to give the speed which she attained. Mr. Barnaby had been induced to take those figures almost upon faith. Mr. Ruthven, junior, had, he thought, proved too much; he had endeavored to show that the screw of the Viper was a great deal better than the screw of the torpedo-boats, but that was so obviously impossible that the rest of his calculations upon the same basis were probably incorrect. The new boats did really give a fair means of comparison. There had been no prejudice one way or the other, either in regard to the Thornycroft or the Swedish boats. In each case the designers of the screws designed the turbines, and the results had been unchallenged. The coefficients which he had given for the Viper and the Waterwitch were supplied to him by the Admiralty, and they represented trials where the displacement and the speeds were as nearly as possible alike. In the tables which Mr. M. W. Ruthven had exhibited, he had mixed up the coefficients of trials where the displacement varied from 995 to 1,205 tons, and where the speed varied as much as 1½ knot per hour; he had mixed them all up and divided them by the number. He would refer to Fig. 13 to show what the effect of that was. In that diagram was the curve of the coefficient of a torpedo-boat at the same displacement under different speeds. The influence of a difference of 1½ knot could be seen. At 9 knots there was a coefficient of 228, and at 1½ knot more speed it had fallen to 160. What was the value of any comparison between two systems of propulsion if carried out upon those two speeds, and if besides the displacements and forms were different? The attempt to make such a comparison betrayed a fundamental misconception of the meaning and value of coefficients of performance. Similar vessels of the same length should be tried at the same speeds and at the same displacements if a comparison of propellers was desired, and similar vessels of different lengths should be tried at corresponding displacements and corresponding speeds. By corresponding displacements he meant displacements varying as the cube of the length; by corresponding speeds, speeds varying as the square root of the length. Mr. Ruthven found a discrepancy between Table 1 and Table 2. He did not understand how the coefficient of the Waterwitch could be so much better than the coefficient of the Thornycroft boat, and nevertheless the efficiency of the hydraulic machine in the latter boat could be so much better than that of the Waterwitch. But there was not the smallest connection between the coefficient of performances in Table 1 and the efficiency of the hydraulic machine in Table 2. If the engines of the Waterwitch were put upon a Thames barge, or if the Waterwitch herself were moored head and stern the efficiency of the hy-

draulic-machine might be exactly the same, but Mr. Ruthven would not like the efficiency of his system of propulsion to be judged by the coefficients of performance obtained in such a trial. Admiral Sir George Elliot had claimed that jet propulsion had never had a fair trial, and that the designers had always been hampered by the conditions imposed by the Admiralty. If he had led him to believe that they had been hampered in any way by conditions as to the draught of water he had misled him. The only conditions imposed were those stated by the Director of Naval Construction, namely that the boats should be as nearly as possible the same, that a similar boiler was to be used, and that the designers were to do their best as far as getting speed was concerned. Every suggestion which they had made as to modifications in the form of the trial boat which they considered would be to its advantage had been cheerfully assented to by the Admiralty. Some of the suggestions were undoubtedly prejudicial to the efficiency of the boat as a torpedo-boat, but as it was a trial of hydraulic propulsion the designers were permitted to make even those modifications. One of them, a small one, was in the position of the boiler; it was carried out to the end of the boat, and far away from the steam impulse for ejecting the torpedoes, which was an objectionable feature, but which was considered advisable for other reasons. Another was the position of the discharges. If the discharges had been below water the noise would have been avoided, and also the serious objection arising from the large amount of white water which made the boat readily detected by the electric light at night. They had, however, fitted up a small centrifugal pump in a launch with the object of determining the best position for the discharges. The nozzles were attached to dynamometers, so that the reaction could be measured. The experiment showed an enormous resistance caused by the nozzle when below the surface; and it was clear that unless long discharge pipes were used carrying the water aft, as in the Swedish boat, where the nozzles were below the surface (Fig. 2), the discharge must be above water. Admiral Sir George Elliot had also said that the screw-boat should have been reduced to the same draught of water as the hydraulic. As a matter of fact the draught of the screw-boat under the keel was less than that of the hydraulic-boat, and it was only the blades of the screw revolving below the keel which made it exceed the draught of the hydraulic boat. Admiral Sir George Elliot would cut off the legs of the screw-boat, and make it run upon its stumps, in order that it should not outrun the hydraulic. That was not fair, but even on that ground the screw-turbine propeller would beat the centrifugal pump. As had been previously stated, in second-class torpedo-boat, with 4 inches less draught of water than the hydraulic now had, nearly 5 knots additional speed per hour could have been obtained. The little boat, of which a model was exhibited, only drew 12 inches of water, and it reached the astonishing speed of 15 miles per hour with a screw-turbine propeller. A great deal of the uncertainty connected with the subject had been produced by loose statements as to the performances which had been attained by hydraulic-propelled vessels. It had been said that the Nautilus had beaten another vessel, the Volunteer; it had been stated perhaps what the speeds of the vessels were, and their H.P., but nothing was mentioned about the displacements, or which had the advantage in point of form. It was asserted, too, that the Nautilus challenged all the Citizen boats on the river, and that they declined the contest. That, however, proved nothing. He might have come before the Institution and said, "Here is a hydraulic-propelled boat two-thirds the length of a Citizen steamer, with very much less displacement, with a boiler of only two-thirds the heating surface and a weight of machinery of only $5\frac{1}{2}$ tons against 14 tons, and yet it can run round and round any Citizen boat on the river." That would have been the truth and nothing but the truth, but not the whole truth. The whole truth was that by means of forced draught an enormous power in proportion to her size was realized. It was a great pity that statements should be put forward which proved nothing, but which led to a great deal of misconception. He thought it was most desirable that scientific questions of that sort should be treated in a more scientific manner.

Sir J. W. Bazalgette, C.B., President,

said that if the Institution, as a body, did not hold itself responsible for the views which had been expressed, he felt sure that the subject had been advanced considerably by the discussion which had taken place. He had no doubt that it would lead to further experiments, and that an increase in the rate of propulsion by different modes would be the result of the consideration that had been given to the subject. The members must also feel that the author had devoted great attention to the subject, and had made a very able reply to the many objections which had been raised in the course of the debate.

ON THE SOLAR TEMPERATURE QUESTION.

By F. GILMAN.

Written for Van Nostrand's Engineering Magazine.

I would like to make one more statement (which is my last) in regard to this question, since Prof. Wood's article in the February number of this Magazine contains some assertions which call for a reply. He says I assume that the intensity, or temperature, will be directly as the quantity of heat passing a given area of the spherical shell, but as this is erroneous my demonstration fails.

Even supposing, however, that the ether is perfectly diathermanous, and consequently that the temperature of space is zero, it will not affect the question in the least; since what we wish to determine is the temperature of a material body like that of which the sun is composed, when brought into the sun's vicinity.

If such a body when brought near the sun will have a temperature of more than a million degrees, no reasonable person would deny that the temperature of the sun must exceed this.

The temperature to which a body will rise when exposed to a given quantity of radiant heat, depends on its specific heat and on its absorptive power.

A body brought near the sun would be converted to the gaseous state, and since the specific heat of all gases is nearly the same, its rise of temperature would be proportional to the quantity of heat transmitted to it, if its absorptive power did not change.

Admitting, however, that the absorptive power of solar substance is very small, and that if conceived to be detached from the sun's surface, it would absorb but a very small fraction of the heat directly radiated to it, this would not invalidate the argument for the high solar temperature. The experiments of Prof. Tyndall have proved that the absorptive powers of different gases follow precisely the same order as their powers of radiation. Hence, the smaller the absorptive power of solar substance, the smaller will be its radiative power, and the higher must be the temperature of the sun in order to radiate the same quantity of heat. If, on the contrary, we suppose the absorptive power of detached solar substance to be very great, so that it absorbs nearly all the heat radiated to it, then it is easily shown (if the law of inverse squares from the center be true) that its temperature due to direct radiation from the sun, when near its surface, will not be less than two million degrees Fah.

Prof. Wood attempts to show that according to the law of inverse squares as applied to a point on the surface of a sphere, the temperature of this point will be infinite, and since, when the entire spherical surface is involved and the inverse squares are reckoned from the center the temperature is finite, therefore the law of inverse squares from the center reduces to the absurdity of giving a smaller temperature when the entire surface is involved than when only a point is considered.

It is evident that any process of reasoning that leads to the conclusion of an infinite temperature must be fallacious, even though that conclusion be derived from a formula.

There are many formulas which, while correct in general, utterly fail to give true results at certain limits. If, however, we consider an actual point as radi-

ating heat the formula will not give an infinite temperature at the point; for though the distance from the point be infinitely small, the amount of heat radiated is also infinitely small, and the expression for the temperature reduces to $\frac{0}{0}$, an expression which may, and in the present case does, have a finite value.

Prof. Wood says: "The nearer a body is to the sun the less will be the hot surface to which it will be exposed. So that while we may admit that the heat received from the small portions of the sun exposed to the pyrometer varies inversely as the squares of the distance from them, it also varies directly as the sum of those small portions; and since this sum diminishes as we approach the surface of the sun, the law of the inverse square from the center fails."

In reply to this I submit the solution of the following problem:

Assuming that the intensity of heat radiated in any given direction from a

And since cos. $gfc = -$ sin. gfd, we have

$$\text{sin. } gfd = \frac{gc^2 - gf^2 - R^2}{2\,gf\,R}$$

Substituting these values we obtain for the measure of the intensity due to the infinitely small zone

$$dH = \frac{\pi R \sin.x\, dx\,(2gc\,R\cos.x - 2R^2)}{(gc^2 + R^2 - 2gc\,R\cos.x)^{\frac{3}{2}}}$$

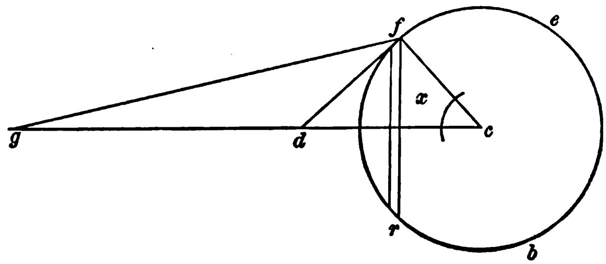
$$H = 2\pi R^2 \int \frac{(gc\cos.x - R)\sin.x\,dx}{(gc^2 + R^2 - 2gc\,R\cos.x)^{\frac{3}{2}}}$$

This integral taken between the limits $x=o$ and $x=\cos.^{-1}\frac{R}{gc}$ will include every portion of the spherical surface that can radiate heat to the point.

Let $a = gc^2 + R^2$ and $b = 2\,gcR$

Then

$$H = \frac{2\pi R^2 gc \int \cos.x \sin.x\,dx}{(a - b\cos.x)^{\frac{3}{2}}}$$



hot surface varies directly as the sine of the angle of obliquity, and inversely as the square of the distance, when the radiating surface is a point, it is required to find the relation between the intensity of the heat at a point g, and the distance gc of that point from the center of the hot spherical body feb.

Consider an infinitely small zone fr, and representing fc by R, and the angle fcd by x, the area of that zone will be 2π R sin. x R$dx = 2\pi R^2$ sin. $x\, dx$. Draw fd at right angles to fc. Then we shall have for the measure of the intensity of heat received at g from the infinitely small zone, fr, the following expression:

$$dH = \frac{2\pi R^2 \sin.x\,dx\,\sin.gfd}{gf^2}$$

But

$$gf^2 = gc^2 + R^2 - 2gc\,R\cos.x$$
$$gc^2 = gf^2 + R^2 - 2gf\,R\cos.gfc$$

$$-\frac{2\pi R^2 \int \sin.x\,dx}{(a - b\cos.x)^{\frac{3}{2}}}$$

$$\int \frac{\cos.x \sin.x\,dx}{(a - b\cos.x)^{\frac{3}{2}}} = -\frac{2}{b^2}\left\{(a - b\cos.x)^{\frac{1}{2}} + \frac{a}{(a - b\cos.x)^{\frac{1}{2}}}\right\}$$

$$\int \frac{\sin.x\,dx}{(a - b\cos.x)^{\frac{3}{2}}} = -\frac{2}{b\,(a - b\cos.x)^{\frac{1}{2}}}$$

Restoring the values of a and b, and taking the integral between the limits $\cos.x = 1$ and $\cos.x = \frac{R}{gc}$ and reducing, we have

$$H = \left\{\frac{2\pi}{gc}\left(gc - \sqrt{gc^2 - R^2}\right)\right\}$$

But

$$-\sqrt{gc^2 - R^2} = -gc + \frac{R^2}{2gc} + \frac{R^4}{8gc^3} + \frac{R^6}{16gc^5} + \ldots\ldots$$

Hence

$$H=\frac{2\pi}{gc}\left\{\frac{R^2}{2gc}+\frac{R^4}{8gc^3}+\frac{R^6}{16gc^5}+\cdots\cdots\right\}$$

When gc equals or exceeds 2R, all terms following the first may be neglected, and we shall have $H=\frac{\pi R^2}{gc^2}$; that is the intensity varies inversely as the square of the distance from the center; while for all values of gc between R and 2R the application of the preceding formula will give even a higher value for the temperature than the simple law of the inverse squares. Therefore it is evident that if the law of the inverse squares holds for a point, it may be said to more than hold for the center of a spherical body, as far as the argument for the high temperature of the sun is concerned.

MEASUREMENT AND FLOW OF WATER IN DITCHES.

By ROSS E. BROWNE, Member of the Technical Society of the Pacific Coast.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

In the April number of this magazine there appears an article with the above heading, by Chas. E. Emery, Ph.D., in which some unaccountable misstatements occur. It is asserted that Mr. Bowie has used in his calculation the *average depth* of the stream in place of the *hydraulic mean depth*. That Mr. Bowie has not been guilty of such stupidity is apparent from the simplest calculation. The diagrams, giving dimensions of the ditches named, and included in the original publication of the Technical Society, were omitted in the magazine article. This fact makes Mr. Emery's method of reaching conclusions, with regard to the coefficients of flow of these ditches, a mystery.

Mr. Emery says: "Full dimensions are not given from which to calculate r on the correct basis, but, assuming probable proportions for the ditches, it will be found that for the Texas Creek Branch Ditch, c should be 109, instead of 59* as stated, &c."

It is presumed that Mr. Emery has carefully read Mr. Bowie's paper, as published in the January number of this magazine, and was aware of the fact that the values of c were determined from actual measurement of the quantities flowing through the ditches. It is distinctly stated (page 35), in connection with the Milton ditch, that c was "determined from the gauging at the measuring box." It is also presumed that there was no intention of ignoring these measurements.

* 38, not 59.

If, then, the Texas Creek Branch Ditch is taken as example, a statement is found to the effect that the sectional area is 13.5 (square) feet, the grade 20 feet per mile, and the flow 32.8 cubic feet per second. If under these conditions the value of c is to be 109, as claimed by Mr. Emery, the "probable proportions" must be assumed such as to make

$$r=\frac{Q^2}{a^2c^2s}=\frac{(32.8)^2}{(13.5)^2(109)^2(\frac{20}{5280})}=0.131 \text{ ft.}$$

hence the wetted perimeter

$$=\frac{a}{r}=\frac{13.5}{0.131}=103 \text{ feet.}$$

The cross-section corresponding to such a wetted perimeter and an area of 13.5 square feet, would have approximately either a width of $102\frac{7}{10}$ feet and a depth of $\frac{13}{100}$ foot, or a width of $\frac{26}{100}$ foot and a depth of 52 feet. Such "proportions" do not seem "probable."

The small values of the coefficient, obtained by Mr. Bowie, are plainly due to the significance given to c. This c is evidently intended to guide one in *projecting* such ditches as are mentioned, and "covers all common sources of loss" such as sharp bends, leakage, diminution and variability of cross-section. It was plainly Mr. Bowie's purpose to furnish the means of estimating, upon the basis of the projected dimensions of cross-section, the capacity of a ditch of the character described, after the natural changes have taken place and it has become more or less permanent under reasonable attendance.

EXPERIMENTS UPON THE HARDENING OF PORTLAND CEMENT.

BY M. PERRODIL.

Translated from "Annales des Ponts et Chaussées."

THE experiments herein cited were made upon specimens of Portland cement from the works of Lonquety and Demarle, at Boulogne-sur-Mer.

The percentage composition of this cement after the complete expulsion of water and carbonic acid was

Silica	23.8
Particles of sand	1.3
Alumina	8.9
Ferric oxide	2.0
Lime	63.6
Magnesia	0.4

Eighty-eight cubes of this cement were prepared without sand on the 12th of August, 1883. Half of the number were immediately immersed in water. The other half were left exposed to the air.

At different periods indicated in the following table, four cubes of each set were subjected to a crushing strain. The fragments were immediately analyzed to determine the amounts of water and of carbonic acid that had entered in combination.

which acquire in the water a considerable degree of hardness; 2d by the action of calcium hydrate upon basic calcium silicate.

The proportions of silica alumina and lime found in the cement permit the formation of these compounds, and assuming that both the silica and alumina play the part of acids and unite with different proportions of the lime, there may result one of the groups represented by the following formulas:

$3\,Si\,O_2.\ 4\,Ca\,O$	53.46
$Al_2\,O_3,\ Ca\,O$	13.74
$Ca\,O$	29.10
	96.30

or

$Si\,O_2\ 2\,Ca\,O$	67.45
$Al_2O_3\ 3\,Ca\,O$	23.40
$Ca\,O$	5.45
	96.30

After twenty-four hours the immersed cement contains about the same amount

Age of the specimen.	Sample in Water.		Sample in Air.		Breaking weight, per sq. centimeter.	
	pr. ct. of water.	pr. ct. of CO_2.	pr. ct. of water.	pr. ct. of CO_2.	Set in water.	Set in air.
					k.	k.
1 day	6.4	0	6.1	0	43.6	33.6
3 days	8.7	0	6.2	1.8	119.2	84.0
7 days	9.2	0	6.0	1.8	141.6	104.9
15 days	12.8	0	8.0	1.0	163.4	122.8
1 month	15.4	0	6.5	2.8	234.2	103.0
2 months	14.6	0	7.6	4.0	298.4	123.8
3 months	11.2	0	4.3	6.0	231.2	126.6
6 months	15.3	0	8.3	5.8	329.6	171.6

M. Fremy explains the hardening of cement:—1st by the formation of an aluminate of lime represented by one of the formulas:

$$Al_2\,O_3,\ Ca\,O;\quad Al_2\,O_3,\ 2\,Ca\,O;\quad Al_2\,O_3,\ 3\,Ca\,O$$

and the subsequent formation of hydrates

of combined water as the cement in the air. Neither of them contains carbonic acid. The amount of water in combination corresponds to about three-tenths of an equivalent, taking the lime present as one equivalent.

At the end of a month the amount of water in the immersed cement has con-

siderably increased, amounting to three fourths of an equivalent, while in the cement left in the air it has remained nearly the same.

The carbonic acid, always absent in the immersed cement, increases gradually in the cement exposed to the air, until at the end of three months the amount is sufficient to indicate the presence of 7.64 parts of lime. Now the amount of lime in the second of the above sets of formulas is somewhat less than this, so that the composition of cement hardened in the air is represented by proportions somewhere between those represented above, but nearer the second set than the first.

A remarkable result exhibited by the table is the relation between the increase of combined water, and the increase of strength of the cement. These figures are all the more striking from the fact that a diminution of the water percentage is attended with a diminution in the breaking weight.

The conclusion that may be drawn from these experiments is:—1st, that the cement acquires a greater degree of strength in the water than in the air; and 2d that the water which enters into combination is an important factor in the hardening process, whether it forms a hydrated calcium aluminate, or establishes a more complex reaction with the free lime and then upon the silica and alumina so as to form an artificial pozzuolana.

A FLASHING TEST FOR GUNPOWDER.

By Prof. CHAS. E. MUNROE, U.S.N.A.

From the Journal of the American Chemical Society.

Among the methods in use for the determination of the condition and quality of gunpowder, is the "flashing test." According to the *Ordnance Instructions U. S. Navy*, p. 345, "about eight drams of powder are poured on a glass plate so as to form a conical heap, and 'flashed' by applying a hot iron; no residuum should be left and only a few smoke marks should be seen on the plate." Capt. Smith, R.A., in his *Handbook of the Manufacture and Proof of Gunpowder*, p. 83, proceeds in the same way, but he places the powder in a thimble-shaped, copper cylinder, "which is then inverted on the flashing plate. This provides for the particles being arranged in pretty nearly the same way each time, which is an all-important point in flashing. The decomposition of the powder will be more thorough if it be thrown together in a conical heap, than if it be spread out in a thin layer on the plate, hence, for comparison of different powders, they should be placed on the plates as nearly as possible under the same conditions.

"If the powder has been thoroughly and effectually incorporated, the small charge placed on the plate will 'flash' or puff off when touched with a hot iron, leaving only smoke marks on the plate. A badly incorporated paper will, on the other hand, leave specks of undecomposed saltpeter and sulphur, and will therefore give a dirty residue. But the 'flashing' test, though apparently most simple, is one which, like the examination by eye and hand, requires experience to enable the observer to form an accurate judgment. Though a very badly incorporated powder could be detected at once, it is by no means easy to judge between two powders, both tolerably good, as to which has undergone the most thorough incorporation. Flashing should therefore be constantly practiced with all classes of powders, and it is useful to keep some samples of bad powders to flash occasionally for comparison. Powder which has once been subjected to and injured by damp will be found to flash very badly, no matter how carefully its incorporation may have been performed. This arises from a partial solution of the saltpeter having taken place, causing a consequent disturbance of the incorporation."

Comdr. J. D. Marvin, U. S. N., in his *Objects and Resources of the Naval Experimental Battery*, p. 18, repeats the above directions and suggests weighing the plate on which the flash has been

made, but, as he provides no means to prevent the absorption of moisture and oxygen and the escape of the hydrogen and ammonium sulphides, the method is of no value.

In the *Comptes Rendus* 78, 1138; 1874, Col. Chabrier proposes, what he terms a *pyrographic* method for the examination of gunpowder and a detailed account is given in the *Revue d'Artillerie*, 4, 396; 1874, of its application by the *Comité de l'Artillerie* in determining the relative value of wheel mills, stamp mills and *moulins à tonneaux* in effecting incorporation and of the length of time necessary in each case to produce the desired result.

This method consists in flashing the powder on sheets of paper, colored blue with iodide starch. Sheets of uniform tint, 0.30 meter long by 0.15 meter wide, are dampened and placed on a plate of glass of the same size. A half gram of powder is then trailed on the paper, following the longer axis. It is flashed by a red-hot iron wire and it is found that the center of the blue paper is bleached, while black spots and streaks appear on the white ground and white spots on the blue ground. The size and shape of the bleached space and the number and arrangement of the spots and streaks are determined by the character and amount of the powder used. Col. Chabrier does not give the rationale of his process but it is to be inferred from the fact that he styles these results *pyrographic* images, that he believes the bleaching to be due to the heat evolved by the combustion. The well-known experiment of the bleaching of starch paste, colored blue with iodine, by heating in a test tube, is an example of the same kind.

This process is an advance upon the older one, but in applying it some years ago I found it difficult to prepare papers of the same degree of blueness and that the evanescent character of the color made it difficult to preserve the test papers intact for any considerable length of time; so, as before, we must either practice the method continually or else flash powders, which we have kept as standards for comparison, with each set of tests we make, in order to arrive at any good results. Or, finally, we may photograph the test papers, but this involves considerable labor and the loss of the color.

Since the flashing test is the simplest, readiest, and in the hands of an expert, the best test for the incorporation of powder, and, since it also fairly indicates the amount of deterioration which a powder has undergone during transportation and storage, it has seemed to me desirable to seek some method by which the record could be made permanent. Such a record could then be filed at the factory with the other data concerning a given powder, or, in the case of the Government, they could be inclosed with the quarterly returns of the inspecting officers, at different stations, to be examined by some expert in the bureau. Specimens of the tests of standard powders could also be furnished inspecting officers, to guide them in the interpretation of the results of their tests, and, finally, a sample of the required test might be attached to the specifications for a gunpowder to be purchased.

After some search I believe that I have secured such a permanent record, by employing a paper colored with Turnbull's Blue, upon which to make my flash. This paper is the same as that used in the "Blue Print Process," of photography, and is easily procured in commerce, The use of the paper was suggested by the following facts. When gunpowder burns, the reaction which takes place may, according to Debus, *Proc. U. S. Naval Institute.* 9, 76, 1883, be represented by the reaction $16 KNO_3 + 13C + 5S = 3K_2 CO_3 + 5K_2 SO_4 + 9 CO_2 + CO \times 8 N_2$.

Since, however, in ordinary gun powders there is more carbon and sulphur than is required by the above equation, secondary, endothermic reactions take place, which may be combined and represented as follows: $3K_2 SO_4 + 2K_2 CO_3 + 7C + 7S = 5K_2 S_2 + 9C O_2$.

Further, on exposure to the air, the polysulphides formed are oxidized to thiosulphates. I have observed that, in my experiments, the characteristic smell of the latter was most noticeable when the powder was badly incorporated.

It is well known that solutions of the alkalies and the alkaline carbonates decompose Turnbull's Blue and thereby destroy its color. Advantage has been taken of this reaction to increase the distinctness of "Blue Prints," or to make

such additions to them or alterations in them as desired. With this I exhibit a specimen of the blue paper, upon which one of the above reactions is written by the aid of a solution of potassium carbonate. The alkaline sulphides and thiosulphates also act upon the blue paper, but with less intensity and with the partial production of a yellow color. By flashing gunpowder then, upon such paper, yellow and white spots will be formed through the action of the substances formed by the reaction.

The test is made as follows: Pieces of the paper, from 15 to 20 centimeters square, are dampened and placed on a sheet of glass or copper. A truncated leaden cone 3 centimeters in capacity is closed with the finger at the smaller end, filled evenly with powder and inverted on the paper. The result is a conical heap. The heap is immediately fired, either by a hot iron or copper wire, or, as in my experiments, by a fine platinum wire, heated to incandescence by an electric current. The paper is exposed to the action of the residue for thirty seconds and then immediately placed under the spigot and washed with running water. When pulverized gunpowder cake is used it will be found that the space described by the base of the cone, has been blackened and partially bleached by the dampened layers of powder in contact with it; that about this space are black smutches and streaks, and that the whole surface of the paper is marked by white and yellow dots. Where the powder is badly incorporated the spots are coarse, and irregular in shape and distribution; where the incorporation is complete, the spots are fine and quite uniformly distributed over the surface so that the paper appears but of a paler blue, with occasional spots and few streaks.

With this, I forward specimens of tests made with powdered "mill cake." All of the specimens belong to the same "charge," but the first was drawn after the mill had been running four hours; the second, at the end of eight hours; the third, after twelve hours; and the fourth, after sixteen hours. The latter is known as the "finished composition." This length of running is rather unusual, but the charge used at the mills is greater than common. The tests exhibited were made October 19, 1883. I have yet others made April 26, 1883, which are to-day apparently as fresh and distinct as when made. It is believed that the papers show what is described above. That importance is given, in interpreting the results, to uniformity of the bleaching and in the arrangement of the spots, depends upon the fact that gunpowder is a mechanical mixture, and, therefore, that the regularity of the combustion and the uniformity of the accompanying reactions must depend upon the fineness of the ingredients and the intimacy and uniformity of the mixture. If the ingredients are coarse, and the mixture imperfect, the combustion will go on slowly and irregularly, and the resulting globules of residue will be of considerable size and be deposited near the center of action. If the incorporation is complete, the reaction will take place nearly simultaneously throughout the whole mass, and the globules will be, as a rule, quite small and projected to some distance. This interpretation is for mealed powders having the same formula. I have not yet been able, personally, to extend my experiments to granulated powders or powders of varying proportions and ingredients, but I believe that this test will form a useful method for the study of these powders.

In order that the indications may be interpreted aright, it is necessary that the conditions under which the experiments are made, shall be as nearly uniform as possible, and the first of these is, that the color of the test paper should be in all cases as nearly as possible of the same depth. The paper may be purchased in an emergency, but it varies among manufacturers owing to the many different formulas according to which it is made. Among these I have selected that issued by the Penn. R. R. Co. for use among its operatives.

"Take 10 oz. (283.5 c. c.) of clean water and put it in an opaque bottle, add 1¼ oz. (35. 44 grms), of Red Prussiate of Potash, allow this to dissolve. In a second containing 6 oz. (170. 1 c. c.) of water, put 2½ oz. (70. 88 grms), of Ammonio-citrate of Iron, allowing this also to dissolve. Add the second liquid to the first and shake thoroughly. Keep closely stoppered and not exposed to light."

"In a room, from which daylight is excluded, but where lamp or gas light may

be used, the paper to be printed on is laid on a table, and the fluid applied with a *clean* sponge. Care should be taken to apply the fluid as evenly as possible, and every part of one side should be gone over. For that reason it would be well to sponge the paper, first in one direction and afterwards crosswise to the first. When a sheet is sensitized it is put away in a drawer to dry, but never place one sheet on the top of another before they are dry; afterwards it makes no difference. Sensitized paper may be kept in a drawer for a week or more, without injuring its sensitive quality.

"In using the fluid, care should be taken to pour out no more than is needed for the time, as it would be apt to spoil the fluid in the bottle if any fluid which had been used, was poured back again.

"For the same reason the saucer into which the fluid is poured, and the sponge with which it is applied, should be washed out immediately after using and also before using."

For the purposes of this test for gunpowder the dry sheets are now exposed to strong sunlight for four or five hours. When about to use, immerse in running water for five minutes, lay on the plate of glass and remove the excess of moisture by aid of filter paper or a blotter. The paper must be thoroughly moistened but without "standing" moisture.

Since writing the above I have received from Lieut.-Commander W. M. Folger, U. S. N., commanding the Naval Experimental Battery, the following statement concerning the testing of a granulated powder by this method.

"In firing a sample of experimental powder lately, I had reason to believe, from its performance in the gun, (caliber 6″), that the powder was badly incorporated. Tested in the manner you suggested with Turnbull's paper and following all your directions, indications were furnished which (when compared with results obtained with a normal sample of approved powder) verified most definitely the value of the method you suggest."

SWIFT CRUISERS.

From the "Nautical Magazine."

It is to be earnestly hoped that the Admiralty will well consider what they are about before allowing themselves to be committed to the very questionable policy of building for the protection of our commerce, swift cruisers, of upwards of 400 feet in length, and of such other extreme proportions as have been proposed to them by a leading firm of shipbuilders.

A large Government order for vessels of this type would be a great help to the present needy condition of the shipbuilding trade; but the first question to be considered is—are such ships really wanted? It appears to be pretty generally admitted, that if war were to break out, and this country were involved, the Admiralty could not, effectively, protect the large trading British fleets from capture; therefore, it may be argued, ships of some kind are assuredly required. Allowing all this, naval men, and seamen generally, are far from being assured that such large vessels as have been proposed would be the most suitable for this important service. If it has not actually been experienced, it may very readily be assumed, that in naval warfare all vessels of great length have many objectionable points. Speed, without doubt, in this connection, is a most necessary qualification, but it is not everything; and before an order is given for such very large and costly ships, it should be clearly understood whether a high rate of speed cannot be got out of much smaller and less costly vessels.

This country can afford to spend twenty millions to protect and maintain her ocean commerce, and when that twenty is finished she should resolve to lay down another hundred millions at the back of it in order to carry out that policy; but though spending money freely and lavishly, she cannot afford to spend foolishly, or upon ships that may turn out to be failures before they have ceased to be new.

It has been said of two long and large ships, which were, to some extent, the outcome of a panic—the Warrior and the Black Prince —and which were laid down in somewhat of a hurry; that amongst other serious defects, they would neither wear nor stay. Perhaps the English of this was that they took double the time they ought to, in performing the circle—a most important evolution in naval war.

Certainly, long and heavy ships work under great disadvantages as battle ships of any kind, and particularly as cruisers, where rapid movements and ready changes of position are so all important. The greyhound is a fast dog, but he frequently loses the hare by his inability to double as quickly as his quarry; and he would turn out to be an entirely useless animal when pitted against a rat.

Apart from the question of her enormous cost, a cruising gunboat of 450 feet in length would quite as easily succumb to the first lucky shot from an enemy, as would a vessel of half her tonnage, being also cellular and equal in everything excepting speed. She would, indeed, be more easily hit in a vital part, from her inability to turn as rapidly as the smaller ship. The proposition, it appears, is, that nothing under 450 feet, and a corresponding breadth and depth, can be made effectually fast, and invulnerable to the heavy shot which are now thrown by ships of all nations; but this argument requires confirmation by more than one authority.

There are several cases on record in the history of shipbuilding where great speed has been obtained out of very moderate proportions, independent altogether of the twenty-five miles an hour of torpedo boats, and the following is one of these cases taken from a well-known periodical:—"September, 1882. A new steamer, named the Mary Beatrice, built by Messrs. Samuda Brothers, Poplar, for the South-Eastern Railway Company, and engined by Messrs. John Penn & Sons, of Greenwich, has had a journey down the river for the purpose of making her official trial trip. The dimensions of the vessel are as follows:—Length, 255 feet; breadth, 29 feet; depth, 15 feet 6 inches; draught, 8 feet 1 inch; and tonnage, 1,063 burthen. The indicated horse-power of the engines is 2,800. She is distinctly an advance on her sister vessels, Albert Victor and Louise Dagmar, built by the same firm for the South-Eastern Railway Company, two years ago, embodying several valuable improvements in her arrangements for comfort of passengers, but notably in her additional length of five feet, whereby room and displacement have been obtained fcr the introduction of larger boiler and surface condensers, to which additions the excellent results obtained are mainly attributable. The usual four runs were made on the Maplin Sands, giving the extraordinary mean speed of 19.019 knots, or something over 22 miles an hour." Thus we have here a distinct practical refutation of the doctrine that high speed and great length, or great tonnage, must necessarily go together.

We have had it illustrated often enough in the trials of torpedo boats, although there is this difference in regard to them, that they are too small to be made invulnerable to heavy shot.

In this little dilemma, it will be well to remember that neither naval constructors nor eminent shipbuilders are infallible in this direction, as they are not, also, in other matters connected with naval architecture.

Some years ago, when Atlantic competition began to set in rather strongly, it was thought by all builders and constructors in this country—and perhaps by all others in any other country, for nobody said nay!—that great length and narrow beam were everything in the direction of speed. This idea continued to prevail amongst all who had anything to with ships, and who gave expression to their opinions; but about five or six years ago owing to the still greater demand for larger ships, and the restricted depth of water upon certain dock sills and bars of harbors, it was found necessary to increase the beam instead of the depth, and with results which surprised nobody more than the designers themselves. The assertion now made, and apparently just discovered by naval constructors and others, that to obtain great speed, great tonnage is indispensable, may be correct, or it may not, but if it be erroneous it only reminds us of other positive assertions made when iron steamers were in their infancy, and to the effect that, respecting length, anything over 300 feet was encroaching upon the confines of

danger. From 250, however, an advance was quickly made to 300—made by men of action, who had a more powerful belief in their own abilities, than in the opinions —eminent or otherwise—of the men of thought. Results have proved that eminent *savants*, scientists, and professors, have shown themselves to be somewhat blind leaders of the blind. Thus in direct opposition to the learning and doctrine of such professors, practical men have impatiently advanced not only to 300 but to 600, and, without any single instance, as far as is known, of the terrible consequences predicted, being fulfilled. This digression only shows what an amount of groping in the dark there has been by the leaders of thought in this science, and how careful one should be before accepting statements coming even from eminent, or supposed to be eminent, personages, who are not also practical men.

One may easily be within the limits of truth in asserting that, during the last 30 years, few iron steamers have been built which have not been found conspicuously faulty, or capable of great improvement before they had ceased to be new. This applies even with greater force to new ships, not as fighting machines only, but as specimens of naval architecture and seaworthiness, as floating bodies. In this important question of cruisers, the Admiralty should invite three or four of the first shipbuilders of the country to take a contract for their construction—say one each, to four builders—and the equally important matter of length, tonnage, and mode of propulsion, to be settled by a committee, composed of builders, architects, and naval commanders. Perhaps as little of science as possible, with its mystifying technicalities, and as much practical knowledge as possible, might, with profitableness, be imported into their deliberations.

What they would have to consider mainly would be,—can great speed, combined with fighting power, be got out of vessels of this class, having *moderate* instead of excessive dimensions, and if not, why not? To non-scientific people, or to those who are neither naval architects nor shipbuilders, it appears somewhat singular that if twenty-five miles an hour can be got out of torpedo boats, twenty-two miles an hour out of passenger ships of the tonnage of the Mary Beatrice, and twenty-one miles out of 400-footers, like those which traverse the Atlantic between Liverpool and New York, why twenty, at least, cannot be obtained for fighting ships ranging from 1,500 to 1,800 tons.

There are difficulties in the way, no doubt, in combining moderate tonnage and high speed with invulnerability to shot, but in this age of inventions those difficulties should be far from insurmountable.

In the case of the Mary Beatrice we have the tonnage, and we have the speed; thus, if other conditions are satisfactory, it only remains to make her shot proof, or moderately so; to be entirely so would be next to impossible. It would be as unreasonable to expect ships to come out of an action unscathed, as to expect soldiers to leave the battle-field without a wound, and perhaps the sooner we do away with such a forlorn hope, the better it may be for us.

As long as naval wars continue, we shall have ships foundering as of yore, independent of all their armor, and it is just possible that the time is not far away when this contrivance will be given up for ships, as it was for men, years ago.

As long as guns are made which can penetrate armor, it is doubtful wisdom, indeed, to build very costly armored ships; and the nation which possesses a great fleet of small gunboats, carrying heavy guns, with great speed, is a nation to be not lightly esteemed, nor gratuitously insulted. The handiest and perhaps the best ships for this service, should have a length from 260 to 280 feet, a suitable beam, and a draught of water not exceeding 18 feet, with all stores and provisions on board. They should be either twin-screws or double-enders, with a propeller forward as well as aft, unless it should be found that strong engineering difficulties are in the way of such a plan.

Costing something like £70,000 each, we should, at such a rate, have three, against one, of the large type proposed —or, for a fleet, sixty *versus* twenty. Even if their speed did not come up to that of the others by a knot or two, a dozen of such ships would be immeasur-

ably superior, and more effective, than half-a-dozen of the others. Let them sail in couples, with two guns each, of great penetrative power. It must be remembered that their primary business will be neither chasing nor running away, but standing by the fleet under their protection. This fleet will be slow sailing, not reaching above 9 to 9½ knots per hour, and what they will require of their great speed, will be to endeavor to choose the fighting position when the enemy puts in an appearance, and to endeavor to sink him without losing sight of their charge. Thus the necessity for a long run would not be sustained. If their object was to chase everything that hove in sight, we should, sooner or later, find the enemy sending in decoys ahead of the actual fighting ships, to prey upon the defenceless fleet, after the gunboats had gone sailing away in chase.

Had the Americans sailed their ships in fleets, with good strong guards, which they could easily have supplied, the Alabama's career of burning and sinking would have speedily come to an end, for she must either, in such a case, have faced the enemy, or kept out of the way.

If, after the breaking out of war, it were found necessary to employ such large vessels as have been proposed, what could be easier, than for the Admiralty to buy on charter a few Atlantic steamers of great speed, and keep them, under such terms, till they find them unnecessary for that service. But the proposal to saddle the country with a fleet of very costly ships, which will be unserviceable after a war is finished, and which also may never be wanted, is not wise. Such vessels as have been suggested in this paper would not only be effective, but also cheap ships, and would be cheaply maintained, as their crews would be small. They would be admirably adapted for general service during peace, and being light-draughted would find all the harbors of the world open to them.

There are probably few people taking any interest in naval matters who will not remember the exploits of the turret ship Huascar on the West coast of South America, during which she withstood the attack, and returned, with interest, the fire of two powerful British frigates. The Shah was one of the frigates, a ship which cost perhaps half as much again as

the Huascar. It is believed on that coast that she actually beat our two ships; but what is certain is that they not only failed to capture her, but did not even cripple her, as the Admiral intended. It is said that the Admiral would have sunk her, and tried to do so, but after witnessing the method of her fighting, was very glad to give the Huascar a wide berth—he was afraid of being rammed by her. This ship was manned by a scratch crew, and officered, so it is said, by a landsman—who happened to have a pretty strong head, and some smattering of artillery practice.

Now, what would this vessel have done had her crew and officers been British? To have fought her more gallantly would have been impossible; but to have sunk both the Shah and the Amethyst would have been probable.

The Huascar was a brig-rigged vessel of about 200 feet in length, and a tonnage ranging somewhere about twelve hundred. She was protected with about four and a-half inches of armor, and her greatest speed was under eleven knots an hour.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers—March 18th, 1885.—Vice-President G. S. Greene, Jr., in the chair. A paper by Mr. D. J. Whittemore, past President Am. Soc. C. E., on Roofing-Slate was read. The writer used, in 1872, a large amount of Pennsylvania black slate for roofing some freight houses in Chicago. The slate seemed to be of fair quality; but in about six years thereafter, disintegration occurred to such an extent that it became necessary to remove nearly the whole of it.

In 1879 there was required a large amount of slate for shop buildings for the Chicago, Milwaukee and St. Paul Railway, and proposals were received from a number of firms for its supply. Seventeen varieties from as many different quarries were received, and in order to judge of their quality, experiments were made as to the comparative strength when submitted to transverse stress, as to the relative avidity for water as shown by capillary attraction, as to the relative specific gravity, and as to the relative absorption of water when immersed. Two slates of each variety were given to one assistant, and two to another, for the purpose of testing, and they were directed to give for each slate a distinctive number in a scale of seventeen parts. Both assistants agreed in their determinations as to the position of each slate with reference to this scale. The assistants were not acquainted with the price of any of the slate. From the determinations thus made a table was formed by the writer, giving in separate columns the figures determined, and in another column the figures showing the cost

of each variety, and an addition of these figures was considered as fairly showing the relative positions of the different varieties.

The slate offered at the lowest price gave the best aggregate, and was purchased. It was a green slate from Vermont, and has been used upon a large number of buildings, some of which have been in service over five years, and no complaint as to quality has been made. The writer is not prepared to say that the deductions thus made can be in all respects considered a fair relative exponent of quality, but in this instance the decision is apparently justified by experience.

A paper by Mr. William P. Shinn, M. Am. Soc. C. E., was then read in reply to a paper by Edmund Yardley, M. Am. Soc. C. E., in discussion of Mr. Shinn's previous paper on Railway Efficiency. Mr. Yardley suggested that the average movement of cars on foreign roads has, contrary to Mr. Shinn's supposition, increased since 1868, and not diminished. To this Mr. Shinn presents the statement—also prepared by Mr. Yardley—showing that the mileage of cars absent from the Pittsburg, Fort Wayne and Chicago Railroad has increased as stated; yet that, as this company is leased to the Pennsylvania Railroad, it is scarcely fair to include the Pennsylvania as a foreign road, and deducting the cars counted as absent which were upon the Pennsylvania Railroad, the movement on actual foreign roads is diminished and not increased.

Mr. Yardley does not consider that there is any necessity for the new system of blanks proposed in Mr. Shinn's previous paper; to which it is replied that, by the use of that system the change can be accomplished, and the per diem plan put into operation, without any addition to the expense at present incurred in keeping trace of cars, provided that the charge for cars is made sufficiently high to operate as a penalty for their detention.

Mr. Yardley criticised the rate proposed: in answer to which Mr. Shinn referred to his paper published in the Transactions for June, 1883, page 215, where he suggests at the rate five cents per ton per day, reckoned on the capacity as marked on the cars. This method is important on account of the many and widely different tonnage capacities—cars running from five to thirty or thirty-five tons. The suggestion of furnishing cars upon the basis of the annual cost of repairs plus the interest on the cost, divided by the number of working days in the year in order to reach a per diem rate, is lacking in equity.

A letter was quoted, illustrating a marked case of increase in the efficiency of railroads to transport freight occurring in Holyoke, Mass.

Mr. Shinn suggested that if the car accountants of the country would apply the experience which they had to the consideration of the facts in their possession and within their reach there would result an early action. The fact that, during the past year of dullness in all traffic, many railroad companies increased their car equipment in the face of the wholly inadequate movement of cars, needs explanation, and the policy which led to the increase needs correction.

Engineers' Club of Philadelphia—Record of Regular Meeting, March 21st, 1885.—President J. J. de Kinder in the chair.

Mr. E. S. Hutchinson proved geometrically that the well-known Latitude and Departure Method of Calculating Areas was true for all positions of the assumed axis or meridian, provided due regard be had to the signs; from which it was obvious that tedious computations could always be very much shortened by conceiving the meridian to pass through a convenient point within the area, instead of through the most easterly or most westerly point, as given in the books.

Mr. John Wood, Jr., visitor, exhibited handsome working models of cars and tracks equipped with the Curtis & Wood Automatic Car Coupler, and fully explained its workings. Various tests of the model were proposed and successfully made. The inventors claim that this coupler insures absolute safety in coupling and uncoupling cars, a great saving of links and pins, and much time in making up trains; that it can be applied to cars with little or no greater cost than the ordinary draw bar, which, in fact, is used, provided with an automatic steel hook instead of the ordinary pin, which hook has about double the strength of the pin now in general use; that it has all the advantages of the present link and pin coupler as to coupling to the ordinary draw-head or any other having a link or provided with a pin; that it can be applied to any freight car without changing the ordinary draught rigging, bolt or springs; that accuracy of coupling with the links in any possible position is assured by the construction of the bull-head throat, which guides the link when forced backwards to the proper position. When it is desired not to couple, the uncoupling lever (at outside end of end sills) is thrown up, thus placing the crank in an opposite position and making it impossible to couple.

The Secretary presented, by title, for Prof. L. M. Haupt, a revision of his paper upon Scales of Maps, which Prof Haupt has prepared for the Club Reference Book.

The discussion on strengthening the West Main Abutment of Chestnut Street Bridge was continued by the author of the paper, Mr. J. Milton Titlow, Prof. Haupt, Mr. Herring and Mr. Howard Murphy.

Record of Regular Meeting, April 4th, 1885.—President J. J. de Kinder in the chair.

The Secretary presented, for Mr. Jacob H. Yocum, an illustrated description of the recently constructed Water Works at Columbus, Ga., which city has a population of 25,000. The Chattahoochee River was investigated as a source of supply, but on account of the expense of filtering after its frequent freshets, and of pumpage, it was abandoned, and a gravity system adopted. Among the adjacent hills were found a pure and soft water, delivered through the gravel beds, and a gathering ground of twelve square miles which would yield, after allowing 50 per cent. for absorption and evaporation, a daily supply of 15,000,000 gallons. The water is impounded in two successive dams, respectively 130½ and 115½ feet above the center of the city. The upper dam is 266 feet

long by 21 feet high; area 20 acres capacity, 100,000,000 gallons. The lower dam is 250 feet long by 21 feet high; capacity 20,000,000 gallons. The forest ground they occupy was carefully cleared, grubbed, and surface removed to the gravel and clay. The discharge of upper into lower dam is arranged with reference to aeration of the water

The water is conveyed to the city by 18,009 feet of 12 inch main, which divides, at the river, into two 9 inch wrought iron pipes laid under the floor girders of a bridge 800 feet long. These pipes unite in a 12 inch main again upon the city side. It is intended to substitute a submerged main for this double pipe.

The distribution consists of 10, 8, 6 and 4 inch cast iron pipes, fitted with the Cassin double fire-hydrant and the necessary valves.

A 1 inch jet can be thrown 85 feet. At the opening test 7 streams were thrown 75 feet simultaneously. The works provided abundance of pure, good water during a four months' drought, and have generally exceeded expectations. An additional 400,000,000 gallon reservoir, is, however, contemplated, to meet prospective requirements.

The discussion of Strengthening the West Main Abutment of the Chestnut Street Bridge was continued.

Proceedings of the Engineers' Club of St. Louis.—St. Louis, April 1, 1885.—The Club was called to order at 8 p. m. by Vice-President McMath, thirty members and four visitors being present.

Minutes of last meeting were read and approved.

Executive Committee reported that the resignation of Mr. E. Harrison and Mr. Samuel Rockwell had been accepted; also, that C. D. Lamb and J. C. Meredith had forfeited their right to membership by non-payment of initiation fee.

The amendment of Sec. 7, of the bye-laws, proposed at the last meeting was adopted by a unanimous vote.

The following gentlemen were proposed for membership: Mr. O. A. Orrman, St. Louis, Mo., by Messrs. J. A. Ockerson and C. W. Clark; Mr. Walter S. Russel, Detroit, Mich., by Messrs. W. B. Potter and J. A. Ockerson.

Prof. F. E. Nipher read a paper on "The Efficiency of a Pair of Holtz Machines, one acting as generator and the other as motor," which was discussed.

Mr. K. Tully read a paper on "Construction in Wood and Iron."

A general discussion followed.

ENGINEERING NOTES.

The River Humber.—The estuary of the Humber extends from the Trent Falls, at the confluence of the Trent and the Ouse, to Spurn Point, a distance of thirty-eight miles. Receiving the drainage from an area equal to one-fifth of the whole of England, and with tidal conditions probably unsurpassed in any other large river or estuary, it is pre-eminently adapted to carry a great commerce. Unlike the Mersey, with its 8 feet or 9 feet over the bar at low water, it has a deep low-water entrance channel of 60 feet to 70 feet available at all states of the tide. This needs no dredging to keep it open, and so powerful is the scouring action that in spite of a million tons of mud being thrown into the river every year, no ill effects have been observed. Hull is the third port of the kingdom, the annual value of its imports and exports being 40 millions, while those of Liverpool and London are, respectively, 206 and 200 millions. Grimsby's trade is 12 millions, and that of Goole 9 millions.

At Trent Falls the Humber is about one mile wide; at Hull it is about two miles, and at Spurn, some four miles at high water, but there are places of greater width than this. In its lower portions the Humber forms a favorable harbor of refuge for coasting vessels, and it is said that as many as 700 to 800 sail have been anchored in the roadsteads at one time. From Spurn Point up to Hull there is a deep low-water channel, such that the largest ship in the British Navy may navigate it, and anchor off the town at any state of the tide. Of course any hostile ship could do the same, and the Stallingboro' fort and Paul battery opposite would prove but a poor defence in the absence of naval reinforcements. But although vessels can reach Hull at all times, they can only enter the docks when the tide serves. The tide at Hull depends upon many causes besides the attractions of the sun and moon, indeed, in this river the direct action of these bodies is scarcely felt. In the open ocean the crest of the tidal wave travels approximately at the same rate as the earth rolls. But as soon as the narrow seas of the British Isles are entered, the wave loses its distinctive character and travels in the different channels at very reduced and varying rates, and with many peculiarities. This tidal *wave* is essentially different from the tidal *current*. The speed of the wave is that at which the crest or high-water mark travels, but the speed of the tidal current is the actual velocity of the water. The average speed of the tidal wave between Spurn and Hull is twenty-two miles an hour, that is, high water at Hull occurs about an hour after high water at Spurn, and the distance between them is twenty-two miles; but the actual motion of the water never exceeds five miles an hour. The great tidal wave arriving from the Atlantic is broken up on reaching the British Isles, and divides north and south, the velocity being reduced from many hundred miles per hour to thirty, forty, or fifty miles. The part which passes round Scotland helps to form the tidal wave in the North Sea, passing the mouth of the Humber in a S.S.E. direction. There is thus no direct impulse into the estuary as there is into the Severn, and consequently the range of tide is not excessive. At the same tme the Humber meets the sea at a point where the tidal wave has its maximum oscillation, and consequently tides of 20 ft. to 22 ft. are of usual occurrence at Hull. Assuming the rate of flow in the river to be four miles an hour, it is evident that the tidal current cannot travel more than twenty-four miles, and yet the river is tidal up to Naburn Lock in the Ouse, a distance of 80 miles, where high water occurs four hours later than at Spurn, by which time nearly all the water

which entered the estuary from the sea has gone out. This is due to the impulse caused by the ingress of the tidal water in the first twenty-four miles, and this impulse is transmitted forward in the same way that an undulation may be sent along a rope by shaking one end of it.

The Humber is an exceedingly turbid river, and it is a debated point whether this silt comes down with the land water, or is carried in by the sea. The advocates of the latter theory point to the fact that thirty miles of clay cliffs between Bridlington and Spurn are wasting at the rate of 2¼ yards per year, and that the material is borne southward by the tide to the mouth of the river ready to be carried into it, and they strengthen their cause by showing that during a freshet, when the land water is abundant, the river is clearer. Those who favor the opinion that the mud comes from the up-country, show that the cliffs are carried away on the ebb tide, when the detritus goes out to sea, and not to the mouth of the river. The clearness during a freshet they ascribe to the fact that some of the affluents of the Ouse come from a part of the country that does not provide mud, and consequently the increase of total water in the stream may be greater than the increase of suspended alluvial matter. The author is one of those who recognize the turbidity of the Ouse as being due to material carried down by the stream. On account of the continual to-and-fro flow of the tidal water, an object moving with the current only makes five miles net progress towards the sea in a fortnight. It therefore follows that in each five miles length of the river there is concentrated fourteen days' detritus from a water-shed of 10,500 square miles, and this should amply account for its muddiness. Analyses also show that at the turn of high-tide the amount of suspended matter is least, and at low tide is geatest.

It is intended to carry out some improvement works in the Ouse by training the channel, but these, the author believes, would have no effect upon the amount of tidal water in the river, and would not lessen the scour below Hull. The proposed works in the Humber, he fears, would not be so harmless. The present deep channels in front of Hull are dependent, he holds, for their very existence upon the maintenance of the existing natural conditions in the Upper Humber, and particularly of the south channel past Chalders Ness. Now the conservancy lines would shut up the present channels and substitute a new one through the sand banks, to serve both for ebb and flood. This trained channel would terminate a good way above Hull, and below it the water would follow its own course, probably with the result of altering the existing condition of affairs opposite Hull. Fortunately there is no immediate prospect of the works being commenced.

Goole and Grimsby are both increasing their trade, while Hull remains nearly stationary. But there is hope that better times will come soon. There are great works in progress by which the tidal facilities of the Humber will be fully utilized. Ships drawing under 18 feet of water will be able to enter the new Alexandra Dock at low water, so that a majority of the vessels frequenting the port will be able to pass in without any delay.—Abstract of a paper read before the Hull Literary and Philosophical Society, by A. C. Hurtzig.

IRON AND STEEL NOTES.

PRODUCTION OF BARS, WIRE, &c., DIRECT FROM FLUID STEEL.—A patent is published in Germany (*Zeitschrift des Vereins deutscher Ingenieure*, January 10th, with an illustration) taken out by C. M. Pielsticker and C. G. Muller, for a process and apparatus the object of which is to produce bars, wire, &c., direct from fluid steel by pressing it out through dies in a manner somewhat similar to the production of rod and pipe from lead. An iron vessel is lined with refractory material and provided with a manhole at one side and a cover at the top, both capable of being securely closed. At the bottom at one side, opposite the manhole, there is attached by bolts a cast-iron outlet pipe. Through this passes a steel tube with a water space around it exactly like a water tuyere, so that the inner tube can be kept constantly cooled by circulation of water in the space. The end of the steel tube, on the inside of the container, is constructed to hold a fireclay end, or nozzle, where the liquid steel comes in contact with it. The container being already hot, a steel bar is passed through the steel tube so as to project through the fireclay nozzle into the container. Liquid steel is then run in, the cover secured, and the container connected by means of a tube at the top with a vessel containing liquid carbon dioxide. The steel bar is then withdrawn through the tube or die, and the pressure of the carbon dioxide forces the fluid steel to follow it. so that it is drawn out as a continuous rod, of the dimensions and shape of the die, to be passed at once still red hot, through rolls to finish it as required.

A NEW PROCESS OF STEEL MAKING.—A patent has recently been granted to Mr. W. A. Otto Wuth, of Pittsburgh. Pennsylvania, for a new process of producing steel from wrought iron with plumbago, the steel produced being, it is stated, of a high grade and practically free from sulphur and phosphorus, while containing a definite percentage of carbon. The process consists in making the steel from wrought iron that is practically free from phosphorus, sulphur and carbon, by melting it on an open hearth in contact with a form of carbon which will not oxidize at the heat necessary to smelt the iron, but which will unite with the iron at that heat. Any wrought iron which is sufficiently low in phosphorus may be used as the basis of this process, though muck bar made according to a previous patent of Mr. Wuth is preferred as being especially pure. The decarburized iron of the requisite purity is first cut into pieces of convenient length and placed in the hearth of the furnace in layers piled one above the other. Between each layer of iron is spread a thin stratum of plumbago, preferably in a pulverized condition, although it may be used in lumps. For this purpose the inventer employs the plumbago of commerce, but of good quality, containing

about 98 per cent. of carbon and 2 per cent. of silica, with a trace of iron. The hearth of the furnace is charged with alternate layers of wrought iron and plumbago, until it is sufficiently full, the relative thickness of the layers depending upon the amount of carbon which it is desired that the resulting steel should contain. If the iron were absolutely free from slag, and the plumbago were also absolutely pure carbon, then the proportions of the charge would be the same as the proportions of iron and carbon in the desired steel product; but as there is always more slag in muck bar (not, however, exceeding 1 per cent.), and some silica in the plumbago, the proportions of which can be ascertained before the furnace is charged, the necessary allowance for these elements will have to be made, and the percentage of carbon can be regulated with great exactness, so as to produce steel, it is claimed, very nearly approaching to an ideal steel. The furnace being thus charged with muck bar and plumbago, the charge is melted in the usual way, and the operation further carried on as in the well-known open-hearth process. Before the melted metal is withdrawn from the furnace a small and definite amount of speigeleisen or ferromanganese is added. By this process Mr. Wuth says he has made several charges of steel of 12 tons each containing as low as 100th of one per cent. of phosphorus, and that he will be able to produce any kind of fine steel that may be desired.

RAILWAY NOTES.

Some rapid tunnel driving has lately been done on the Mersey Tunnel Railway, by Colonel Beaumont's boring machine. The distance accomplished last week, through the red sandstone under the Mersey, was 87 yards, which is the "fastest on record." The heading now being driven, and which is nearly completed, has a total length of about 950 yards; and this, as well as the previous heading of about 700 yards in length, are intended for effecting the ventilation of the main tunnel. The total distance driven by Colonel Beaumont's machine—which cuts a circular heading rather over 7 feet in diameter—in connection with the Mersey Tunnel, is about 2,250 yards, which includes the first operation, viz., the boring of the drainage heading.

The Midland Railway from Woodlesford, near Leeds, to Barrow-in-Furness was obstructed throughout Sunday. A correspondent says:—Messrs. Cammell and Co., of Sheffield, recently completed an immense steel propeller for a steamship, which is being constructed at Belfast. The blades of the propeller—one of the largest yet made—were so wide that they overlapped the opposite line of rails to that on which the propeller was being transported. On Sunday arrangements were made for the conveyance of the propeller from Woodlesford to Skipton, and in order to effect this the passenger trains along the route were shunted and blocked to allow the special train to pass. At the stations and junctions the propeller excited great interest. The train journeyed to Skipton safely, where it was intended that it should remain until next Sunday, but it afterwards proceeded to Carnforth, and subsequently to Barrow, where it arrived on Sunday night.

ORDNANCE AND NAVAL.

The Next Big Gun.—According to the *Morning Post*, preparations are being made at Woolwich Arsenal for the proof trials of an enormous gun which is now in process of construction at Elswick, and will be delivered a few months hence. It will weigh 110 tons and have a carriage of 90 tons, the total weight of 200 tons being considerably in excess of previous undertakings. The gun will be a breech-loader and have a bore of 16 inches. Its length will be 48 feet 8 inches, but its extreme diameter at the breech will be only 5 feet 6 inches, and it will have a very elongated chase or barrel tapering down to 28 inches, with a slight swelling at the muzzle. The carriage will run on the ordinary railway gauge, but the line leading to the proof-butts will have to be partly relaid, and the bridge over the canal will probably be strengthened. After the gun has been proved at Woolwich it will be taken to Shoeburyness for the purpose of trying its range and accuracy, and it is at present a question whether the gun barge Magog can be altered to receive it, or whether it will be necessary to provide another vessel. Three guns of this description are to be made, and they are intended for the Royal Navy.

Russian Heavy Ordnance.—Very large orders for naval artillery have recently been given to the Oboukhoff Steel works, near St. Petersburg, which is virtually a government establishment, and under the control of officers of the Russian war department. The orders given by the minister of marine comprise two 12-inch guns for the ironclad Katrina II., now in course of construction at Nicolaieff; four 11-inch guns for turret frigates, nine 9-inch guns for monitors, seventeen 6-inch guns for the corvettes Rynda, Vityaz, and Bobr; six 9-pounders for the Bobr, eight 4-pounders for the Rynda and Vityaz, and two 2½-inch field pieces for other vessels. There are also large requisitions for shot and shell. All the steel for the guns is of home production, and made at the government works of Briansk, where also is produced the steel for the new ironclads Nicolaieff and Sebastopol. Steel manufacture is being very much encouraged at the present time in Russia. The small-arms factory at Sestroretsk is entirely supplied with steel made at Zlataoust, which also furnishes the 12-pounder guns used by the Russian artillery. Prince Belozensky has established large steel-works at Katel-Ivanova, and has obtained from the government an order for steel rails which will keep his works employed for several years to come.—*Iron*.

Floating House for Torpedo Experiments.—A floating house for torpedo trials is being built at the Royal Arsenal, Woolwich, by the Laboratory Department. It is intended for use at Chatham Dockyard, where the Lords of the Admiralty have placed one

of the largest basins in the kingdom at the disposal of the War Department for the purposes of these experiments, stipulating only that no building is to be erected, and that possession must be given up at any time without notice. For these reasons a floating house has become necessary. It will contain the engine for charging the torpedoes with compressed air, the tube for firing them under water, and the other apparatus requisite for a torpedo range. For some time past the canal at the Royal Arsenal, Woolwich, which was the only piece of still water available for testing and proving the newly constructed torpedoes, has been found inadequate as regards length and depth, and attempts have been made to try the torpedoes in the Thames, but not with any great success.

BOOK NOTICES.

Publications Received.

Bulletins of the United States Geological Survey:

No. 2.—Gold and Silver Conversion Tables; a pamphlet of 8 pages.

No. 3.—Fossil Faunas of the Upper Devonian; pamphlet 86 pp.

No. 4.—On Mesozoic Fossils; pamphlet 37 pp.

No. 5.—A Dictionary of Altitudes in the United States; pamphlet 325 pp.

No. 6.—Elevations in the Dominion of Canada; pamphlet 42 pp. Washington: Government Printing Office.

Transactions of American Society of Civil Engineers. January.

Bulletin of the Philosophical Society of Washington, Vol. VII.

From Cassell & Co.: The Quiver, The Family Magazine, and The Magazine of Art; the latter embellished with five full-page illustrations.

Selected Papers of the Institution of Civil Engineers:

The General Theory of Thermo-Dynamics. By Professor Osborne Reynolds, F. R. S.

No. 1,992.—The Art of Making Paper by the Machine. By James William Wyatt, Assoc. M. Inst. C. E.

No. 1,999.—On Hauling out and Launching Vessels Sideways. By Murray Jackson.

No. 2,016.—Pumping Machinery for Draining Marshes. By Thomas Richard Guppy, M. Inst. C. E.

No. 2,022.—Removal of Buddonness Lighthouse. By David Cunningham, M. Inst. C. E.

No. 2,023.—Notes on Compressed Air. By John Kraft, M. Inst. C. E.

No. 2,025.—The Burnham Sewage Outfall Works. By Alfred Barton Brady, Asso. M. I. C. E.

No. 2,029.—Electric Lighting for Steamships. By Andrew Jamieson, F. R. S. E.

No. 2,036.—The Barmouth Waterworks. By Thomas Roberts, Assoc. M. Inst. C. E.

The Civil Engineer's Pocket-Book. By John C. Trautwine, C. E. Revised, corrected and enlarged by John C. Trautwine, Jr., C. E. New York: John Wiley & Sons.

A book so widely known as this needs no comment, certainly no praise. Many engineers regard it as the only necessary cyclopedia of useful knowledge, and many of the profession who began to use the first edition fourteen years ago are probably now suffering from impaired or lost eyesight from too assiduously laboring over the painfully small type which was necessarily employed in condensing an encyclopedia into a pocket-book.

The revisions and corrections in the new edition are numerous. It requires five pages of the preface to specify them. Many omitted paragraphs have not been replaced, so that a large number of scars are apparent when turning over the leaves.

The additions to the work are an undoubted improvement.

Annual Report of the State Geologist of New Jersey for the Year 1884. By Geo. H. Cook, LL. D. Trenton: State Printer.

Superintendent Cook's Reports are always valuable to the people of the State at least, and always contain something of interest for students of geology everywhere.

Considerable space is given to the description of the columnar trap rocks at Orange, in which there was a widespread popular interest manifested last Fall.

The chapters on the Drainage of the Great Meadows, and on the Purifying of Water, possess a far more than local interest.

The maps and other illustrations are excellent.

How to Drain a House. By Geo. E. Waring, Jr., M. Inst. C. E. New York: Henry Holt & Co.

This little book of 222 pages presents the subject of House Drainage in 26 chapters. It is a compact manual for the householder. Many who would refuse to read the more complete treatises on this important subject will probably be attracted by the exceedingly concise form in which the leading principles are presented.

The few illustrations are pretty good.

Photography for Amateurs; a Non-technical Manual for the Use of All. By T. C. Hepworth. Cassell & Co. 1884.

This little volume contains more information valuable to the amateur than many of the larger and more pretentious publications. The chapter on portraiture is valuable as giving directions for taking properly-lighted pictures in ordinary dwelling rooms—a feat which few amateurs accomplish successfully. Pages 29 and 30, on lenses, are worthy the perusal of anyone intending to buy an outfit, and may save the purchaser many dollars. On p. 72 a negative washing-box is given, which most workers in photography will find unpractical. In the chapter on development an improvement could be made by using the same terms when speaking of fluids, and not employing "oz." in one place and "pints" in another. Chapter 11 contains some good hints on mounting prints, including the making of starch paste and mounting with gelatine in alcohol, a method which will not cockle the mounts. A good index is another recommendation for this little volume.

TOPOGRAPHICAL SURVEYING. VAN NOSTRAND'S Science Series, No. 72. Price 50 cents.

This work contains four papers on the following topics, viz.: "Topographical Surveying," by Geo. J. Specht, C. E.; "New Methods in Topographical Surveying," by Professor A. S. Hardy; "Geometry of Position applied to Surveying," by John B. McMaster, C. E.; "Coordinate Surveying," by Henry F. Walling, C. E. The Art of Surveying, now but in its infancy in this country, and with surroundings adapted to afford it a huge growth, finds in these short essays elements tending to promote a rapid and healthy development, a boon which it cannot be said to have enjoyed at the hands of the American surveyor, because, perhaps, necessity has not seemed to demand it. Now that potent reasons for determining the position and configuration of our lands are arising, it is a source of satisfaction to see that scientific literati are giving this important subject the attention it deserves.

The paper on Topographical Surveying treats on the use of the stadia. The essential details of field practice and the manner of keeping and plotting field notes are given in a very complete manner. The paper closes with a short chapter on the use of the slide rule to facilitate the computation of the field notes, the usefulness of which as a labor-saving machine is apparent.

Mr. Hardy's description of photographic surveying is very interesting, and undoubtedly will inform a large body of our surveyors, for the first time, of the possibility of locating, by intersections, an almost infinite number of points by but two observations. The office work consists in finding the intersection of the two cones determined in the field, a simple geometrical operation.

The paper on the Geometry of Position applied to surveying is very instructive, and every surveyor who aims to become accomplished in field work should number this method among his resources.

The paper on Co-ordinate Surveying presents a plan for conducting land surveys by referring them to stations, the co-ordinates of which have been determined by a complete system of trigonometrical surveys, with references to some established point. The objects to be attained are an accurate determination of the positions of corners and directions of boundaries, and the avoiding in a large measure of land litigation. A review of what has been accomplished in this direction, the faults of the present method, or rather lack of method, of conducting our surveys, is given in a series of short chapters as an introduction to the writer's plan. The captions of two of these chapters speak volumes to those who have had opportunities of becoming familiar with how our land surveys are made. They are "Faulty descriptions in land conveyances," and "Imperfections of the surveyor's compass." The method of procedure in carrying on the different operations relating to this plan and two examples of its application are presented. The manner in which the subject is handled is very clear, and the demonstration of the principles require no more mathematical knowledge than what every surveyor should be expected and required to possess. Apropos of this question it may only be necessary to refer incidentally to the circumstance, quite widely known, we presume, that one of the results of the trigonometrical survey of New York State was to establish the fact that most remarkable errors existed in the location of towns and boundaries of counties as laid down on the then current maps. It is to be hoped that this paper will be extensively read by those interested in this subject, and hasten the millennium of bringing order out of the present chaos in our land surveys.—*Abstract of Review in Mechanical Engineer.*

MISCELLANEOUS.

THE "NOVELTIES" EXHIBITION.—The announcement of the Managers of the Franklin Institute of their intention to hold another exhibition in Philadelphia, during the autumn of the present year, will be news of interest to all our readers. The Electrical Exhibition of last year was an unqualified success in every way, and was most creditable to this useful and venerable institution, and to all who were concerned in it.

The managers have decided this year to hold an exhibition of a more popular character. It will be known as the "Novelties" Exhibition, and will be devoted, as its title indicates, to the display of such recent inventions, improvements and discoveries in the arts and manufactures as may be deemed worthy of admission.

It will afford our inventors and manufacturers an excellent opportunity of bringing to public notice their best and most recent achievements. That the character of the forthcoming event will undoubtedly be maintained up to the highest standard of excellence, goes without saying, and our people may look forward to the "Novelties" as a rare source of interest and instruction.

From the announcement of the managers, the "Novelties" Exhibition will be opened on Tuesday, September 15th, and will be closed on Saturday, October 31st.

CEMENTS FOR SPECIAL PURPOSES.—The value of a cement is, first, that it should become a strongly cohering medium between the substances joined; and, second, that it should withstand the action of heat, or any solvent action of water or acids. Cement often fails in regard to the last consideration. For waterproof uses several mixtures are recommended, and the following may be mentioned: One is to mix white lead, red lead, and boiled oil, together with good size, to the consistency of putty. Another is powdered resin, 1 oz., dissolved in 10 oz. of strong ammonia; gelatine, 5 parts, solution of acid chromate of lime, 1 part. Exposing the article to sunlight is useful for some purposes. A waterproof paste cement is said to be made by adding to hot starch paste half its weight of turpentine and a small piece of alum. As a cement lining for cisterns, powdered brick 2, quicklime 2, wood ashes 2, made into a paste, with boiled oil, is recommended. The following are cements for steam and water joints: Ground litharge, 10 lbs., plaster of

Paris, 4 lbs., yellow ochre, ½ lb., red lead 2 lbs., hemp, cut into ½-in lengths, ½ oz., mixed with boiled linseed oil to the consistency of putty. White lead, 10 parts, black oxide of manganese, 3, litharge, 1; mixed with boiled linseed oil. A cement for joints to resist great heat is made thus: Asbestos powder, made into a thick paste, with liquid silicate of soda. For coating acid troughs, a mixture of 1 part pitch, 1 part resin, and 1 part plaster of Paris is melted, and is said to be a good cement coating.

Correspondents frequently ask for a good cement for fixing iron bars into stone in lieu of lead, and nothing better is known than a compound of equal parts of sulphur and pitch. A good cement for stoves and ranges is made of fire-clay with a solution of silicate of soda. A glue to resist damp can be prepared with boiled linseed oil and ordinary glue; or by melting 1 lb. of glue in two quarts of skimmed milk, shellac, 4 oz., borax, 1 oz., boiled in a little water, and concentrated by heat to paste. A cement to resist white heat may be usefully mentioned here. Pulverized clay, 4 parts; plumbago, 2; iron filing, free from oxide, 2; peroxide of manganese, 1; borax, ½; seasalt, ½; mix with water to thick paste, use immediately, and heat gradually to a nearly white heat, Many of the cements used which are exposed to great heat fail from the expansion of one or more ingredients in them, and an unequal stress is produced; or the two substances united have unequal rates of expansibility or contractility; the chemical or galvanic action is important. The whole subject of cements has not received the attention it deserves from practical men. Only Portland cement has received anything like scientific notice, and a few experiments upon waterproof, heat-resisting, and other cements would show which cements are the best to use under certain circumstances.

Assyrian Science.—Last Wednesday afternoon Mr. W. St. Chad Boscawen delivered one of a series of weekly lectures at the British Museum on "Assyria;" the attendance was large. In the course of his address the speaker drew attention to the amount of scientific knowledge possessed by the dwellers in the city of Nimrod, mentioned in the opening chapters of the Book of Genesis, and he stated that the present knowledge of the manners and customs of the people who dwelt in those cities during a period ranging from 2000 to 4000 years B. C. is considerable, even to the details of the lives of certain private persons down to their very family squabbles. As to their scientific knowledge, they had made some progress in mensuration, and they laid down plans of buildings "to scale;" their standard of measurement was the Babylonian cubit, which they subdivided into a number of equal sections. They had a table of square and cube roots, they calculated by the scale of 60, and they divided the circle into 360 parts. In the earlier periods their sculpture, in which they were advanced, was more true to nature than in the later periods, when it became conventional. He should like to know what the British workman would think of the material they carved, for it was porphyry so hard that it would turn the edges of the best chisels; in all probability they used the diamond drill, as the Egyptians certainly did, and spent unlimited time over their work out of fealty to their king or reverence for their God. They worked bronze with the hammer, and they cast statues with the same alloy; one of their earliest hymns speaks of a good man shining like brass cast out of a crucible. In music, they had the harp, pipes, and cymbal. They knew the colors assumed by light; Mars was described to them as a red orb, and Mercury as a blue one; in one of their hymns a scribe speaks of the sky being as blue as the sea. They had words for the compound as well as the primary colors, including names for reddish-brown, purple and gray. Blue and purple were connected in their minds with the idea of darkness, and the same appears to have been the case with the ancient Greeks; he was not quite sure that such was the case with the latter people. The Euphrates Valley, in fact, was the cradle of civilization; upon its banks were city kingdoms, each city having its own king, one of whose duties it was to sit at the gate of the city to give judgment. Old commentators had found various sites for the four cities of Nimrod, varying in position from Ireland to the banks of the Ganges; but modern explorations had solved the mystery. The true site of Babylon was the only one which had been preserved by tradition; the literal interpretation of the word "Babylon" was "The Gate of God."

A paper on some irregularities in the values of the mean density of the earth, as determined by Baily, was read on the 26th ult. by Mr. W. M. Hicks. The author showed that the numbers obtained by Baily for the mean density of the earth depended on the temperature of the air at which the different observations were made; and he exhibited a table showing that as the temperature increased from 40 deg. Fah. to 60 deg. Fah. the deduced mean density fell continuously from 5.734 to 5.582. He considered several possible causes of error, but showed that they were either inadequate to explain the irregularities, or tended in the opposite direction. The only further suggestion that occurred to him was that Baily's personal equation was a function of the temperature, leading him, as his temperature rose, to estimate distances more liberally.

At a recent meeting of the Cambridge Philosophical Society, a paper was read by Mr. A. H. Leahy on the pulsation of spheres in an elastic medium. The problem of two pulsating spheres in an incompressible fluid has been discussed by several writers. The author considers the analogous problem in the case in which the medium surrounding the spheres has the properties of an elastic solid. He finds that the most important term in the expression of the law of force between the two spheres varies inversely as the square of the distance between them. This force will be an attraction if the spheres be in unlike phases, a repulsion if they be in like phases at any instant. The next term in the expression varies inversely as the cube of the distance between the two spheres, and is always a repulsion.

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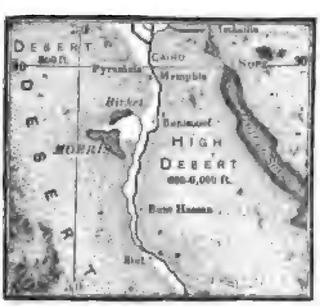
NO. CXVIII.—JUNE, 1885.—VOL. XXXII.

THE PYRAMID BUILDERS.

By COPE WHITEHOUSE,

Written for Van Nostrand's Engineering Magazine.

The East and West have always been divided and opposed. The Pyrenees and Alps, or the Mediterranean, or the rampart of the Lebanon, with its tremendous counterscarp and fosse, sunk 1,300 feet below the surface of the earth, have, each in turn, been a frontier. The East never effected a lodgment in Central Europe. The West never penetrated into Arabia. The opposition is not limited to language,

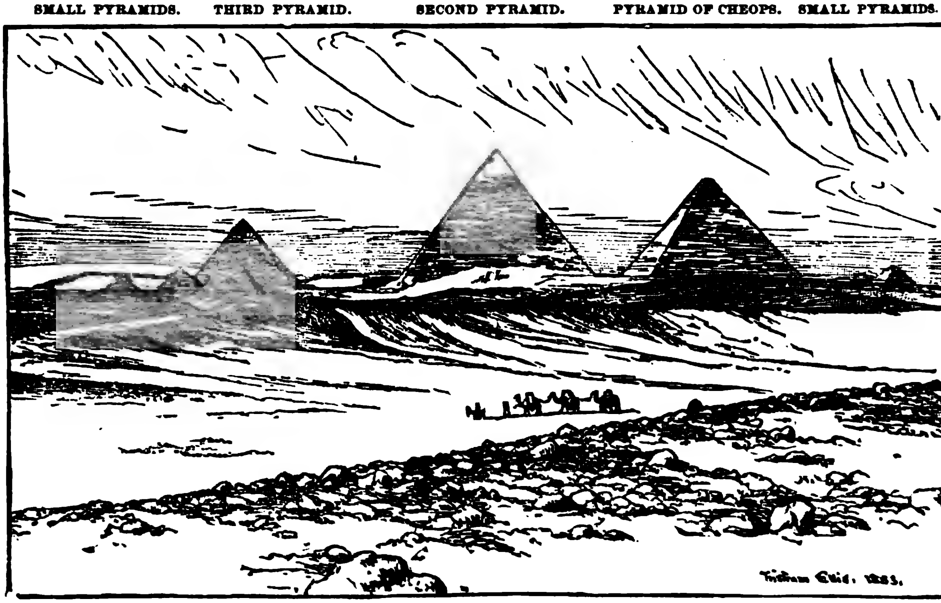


religion and morals. It extends to art, architecture and engineering science. It is due to recondite causes and remote events, as well as to existing geological and geographical conditions. The characteristic of oriental life is its common sense. Men live in far closer and more friendly relation to nature than in the artificial and mechanical civilization of the West. There is less disposition to domineer over the lower forces. The power of the earthquake and the majesty of the volcano are recognized and not defied. Thus the types of religion and architecture stamped by Fusi-yama on the Japanese are much more legible than those which Ætna imposes upon the Sicilians. A rapid change of sentiment is taking place among ourselves. The two ends of Manhattan Island are laid out in conformity with the antecedent work of nature. The central portion exhibits reckless prodigality of labor with the worst conceivable results. It is a favorable sign that the period of rude self-assertion is drawing to a close. The sun and the wind are not yet worshiped, but there are statutes passed every day to secure due respect for light and air from an ignorant, greedy and gainsaying people. If the gun is still in the hands of any one, whose inferiority to the beast or bird whose life he seeks requires its elaborate mechanism, it is muzzled for a large part of the year. It kills at a few more yards than the blow-pipe of the South American, but it frightens the game to a proportionately greater distance. The discriminating bow of Æneas supplied his companions with as many selected deer as he required before the does and fawns were alarmed. An hour later

he was in the midst of Carthage, with all the turmoil of a busy manufacturing and commercial metropolis. The ancient Egyptians like the modern Chinese could snare wild fowl in their gardens. Progress in the transmission of intelligence and the exchange of products can no longer conceal the fact that we have overlooked true sources of wealth and comfort. We are impoverishing and disfiguring with frightful rapidity the domain we possess.

Although the nature-worship of the East is to some extent a superstition, yet it is also a religion. It formulates rules by which men are compelled to recognize that they are bound hand and foot to this planet. The definite and measurable forces of the world can be temporarily disturbed by human agency. Its more majestic powers could not be arrested for an instant by the sum total of human energy since the world began. Even where change has been effected, atoms unweariedly seek their normal state. We vex the sunbeam with an aniline dye. Straightway it seeks to restore the particles to freedom and the molecules respond by insidious movements. Steel is the favorite instrument of modern violence. The tool converts itself into crystals and snaps like a pipe stem. This is one cause of the feverish activity of modern society. We have no time to lose. The life of engineering works is, as it were, but a span long. The engine must hasten to do its work ere it die. It is not wholly the fault of the material employed. There is a wooden building at Nara in Japan, in which the Mikado's art treasures have been preserved for twelve hundred years. It has had four roofs. The third lasted five hundred years. The present tiles have been in place since 1730. There was a pyramid of sun-dried bricks on the banks of the Nile, whose age was unknown when Herodotus visited it and said that Asychis was rumored to have made it from the alluvial deposit in a reservoir. It stands there still after twenty-two hundred years. There is a dome of flat stones, corbelled out until the hemisphere was complete, beneath earth, bushes, and even trees, which is as perfect now as when the women of Argos pointed the finger of jealous scorn at the beautiful Helen. In wood, earth and stone, Asia, Africa and Europe thus show how architects may conform to the laws of earthquakes, climate and society, and thus leave enduring monuments to their tact. Comparative engineering is a new

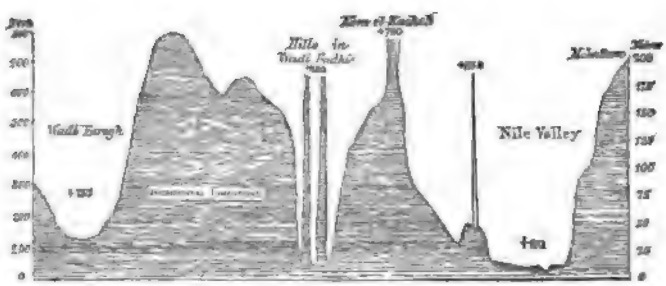
SMALL PYRAMIDS. THIRD PYRAMID. SECOND PYRAMID. PYRAMID OF CHEOPS. SMALL PYRAMIDS.



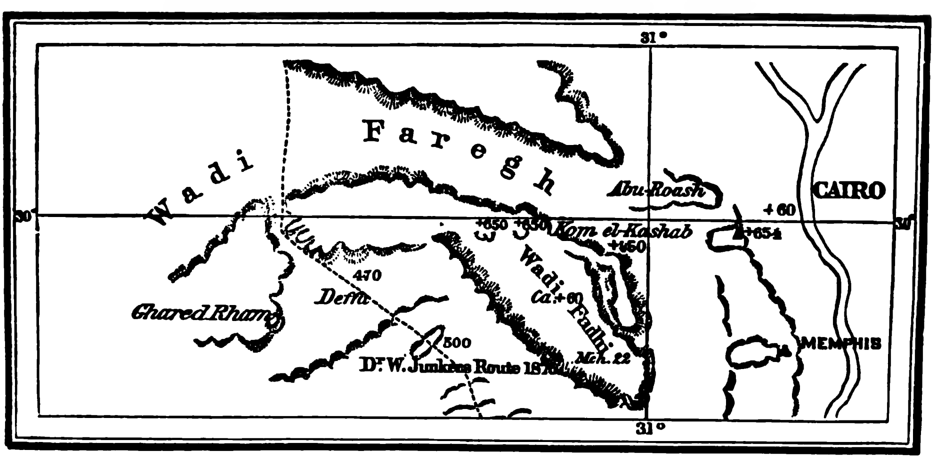
The Pyramids and Temples of Gizeh. By W. M. Flinders Petrie.

THE PYRAMID-HILL OF GIZEH FROM THE SOUTH.

study. The old masters teach one to suspend judgment. Prejudice speedily yields to admiration. Weight of material may cost less than more elaborate construction. Dry earth may make a better wall to keep out heat than thinner layers of burnt clay. Megalithic structures show no tool marks. But, perhaps, skill in cleavage saved the labor which would have left the traces of the instrument. This economy of force goes much farther. fied mass of soft stone, offered the cool depths of a grotto, and the deep shadows of its exterior ornament as a welcome relief to the glare and heat of a torrid sun. An artificial stream bursts from a mountain side in the heart of a town. The labor of a few days sufficed to cover the portal of the fountain with well-known signs telling to child and man that the anger of the God of Health dwelling in the shades of the distant grove would



SECTION OF FIFTY MILES ON LAT. 30°.



The architects and engineers of the ancient world strove to effect only such modifications of nature as would accomplish their purpose. The purpose was in turn defined by the material. But by material must be understood more than the granite or limestone, the clay or earth, which lay most convenient to the builders' hand. These builders also considered the presence of a ravine or hill as a gift of nature not to be lightly disregarded or destroyed. Thus in a warm dry climate the rock-hewn temple, carved in a stratismite with deadly weapon the offender against purity. A monolithic house at Amrit is one of the most interesting remains of Northern Syria. The material was cut away in such a fashion that only thin walls and partitions were left. This house was a chateau of important dimensions. The principal façade is one hundred feet long. The edifice was nearly square. The walls still retain the respectable height of twenty feet. Their thickness of thirty-two inches does not exceed that of the basement walls of a

modern structure. Twenty-five centuries have elapsed since the Phœnician merchant determined to exhibit the wealth acquired, perhaps, in the tin trade with Cornwall, from a profitable but nauseous dye-house in Tyre, or the pearl fisheries of Ceylon. He built his house in the quarry, and moved the least amount of stone the shortest distance. He used the spaces separated by the live rock as chambers, and plastered without fear of settlement upon the natural walls. The interior was divided into at least three chambers by partitions left in the same way. The external wall to the north was artificial; its lowest courses are still to be found hidden in the soil, the south wall was partly rock, partly masonry. In the island to the north of Saïda the rock still bears traces of similar works. The lower parts of walls are shaped as they stand. We find them pierced in many parts with niches and rectangular or round-headed doorways. In some instances even partition walls are rock-hewn. Claudius Iolaus said, sixteen centuries ago: "When the Phœnicians began to settle on the rocky shores to which they were attracted for the sake of the purple dye, they built houses for themselves and surrounded them with trenches; as they cut the rock for this latter purpose, they used the material removed for the walls of their towns and so protected their ports and jetties." Such examples as these are to be analyzed, not imitated. They have, however, a direct bearing upon the problem of those pyramids at Gizeh, upon which such an extraordinary amount of thought has within a few years been concentrated and wasted.

MIDDLE COURSES OF THE PYRAMID OF CHEOPS.

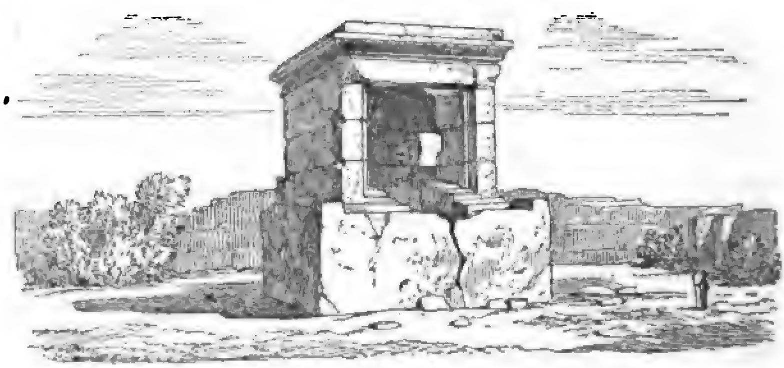
The gorge of the Nile Valley is parallel with the Red Sea. Recent events have shown that the Upper Nile is more readily reached from Suakim than from Cairo. The stream of commerce may be taken in the flank at many points from Massowah to Suez. The British troops, under Abercromby, fought the battle of Alexandria on March 13th, 1801. They entered Egypt at Thebes. The same nation is repeating the same tactics at a more southerly point. It aims to effect a junction with troops encamped near Meraweh. The name Meraweh, or town he built to her memory still bears her name. Whether his campaign was undertaken to crush a Mahdi, or to confront a maritime power with its seat in the Bahrein Islands of the Persian Gulf, is obscure. But it is curious that both at Meroe and at Cairo there are groups of pyramids. Nowhere else, from the Cape of Good Hope to the Baltic are there similar structures. Two garrisons of foreign troops, separated by a thousand miles, are now watched by these mysterious sentinels. At Meroe and the sixth cataract, as at Mœris and the great gate of

SECOND PYRAMID. NATURAL ROCK.

STRATIFICATION OF THE HILL OF GIZEH.

Meroe, was known to the cartographers who provided Ælius Aristides with a traveling library. It has been the current appellation of this place for more than three thousand years. Before the remotest date assignable to Buddha or the Trojan War, the same musical sounds expressed admiration for the "lovely" capital of Nubia. According to an Alexandrian tale, which was very ancient sixteen hundred years ago, it was the headquarters of Moses. His adoptive mother, Mary, in her devotion, had accompanied him to the Soudan. She died. The the Delta, these square masses with triangular sides continue to present a difficult, if not insoluble, problem. Between these points there are many natural hills which have been used for architectural purposes. Two temples in the gritstone rock known as Aboo-Simbel, or Ipsambool, are generally considered, next to Thebes and the Pyramids, as the most interesting remains of Africa. How little labor accomplished this result! An insignificant spur juts out into the Nile. A portion of the rock has been cut away to form a façade about a hundred feet in

width and as many in height. No edifice better deserves the name of great, in its best sense of the ratio of means to purpose. The amount of material removed is less than that excavated for a common dwelling-house in New York. The stately cornice with its freize, and the four gigantic statues sixty-six feet high, did not cost as much as the brown-stone veneering of an equal superficies. These twenty-two cynocephali, the large statue of the sun-god Ra and the four collossi could be repeated at the price of a wooden spire for a village church. Yet, a steamer is kept upon the Upper Nile for the sole purpose of carrying persons of the highest education and most refined taste to see the summer's amusement of some engineer corps, who spent less in perpetuating the fame and features of their monarch and his queen for thirty centuries, than any single group of travelers in getting to the spot. The façade of Ipsambool was, of course, begun at the top. The fragments of loose stone were picked off by a handful of men. Then a trench was cut across the steep face to mark the upper member. Two deep grooves were scored at either side. The Egyptian Buonarotti sat in the shade at a convenient distance, and directed with rod and voice the movements of those who split off and pried away the layers of gritstone until they had blocked out a rough draught of his idea. The material removed was as available for the purposes to which it was applied as if the quarry had been left as a scar instead of a temple. The unfinished façades of Petra prove, if evidence be required, that these Titans in design always worked downwards, not upwards. The problem would have been far more complex if the garrison of "the hill of the pure waters" had labored for years instead of weeks, on four sides instead of one, and reduced the intervening spaces to the thin walls of the Phœnician dwelling. They might have pierced the mass like the ivory ball of the Chinese, left monolithic shrines detached from floor and roof in chambers, accessible only by openings of smaller size. The Egyptian tabernacles of a single block excited the admiration of Herodotus. The same effect was obtained at Amrit, in Northern Phœnicia, without any extraordinary effort. A large quadrangle, 192 feet by 160, has been cut in the living rock. In the center, a block has been left some twenty feet square and 10 feet high. Upon this cubical mass, which is part of the actual floor of the temple, has been built a small tabernacle. It is composed of four stones. Three of them are interposed between the mass of living rock which forms the foundation, and the roof which is a monolith. This upper stone seems to have been supported originally by metal columns, forming a kind of portico. Placed one beside the other on the apex of a mass of rocks two monuments, known as El-Meghazil, dominate the surrounding country and recall the famous colossi in the plain of Thebes. "One of these monuments," says M. Renan, "is a masterpiece of proportion, elegance and majesty." They rise above a large enclosure hollowed out of the rock. Lime-

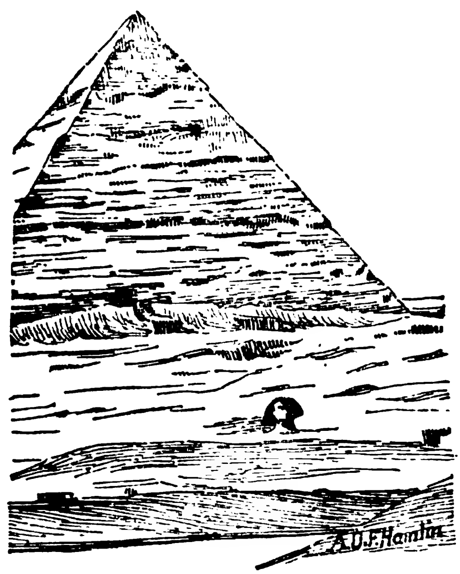


THE HISTORY OF ART IN PHŒNICIA. MM. PERROT AND CHIPIEZ.

stone differs in its successive strata. Suppose that some blocks of exceptional quality had been reserved for columns, sarcophagi, or shrines. Apparently brought from a distant quarry, they might have been originally part of the temple-hill which still enclosed them.*

When, then, we find in the Sphinx, at the other end of the Nile valley, an equally simple and inexpensive piece of decorative engineering, one is naturally induced to enquire whether the enormous bulk of the pyramids above it is necessarily due to stones transported from the east bank of the Nile and raised to their present position. The three pyramids of Gizeh of granite, but there is nothing to prove that these are not surrounded, as all other similar chambers and passages, by live rock. It is not a question of evidence but of belief. If you like to think that you are gazing upon the most stupendous and ineffectual exhibitions of human vanity, and that no other structures even furnish a scale with which to measure either the labor or the folly, there is no possible refutation of your opinion. If you prefer to believe that they are revetted hills, tradition aids this as well as the opposite view. It is also the stronger case. If this butte of Gizeh was like any other hill from Cairo to Khartoum, it must have had conical

Top of Second Pyramid	642.9
Top of Great Pyramid	612.10
Top of Third Pyramid	406.8
Top of Hill	250.0
Base of Third Pyramid	203.8
Base of Second Pyramid	195.3
Base of Great Pyramid	162.1
Head of Sphinx	80.0
Excavated Chamber	60.0
Sand plain at Base	33.8
High Nile of 1838	24.8
Low Nile of 1838	0.0

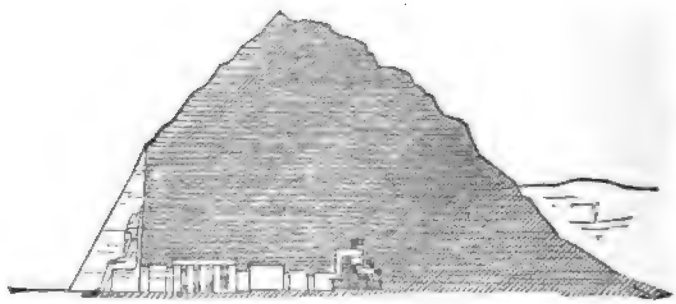


suggest the natural summits of a limestone hill. There is nothing artificial about the second and highest, except the triangular walls which revet its sides. The little pyramids seem to be only piles of rubbish. The top of the pyramid of Cheops is two hundred feet below the upper stratum of the cañon of the Nile. It is exactly the height of two natural hills about twenty miles to the west on the same parallel of latitude. It contains two chambers, a passage, and a few blocks summits. After the quarrymen had honeycombed them, the engineers would have been compelled to take them down in part, and reconstruct them. Clouds of dust and falling stones would have impaired the use of the terrace below. It was indispensable to buttress these crumbling masses and fill their catacombs with rubbish. It was equally desirable to save them as great rocks in a weary land to shade the pools and temples. If this question were given to experts to be decided according to the strict rules of evidence, the decision would be in favor of those who hold that like Ipsambool and Petra these mounds were constructed with gravity and not against it. No importance appears to have attached to

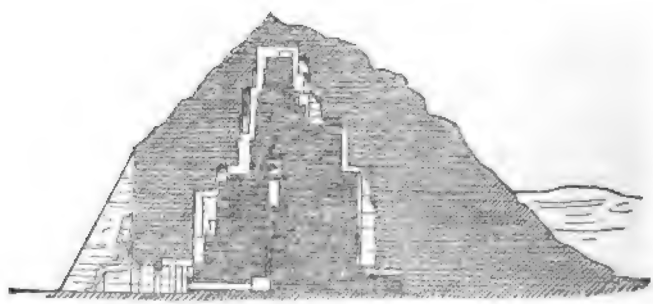
* The fame of the *trilithon* of Baalbek is largely due to a misconception. The immense stones sixty feet in length were quarried on the spot, above their present site. The word is not Greek but Arabic. It does not mean *three stones*, but refers to the Litany, at whose source this fortress stands. The so-called "fourth stone" is simply a partly quarried mass of much more recent date.

them in popular estimation. The peasants told Herodotus that they were constructed from above by the same iconoclastic Philistines who carved the hills at Amrit into country houses. The Copts told ibn-Wasyf that they were worked down from the top. It is the require to be dressed with some pains, but the perpendicular joints might be neglected.

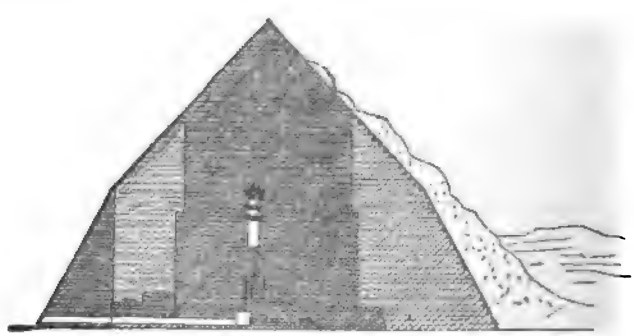
Positive evidence in favor of this theory is offered by a careful inspection of the middle courses of the pyramid of Cheops. Some of the stones show traces



SECTION OF THE SPEOS OF ABU-SIMBEL.



THE TEMPLE DESTROYED BY OVERSTOPING.

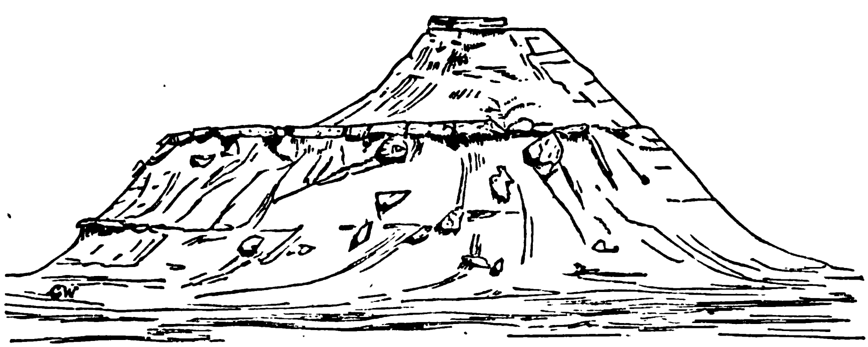


THE PYRAMID COMPLETED "FROM ABOVE."

easier method and the more pleasant alternative. The engineer would mark out a rectangle with care. Then the stones, detached from the upper part of the hill, would be quickly moved to the nearest spot. The bearing surfaces would

of geological waste. If a large part of the material was quarried at a considerable distance, where did the workmen pick up such blocks as these? They are evidently fragments of exposed strata. They are literally crust; baked by the

sun, worn by the sand, eroded by the Nile in prehistoric ages. There is no wear and tear in their present site. Mere scratches made by tourists of the Christian era are still legible. Besides, there is a tradition of Herodotus that in building these pyramids, temples were destroyed. There would be only one way to destroy Ipsambool. A rock-hewn temple is as imperishable as it is cheap. But it is always possible to fill up the temple from above, until the hill has been rebuilt downwards. The engineer could overstope to the summit, and every vestige of the shrine would disappear. If he then revetted it from above, the high-place would lose every distinctive feature. By such a method alone could the crust of the hill be the outside of the pyramid. If this is so, the pyramid did not grow by concentric coatings like the bark of a tree, nor by horizontal layers diminishing in size. A space in the center of the natural mass was carried by passages and chambers through the whole elevation. The interior would be rebuilt, and the natural pillars strengthened. "The top was finished first," said Herodotus, "then the middle parts, and lastly, those that were on the ground." The engineer had the satisfaction of knowing that enduring work had been accomplished at little cost. The Government was so well satisfied with the prodigious equivalent for the million and a half of dollars, which it had expended, that it inscribed the cost so that posterity might audit the accounts of its stewardship. Did the elector of Hesse dare to engrave on Wilhelmshöhe the price his subjects paid for the forms but not the virtues of antiquity? Did even Pericles care to put on the walls of the Parthenon the nominal outlay for the temple which served as a pretext for plundering the military chest of the allies. When a handful of men justified the distinction made by Aristotle between the government of the best, who are necessarily few, and the government of the basest, who are accidentally whirled into power, did they record upon the successive stages of the Court House the successive issues of county bonds? Will the Archbishop of Paris write upon the water table of Notre Dame de Montmartre, that before the first stone was visible on that ill-chosen site more money had been expended than when the summit of the second pyramid rose completed six hundred feet above the altar of the Sphinx? Yet these pyramid-builders, of whom the Egyptians said, "kings reign, engineers govern," have been a by-word and reproach for centuries. But converse with the mighty dead by their great canals, and lamentation over the sepulchres of their sand-strewn reservoirs, give courage to utter a word in their defence. Should Lake Mœris be restored in my lifetime, I will surely build another pyramid to the memory of these engineers. I will take the hill which lies to the north of the Fayoum and reconstruct it within and without. Upon it I will cut, as they did, the cost of the work in terms of several standard articles of food, and leave this monument on the frontier of human thought as an eternal evidence at what a price one may do his work too wisely and too well.



PYRAMIDAL HILL IN THE DESERT. Long. 30° 55′. Lat. 29° 30′.

THE GENERAL THEORY OF THERMO-DYNAMICS.

By Professor OSBORNE REYNOLDS, F.R.S.*

From Papers of the Institution of Civil Engineers.

In lecturing on any subject, it seems to be a natural course to begin with a clear explanation of the nature, purpose, and scope of the subject. But in answer to the question—What is thermo-dynamics? I feel tempted to reply—It is a very difficult subject, nearly, if not quite, unfit for a lecture. The reasoning involved is such as can only be expressed in mathematical language. But this alone should not preclude the discussion of the leading features in popular language. The physical theories of astronomy, light, and sound involve even more complex reasoning, and yet these have been rendered popular, to the very great improvement of the theories. Had it appeared to me that it was the necessity for mathematical expression which alone stood in the way of a general comprehension of this subject, I should have felt compelled to decline to deliver this lecture, honorable as I acknowledge the task to be.

What I conceive is the real difficulty in the apprehension of the leading features of thermo-dynamics is, that it deals with a thing or entity (if I may so call heat) which, although we can recognize and measure its effects, is yet of such a nature that we cannot with any of our senses perceive its mode of operation.

Imagine, for a moment, that clocks had been the work of Nature, and that the mechanism had been on such a small scale as to be imperceptible even with the highest microscope. The task of Galileo would then have been reversed; instead of inventing machinery to perform a certain object, his task would have been from the observed motion of the hands to have discovered the mechanical principles and actions of which these motions were the result. Such an effort of reason would be strictly parallel to that which was required for the discovery of the mechanical principles and actions of which the phenomena of heat were the result.

In the imaginary case of the clock, the discovery might have been made in either of two ways. The scientific method would have been to have observed that the motion of the hands of the clock depended on uniform intermittent motion; this would have led to the principle of the uniformity of the period of vibrating bodies, and on this principle the whole theory of dynamics might have been founded. Such a theory would have been as obscure, but not more obscure, than the theory of thermo-dynamics. But there was another method in the case of timekeepers, the one by which the theory of dynamics was actually brought to light—namely, the invention of an artificial clock, the action of which could be seen, and, so to speak, understood. It was from the pendulum that the constancy of the periods of vibrating bodies was discovered, and from this followed the dynamical theories of astronomy, light, and sound. There is no great difficulty in the apprehension of these theories, because they do not call for the creation of a mental picture, but merely for the exaggeration or diminution of what we can actually see in the clock.

As regards the mechanical theory of heat, however, no visible mechanical contrivance was discovered or recognized which afforded an example of this action; apparently, therefore, the only possible method was the scientific method—namely, the discovery of the laws of its action from the observation of the phenomena of heat, and accepting these laws, without forming any mental image of the dynamical origin, was the only method open. This is what the present theory of thermo-dynamics purports to be.

But although the theory of thermo-dynamics may be said to have been discovered in the form in which it is now put forward, this is not quite true. For one of the discoverers of the second law, and the one who had priority over the others, worked by the aid of a definite mechanical hypothesis as to the actual molecular motions and forces on which the phenomena of heat depend, and many of the most important steps in the theory are

* The lecturer was elected M. Inst. C.E. on the 4th of December following.

solely to be attributed to his labors. But to return to the theory. This may be defined as including all the reasoning based on two perfectly general experimental laws, without any hypothesis as to the mechanical origin of heat. In this form thermo-dynamics is a purely mathematical subject and unfit for a lecture. But as no one who has studied the subject doubts for a moment the mechanical origin of these laws, I shall be following the spirit, if not the letter of my subject, if I introduce a conception of the mechanical actions from which these laws spring. And this I shall do, although I should hardly have ventured, had it not been that, while considering this lecture, I hit on certain mechanical contrivances which afford sensible examples of the action of heat, in the same way as the pendulum is an example of the same principles as those involved in the phenomena of sound and light. These examples, thanks to the ready aid of Mr. Forster in constructing the apparatus, I am in a position to show you, and I am not without hope that these kinetic engines may in a great measure remove the source of obscurity on which I have dwelt.

The general action of heat to cause matter to expand, or to tend to expand, is sufficiently obvious and popular. That the expanding matter will do work is also sufficiently obvious, but the exact part which the heat plays in doing this work is very obscure.

It is now known that heat performs two, and it may well be said three, distinct parts in doing the work. These are—

(1) To supply the energy equivalent to the work done.

(2) To give the matter the elasticity which enables it to expand, *i.e.*, to convert the inert matter into an acting machine.

(3) To convey itself (*i.e.*, heat) in and out of the matter.

This third function is generally taken for granted in the theory of thermo-dynamics.

In order to make any use of thermodynamics, a knowledge of the experimental phenomena of heat is necessary; but as time will not permit of my entering largely into these, I have had some of the leading facts suspended as diagrams. One or two it will be well to mention.

Heat as a quantity is independent of temperature, the thermal unit taken being the amount of heat necessary to raise 1 lb. of matter 1° Fahrenheit.

Temperature represents the intensity of heat in matter. Matter in most of its forms expands more or less uniformly as we add heat to it; hence the expansion of matter measures temperature. Gases such as air expand in absolute proportion to the heat added under a constant pressure.

Absolute temperature is an idea derived from the observed rate of contraction of gases; they would vanish to nothing with the temperature 461° below zero Fahrenheit. For the other phenomena I must refer to the diagrams as I proceed.

Our knowledge of these facts has been accumulating during the last two hundred years, and it was in a very complete condition forty years ago, before thermodynamics was born. The birth of this science may be considered as the result of the recognition of work—motion against resistance as a true measure of mechanical action, and of accumulated work or energy as the potency of all sources of power. These ideas have now become extremely popular, and all are able to recognize in the raised weight, the bent spring, the moving hammer, the same thing, energy, which is measured by the amount of work which can be derived from any of these sources.

Before the recognition of this means of measuring mechanical potency, any definite idea of the true mechanical action of heat was impossible, for we had not recognized the only mechanical action by which it can be measured.

In 1843 Joule conclusively proved that, by the expenditure of 772 ft.-lbs. a thermal unit of heat must be produced, provided all the work was spent in producing heat. The simplicity of the ideas here involved, and the completeness of Joule's proof, acted at once to render the first law popular. No language can be too strong in which to express the importance of this discovery; yet, as was long ago pointed out by Rankine, the very popularity of Joule's law went a long way to obscure the fact that it did not constitute the sole foundation of the theory of ther-

mo-dynamics. Before Joule's discovery it was recognized that heat acted a part in causing work to be performed. It was clearly seen that it was heat which caused the water to expand into steam, against the resistance of the engine, and the necessity of heat to cause matter to expand was recognized.

To make matter do work, it was only necessary to heat it. It would expand, raising a weight; and since after doing its work the matter was still hot, it was supposed that the only necessity for the heat was to add increased elasticity to matter. It was seen that the heat that had once been used was so degraded in temperature that it could not be all used again. So that, although there was no idea that heat was actually consumed in doing the work, it was seen that for continuous work a continuous supply of heat at a high temperature was necessary. As regards the exact proportion of heat required for the supply of elasticity, to perform a certain quantity of work, fairly clear ideas prevailed. It was seen that this depended on various circumstances. These were formulated by Carnot, who in 1828 gave a formula, which is equivalent to our second law of thermo-dynamics, of which it was the parent.

Now this idea that heat merely caused work to be done was not absurd, as is sometimes supposed. Indeed we may say that the present popular idea that the whole heat is convertible into work is more erroneous than the old idea in the ratio of 10 to 1; because the old idea that the function of heat is to supply elasticity was right, as far as it went. Although the present idea that the function of heat is to supply energy from which the work is drawn is also right; yet, in any known possible heat-engine ten times more heat is necessary for the purpose of giving elasticity to matter than is converted into work by elasticity. This error, which seems to be very general amongst those who have not made a special study of the subject, may, I think, be attributed—first, to the popularity of the first law of thermo-dynamics, and secondly to the fact that although the second law of thermodynamics is nothing more nor less than a statement of the proportion which the quantity of heat necessary to produce elasticity bears to the quantity which this elasticity will convert into work, yet that it is the invariable custom in stating this law to omit all attempt to explain the purpose which this excess of heat serves; the reason for this omission being that experiment only shows that this heat is necessary, and hence this is all that we have a right to say.

If such an error prevails it is only a popular error, for it certainly did not affect the progress of the science. No sooner did Joule's law become known than it was taken up by Rankine, who, in 1849, published a complete theory of thermo-dynamics, based, as I have said, on a hypothetical constitution of matter. This was almost simultaneously followed by theories based on an improved form of Carnot's reasoning by Thomson and Clausius.

Rankine's theory was based on a hypothetical constitution of matter. He invented a system of molecular motions and constraints, which he called molecular vortices, and he then calculated the effects of these motions by the theory of mechanics. The fact that his reasoning was based on a hypothesis was considered by many as a fault in his reasoning. But on the other hand the clear idea thus obtained, as to the reason of everything he was doing, gave him such an advantage over those who were working by experimental laws, of the meaning of which they would venture no opinion, that he was led to make discovery after discovery in advance of his competitors, while some of his discoveries are still beyond the reach of experiment.

There was, however, a difficulty Rankine had to face; some properties of matter were pointed out which his hypothetical matter did not possess. This was not much to be wondered at, for although Rankine had invented machinery which would account for the mechanical action of heat, there was no reason to suppose this to be the only machinery. Rankine, with a view to the difficult calculations he had to make, had chosen machinery as simple as possible. Instead, however, of trying to complicate it, he, yielding to the opinion of his contemporaries, adopted the general conclusions to which it had led him as axiomatic laws, and so cut himself adrift from his hypothesis.

It comes to be, then, that the student of thermo-dynamics finds as a reason why we must pass a large amount of

heat through his engine, besides that which is converted into work, he is to accept an axiomatic law as to the greatest possible amount that can be converted under the circumstances.

To tell a child who asks why he cannot have more food, that he can only have 6 oz. a day, would be considered cruel. So, to tell a student who wants to know why, out of the ten million foot-lbs. in 1 lb. of coal, a steam engine can only give one million as work, that he is only allowed $\frac{T_1 - T_2}{T_1 + 461}$, is cruel, yet this is all he can have from the theory of thermo-dynamics based on its experimental laws.

Rankine, when compelled to abandon his hypothesis as the foundation of his theory by the objections justly urged against it, pointed out the great disadvantage of a mechanical theory conveying no conception of the mechanical basis of its laws; and called on all those who taught the subject to try and find some popular means of illustrating the second law.

This call was made twenty years ago, but, I believe, up to the present time no such illustration has been forthcoming. When undertaking this lecture I had no idea of such an illustration, and I did not intend to say much as to the reason of the second law. But, as I have said, three weeks ago an idea occurred to me. It arose in this way: Heat acts in matter to transform heat into work by molecular mechanism. Having much studied the subject, I have in my mind a picture, right or wrong, of the mechanism, and the part which heat acts. The question occurred—Is there no way of making a machine such that, although the parts are in visible motion, and the energy transformed to work is visible energy, yet the energy supplied shall have the characteristics of heat energy, and the machine shall act simply in virtue of the elasticity caused by the motion of its parts?

The question had no sooner arisen than several ways of carrying out the idea presented themselves.

The general idea of the mechanical condition which we call heat is, that the particles of matter are in active motion; but it is the motion of the individuals in a mob, with no common direction or aim. Rankine assumed the motion to be rotatory, but it now appears more probable that the motion in the particles is oscillatory, undulatory, rotatory, and all kinds of motion whatsoever; so that the communication of heat to matter means the communication of internal agitation —mob agitation. If, then, we are to make a machine to act the part of hot matter, we must make a machine to perform its work in virtue of the communication of internal promiscuous motion amongst its parts. The action of heat-mechanism to do work is simply that of expansion of volume, or the increased effort to expand owing to increased agitation. I first tried to think of some working arrangements of small bodies which should forcibly expand when shaken; but it appeared that it would be much easier to effect a contraction. This was as good. As long as any definite alteration in shape could be produced against resistances by a definite amount of agitation in its parts, we should have a machine illustrating the action of the heat engine.

Suppose we want to raise a bucket from a well. Our best way is to pull or wind up the rope, but that is because the energy we employ is in a completely directable form. Suppose we had no such directable energy, but could only shake the rope, it having been first made fast at the top (Fig. 1). Then, it being a heavy rope, a chain is better; suppose we shake the chain laterally, waves will run down the chain, and, if we go on shaking, the chain will assume a continuously-changing sinuous form (Figs. 2 and 3); and, as the chain does not stretch, the bucket must be raised for the sinuosities. The chain will have changed its mechanical character, and from being a tight line or tie in a vertical direction, will possess kinetic elasticity, that is, elasticity in virtue of its motion, causing it to contract its vertical length.

The bucket will be raised, although not to the top of the well, and work will have been done in raising it, but the work spent in shaking the chain will be not only the equivalent of the work spent in raising the bucket, but also of all the kinetic agitation in the chain necessary to raise the bucket. Having raised the bucket as far as possible with a certain power of agitation, if the supply of

agitation be cut off, then that already in the chain will sustain the bucket until it is destroyed by friction, when the bucket will gradually descend.

But, if we want to do more work, to raise another bucket, we may take that which is raised off at the level at which it is raised; then, to get the chain down again, we must allow it to cool, *i. e.*, allow the agitation to die out; then, attaching another bucket, to raise this we shall again have to supply the same heat, perform the same work, *i. e.*, the work to raise the bucket, and the agitation energy of the chain. Thus we see that the energy necessary to the working of the machine serves two purposes, it supplies the energy necessary to raise the bucket, and the energy necessary to convert the chain from an inextensible tie into an elastic contracting system, capable of raising the weight, neither of which portions of energy is again serviceable after the bucket has been raised. The one portion is already converted into work, and the other, although still in existence in the chain as energy, can only sustain the position of the chain. Before it could be used to do more work it must be got out of the chain and back again, which is just the thing you cannot do; we can get some of it out and some of it back, but not all.

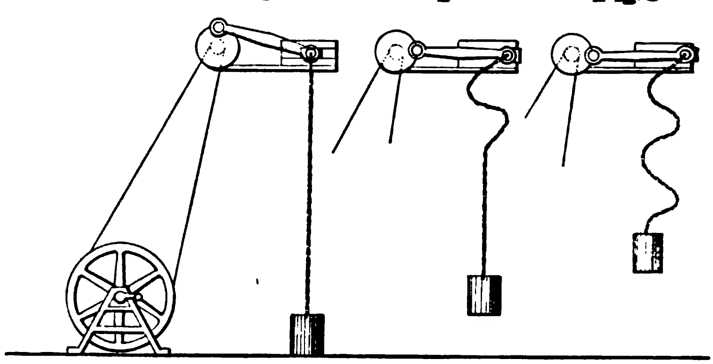
It must not be supposed that this method of raising a bucket by shaking the rope is recommended as the best means. No one would dream of using it if we could get a direct pull, but that is nothing to the point. We are considering the action of heat, and we have limited ourselves to using energy of the same kind that heat supplies; that is, energy in the form of promiscuous agitation, absolutely without direction, so that the question is, how can we raise the bucket by shaking?

I feel that there is a childish simplicity about this illustration that may at first raise the feeling of "Abana and Pharpar, rivers of Damascus," in the minds of some of my hearers; but, should this be the case, I have every confidence that calm reflection will have the same effect as on Naaman.

The case of the shaken rope, as I have put it, is no mere illustration of the action of heat, but an instance of the same application of the same principles. The sensible energy in the shaking rope only differs from the energy of heat, *i. e.*, a bar of metal is the scale of the motion; we see that in the chain, but not in the bar, not because the molecules of the bar are moving slower, but because the scale of motion is infinitely smaller. The temperature of the bar, from absolute zero, measures the mean square of the velocity of all its parts, multiplied by some constant depending on the mass of the parts which are moving together; so the mean square of the velocity of the chain, multiplied by the weight per foot of the chain, really represents the absolute temperature of the sensible energy in the chain.

The apparatus which I have on the table is an obvious adaptation of the rope and the bucket. There are three different illustrations apparently very different

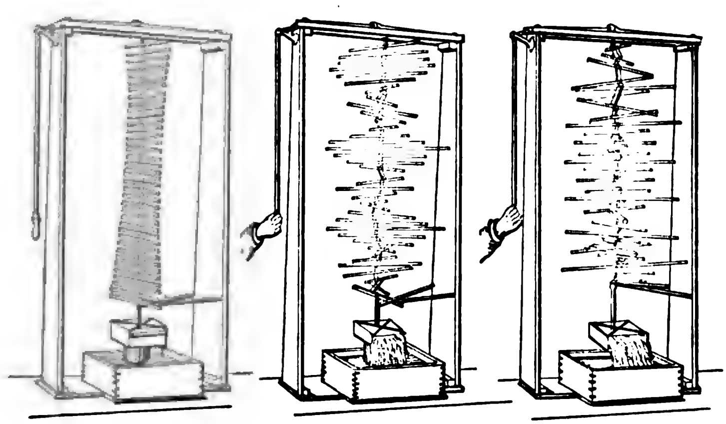
Fig. 1 Fig. 2 Fig. 3



in form, but all working by the same principle.

Here is the chain (Figs. 1, 2, 3), by the shaking of which (addition of promiscuous energy) a weight of 2 lbs. is raised 3 feet, or 6 foot-lbs. of work done; here is another sort of chain, a series of parallel horizontal bars of wood, connected and suspended by two strings (Figs. 4, 5 and 6). By giving a circular oscillation to the upper bar, the whole apparatus is set into a twisting motion (agitation); the strings are continually bent, and the vertical length of the whole system is shortened, and a weight of 10 lbs., or the bucket of the pump, is caused to rise, raising water just as if we boiled water under the piston of a steam engine. To get the bucket down again for another stroke, we must quiet or cool the chain, just as we must condense the steam, and the energy taken out of the chain in cooling corresponds exactly with the heat that must be taken out of the steam in order to condense it.

Fig. 4 Fig. 5 Fig. 6



The waves of the sea constitute a source of energy in the form of sensible agitation; but this energy cannot be used to work continuously one of these kinetic machines, for exactly the same reason as the heat in the bodies at the mean temperature of the earth's surface cannot be used to work heat engines. A chain attached to a ship's mast in a rough sea would become elastic with agitation, but this elasticity could not be used to raise cargo out of the hold, because it would be a constant quantity as long as the roughness of the sea lasted.

In practical mechanics we have no source of energy consisting of sensible agitation, besides the waves of the sea; so that there has been no demand for these kinetic engines to transform sensible mob energy into work; had there been, I might have patented my idea, though probably it would have long ago been discovered. But there has been a demand for what we may call sensible kinetic elasticity, to perform for sensible motion the part which the heat elasticity performs in the thermometer, and for this purpose the principle of the kinetic machine was long ago applied by Watt. The common governor of a steam engine acts by kinetic elasticity, which elasticity depending on the speed at which the governor is driven, enables the governor to contract as the speed increases. The motion of the governor is not of the form of promiscuous agitation, but, though systematic, all the motion is at right angles to the direction of operation, so that the principle of its action is the same.

The kinetic elasticity of the governor performs the same part as the heat elasticity in the matter of the thermometer; the first measures by contraction the velocity of the engine, and the other measures by expansion the velocity of the molecules of the matter by which it is surrounded, so that while we now see that while measuring the speed of sensible revolution, we are performing on a

different scale the same operation as measuring the temperature of bodies, which depends on the molecular velocities, and that quite unconsciously we have constructed instruments to perform the two similar operations which act by means of the same mechanical action, namely, kinetic elasticity.

These kinetic examples of the action of heat must not be expected to simplify the theory, except in so far as they give the mind something definite to grasp; what they do is to substitute something we can see for what we can barely conceive.

The theory of thermo-dynamics can be deduced from any one of these kinetic examples by the application of the principles of mechanics; such application involves complex dynamical reasoning, such as can only be executed by the aid of mathematics, and would be altogether unfit to introduce into a lecture. I shall therefore pass on to some considerations resulting from the theory of thermo-dynamics.

The discovery of the two laws have enabled us to perfect and complete our experimental knowledge of the phenomena of heat. But probably the greatest practical use is that these two laws enable us to calculate with certainty, from the experimental properties of any matter, the extreme potency of any source of power.

Thus we find by experiment that a pound of coal burnt in a furnace yields fourteen to sixteen thousand thermal units of heat. The first law, Joule's law, tells us at once that this is equivalent to from 11,000,000 to 13,000,000 foot-lbs. of energy. But this is not, as seems to be generally supposed, the power of coal. The second law of thermo-dynamics tells us that in order that this energy might be realized, it must be capable of being developed at an infinite temperature, whereas we know that this cannot be the case; and there is a growing idea that the temperature at which coal will burn is not so extremely high, about 3,000° Fahrenheit. Taking this temperature, and assuming the temperature of the atmosphere to be 60°, we have for the proportion of the heat of coal, that we could with a perfect engine call power, $\frac{2940}{3461}$, about 80 per cent., or from 9,000,000 to 11,000,000 foot-lbs.

Again, we know the heat properties of all known liquids and gases, so that we can, by the second law, tell the greatest possible proportion of the heat received, which can be converted into power by any of these agents.

In the steam-engine, for instance, we see that the present limits of art restrict the temperatures absolutely to 400°, and practically the limits are much less; while the lowest temperature that can be worked to in a condenser is 100°. Then, as the limit to the possibility, we have one-third as the greatest proportion, or three out of the nine million foot-lbs.

The greatest actual achievement by Mr. Perkins has been about two millions, while the best engines in use only give us a little over one million, or about one-ninth of the possible realizable portion between 3,000° and the mean temperature of the earth's surface.

I cannot here enter upon these, but the reasons why higher temperatures cannot be used in the steam-engine are obvious enough.

The same reasons do not apply to hot air as an agent. This may be worked at much greater temperatures; and about thirty years ago, as soon as it appeared from the science of thermo-dynamics that the limit of efficiency depended on the range of temperature, attention was much directed to air as a substitute for steam. The attempts then made failed through what were then called practical, or art difficulties.

Just at the present time the possibility of other heat-engines than steam-engines is again come to the front; and as this is so, it seems desirable to call attention to a circumstance connected with heat-engines which has as yet occupied quite a subordinate place in the theory of heat-engines. This is the law as to the rate at which heat can be made to do work by an agent, such as steam or air. The greatest possible efficiency of the agent, *i.e.*, the proportion which the work done bears to the mechanical equivalent of the heat spent, is a matter of fundamental importance; but the rapidity with which the heat can be so transformed with a given amount of apparatus, as an engine of a given weight, is a matter of at least as great importance.

Which would be the best engine for a steamboat; one that would develop 20 HP. for every ton gross weight, consuming 2 lbs. of coal per HP. per hour, or

one that only gave 2 HP. per ton weight, and only consumed 1 lb. of coal? Unquestionably the former; yet hitherto the question of heat economy has been considered theoretically, to the exclusion of time economy. Yet the latter forms a legitimate part of the subject of thermodynamics, and has played a greater part in the selection of steam as the fittest agent than the consideration of the heat-economy.

In the theory of thermo-dynamics it is assumed that the working agent, be it water or any other, can be heated up and cooled down at pleasure, without any consideration as to the time taken for these operations, which are considered to be mere mechanical details.

Yet in the science of heat a great amount of labor has been spent; a great amount of knowledge gained as to the rate at which heat will traverse matter. And more than this; it is well known that heat cannot be made to enter and leave matter without a certain loss of power, *i.e.*, a certain lowering of the working range of temperature. It is by heat that heat is carried into the substance; and hence, as I have indicated, there is a third law of thermo-dynamics relative to this transmission. Heat only flows down the gradient of temperature, and in any particular substance the rate at which heat flows is proportional to the gradient of temperature. Hence to get the heat from the source or furnace into the working substance a certain time must be consumed, and this time diminishes as the difference of temperature of the furnace and the working substance increases.

The examples of the kinetic engines which I have shown you will illustrate this. If we shake the end of a chain, the wriggle passes along the chain at a given speed. It appears that an interval must elapse between the first shaking of the chain and the establishment of sufficient agitation to move the bucket; a further interval before the bucket is completely raised; and further still, another interval must elapse before the chain can be cooled again for another stroke; so that this kinetic engine will only work at a given rate. I can increase this rate by shaking harder, but then I expend more energy in proportion to the work done.

This exactly corresponds with what goes on in the steam-engine, only, owing to the agent—water—being heated, expanded, and cooled severally in the boiler, cylinder, and condenser, the connection is somewhat confused.

But it is clear that for every HP. something like 15 million foot-pounds of power have to pass from the furnace into the boiler. As out of this 15 we cannot use more than two million, the remaining 13 are available for forcing the heat from the products of combustion into the water, and out of the steam into the condensing water, and they are usefully employed for this purpose.

The boilers are made small enough to produce sufficient steam, and this size is determined by the difference of the internal temperature of the gases in the furnace and the water in the boiler, and whatever diminishes this difference would necessarily increase the size of the heating surface, *i.e.*, the weight of the engine. The power which this difference of temperature represents cannot be realized in the steam-engine, so that it is most usefully employed in diminishing the necessary size of the boiler. Still it is an important fact to recognize that our present steam-engines require the expenditure of more than five times as much of the power of the heat (not of the heat) in getting the heat into the working substance as in performing the actual operation. This loss of power does not so much occur in the resistance of the metal which separates the furnace from the water as in the resistance of the gases. Gas is a very bad conductor; and though a thin layer adjacent to the plates is always considerably cooled, little further cooling goes on until, by the internal currents, this layer is removed, and a fresh hot layer substituted in its place.

Similar resistance would occur inside the boiler between the water and the hot plate, nay does occur, until the water begins to boil, but then the evaporation of the water takes place at the hot surface, and every particle of water boiled absorbs a great deal of heat, which leaves the surface in the form of bubbles, allowing fresh water to come up.

If we had air inside the boiler instead of water, we should require from five to ten times the surface to carry off the same heat, which is a sufficient reason why what are called hot-air engines can-

not answer, even did not the same argument hold with enormously greater force in the condenser.

Steam is as bad a conductor of heat as air as long as it does not condense, but, in condensing, steam will conduct heat to a cold surface at an almost infinite rate, for as the steam comes up to the surface it is virtually annihilated, leaving room for fresh steam to follow, which it will do if necessary with the velocity of sound. If, however, there is the least incondensable air in the steam this will be left as a layer against the fresh steam. Some years ago I made some experiments on this subject, which showed that 5 or 10 per cent. of air in the steam would virtually prevent condensation.

If a flask be boiled till all the air is out, and nothing but pure steam is left, and if the flask be then closed and a few drops of cold water introduced, the pressure instantly falls to zero, though it immediately recovers from the boiling of the water in the flask. If now a little air be admitted, and allowed to mix with the steam, the few drops of water produce scarcely any effect.

The facility with which steam carries heat to a cold surface is both an enormous advantage and some drawback; as compared with air it is an enormous advantage in enabling the steam to be cooled in the condenser. But during the working of the steam in the cylinder, when the steam is wanted to keep its heat, the facility with which it condenses is a great drawback, and necessitates the keeping of the cylinder hotter than the steam by a steam-jacket. For this part of its work the non-conductivity of incondensable air is a great advantage.

In dwelling thus on the conducting powers of air and steam, my purpose has been to prepare the way for a few remarks I wish to make on another form of heat-engine—the engine in which the heat is generated in the working substance itself.

The combustion engine, in the form of the cannon, is the oldest form of heat-engine. Here the chemically separate elements in the form of gunpowder are the working substances put into the cylinder; they take in with them the potential energy of chemical separation, which by means of a spark take the kinetic form of heat. Here there is no conduction, the kinetic elasticity propels the shot, and all the heat over and above that used in imparting energy to the shot is lost. The advantages of this form of engine are two. There is no time necessary for conduction, and as the gas generated is not condensable, there is little loss of heat by conduction to the cold metal.

These two advantages are very great, but I should not have mentioned them in reference to guns were it not that there appears to be the dawning of an idea of taming this form of engine so as to substitute it for the steam-engine. To do this it is necessary to introduce coal or coal gas;—and oxygen in the form of air in place of gunpowder. The thermo-dynamic theory applied to such engines shows that they should possess great advantages over the steam-engine in point of economy. And the considerations I have brought forward as to the loss of the power of heat in the transference of heat from the furnace to the boiler seem to promise such engines an enormous advantage in rate of work, while the substitution of a non-condensable gas for steam in the cylinder seems to get over the art-difficulty of making cylinders to work under high temperatures. We cannot expect any piston to work in a cylinder of over 300° or 400° temperature, but with non-condensing gases the cylinder may be kept cool with little cooling effect on the gases contained in it, even if the temperature of these is 3,000°. This will be the case if the gas in the cylinder is not in a violent state of internal agitation, but it should be remembered that all internal currents much facilitate the conveyance of heat to the walls.

There is one drawback shown by the theory of these engines. The simple expansion of the gases resulting from combustion is not sufficient to cool them to anything like the temperature of 60°, and to get the greatest economy some of the remaining heat should be used to heat the fresh charge. To do this, however, would necessitate the extraction of the heat from one mass of gas to communicate it to another, which would introduce all the difficulties of the boiler increased by having gas instead of water.

But even wasting this heat, the theory still shows a large margin of economy for such engines over the present perform-

ance of steam-engines, a margin which is said to have been already realized in the gas engine, which is a form of combustion-engine in a high state of efficiency. Now, by means of Dowson gas, Messrs. Crossley seem to have obtained 2,000,000 out of the 10,000,000 ft.-lbs. in 1 lb. of coal. Further accomplishment in this direction is a question of art; but while on all other hands science shows impassable barriers not far in advance of the present achievements of art, in this direction thermo-dynamics extended to include the rate of operation shows no known barriers; while the fact that, as gas-engines, this system of combustion heat-engines has already established a footing assures them continual improvement.

In conclusion I would say, by way of caution, that the theory of thermo-dynamics does not lead to the conclusion, which seems to be generally held by those who have only realized the first law of the science, that the steam-engine is a semi-barbarous machine, wasting more than it uses, very well for those who know no science, but only waiting until those better educated have time to turn their attention to practical matters, and then to give place to something much better. Thermo-dynamics shows us not the faults but the perfections of the steam-engine in which there is no waste of power, since all is used either in doing work or in promoting the rate at which the work can be done. Next to the watch the steam-engine is the highest development of mechanical art, and the science of thermo-dynamics may be said to be the result of the study of the steam-engine.

On the motion of the president a cordial vote of thanks to Professor Reynolds was carried by acclamation, for his valuable contribution to a most important subject.

CUSHIONING IN ENGINES.

By GEO. L. MORTON, M. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

In order to avoid filling the entire clearance space of engine cylinders with live steam for each opening of the admission port, it is found advisable to resort to cushioning. This consists in closing the exhaust port before the return stroke is completed, so as to re-compress the enclosed spent steam, and by this means aid in filling the clearance space. The quantity of steam used is in this manner diminished by the amount thus compressed; but in accomplishing this reduction, the work required to re-compress the residual steam is sacrificed. Thus one objectionable feature is obviated by introducing another one, which, in itself, is also objectionable. Evidently there is some point for each case beyond which this last detrimental effect will predominate over the good result following from it, or a point at which the benefits derived from cushioning are the greatest possible under the circumstances. With such adjustment it is apparent that we should be able to realize the greatest amount of work from a given quantity of steam that is attainable under the imposed conditions of pressure, proportion of clearance, and ratio of expansion; in other words, the amount of work realized from a unit volume of steam at constant pressure will be a maximum.

A means is thus afforded for finding the most advantageous point in the stroke for closing the exhaust port and commencing to cushion. An expression for the ratio of the work, divided by the volume of steam from which this work is derived, must first be found, and then, regarding the part of the stroke through which cushioning takes place as the independent variable, ascertain what value of this variable will make the above ratio a maximum. In the solution of this problem, which follows, it will be supposed that expansion and compression take place so that the pressure varies with reference to the volume in such manner that the product of these two factors is a constant—that is, the curves of expansion and compression will be regarded as hyperbolic. This is done, not

alone because it will render the equations of these curves more simple than they otherwise would be, but rather because ordinarily it is found that in practice the curves of the indicator card agree most nearly with the hyperbola. Theory alone would perhaps indicate a different curve, but practice has demonstrated that the hyperbola is the standard for the expansion of steam. In what follows, release will also be assumed to take place at the end of the stroke, but a means of making a correction for its earlier occurrence will be indicated.

Let Fig. 1 be an ideal indicator diagram, and let the following symbols denote the quantities pointed out in each case, viz.:

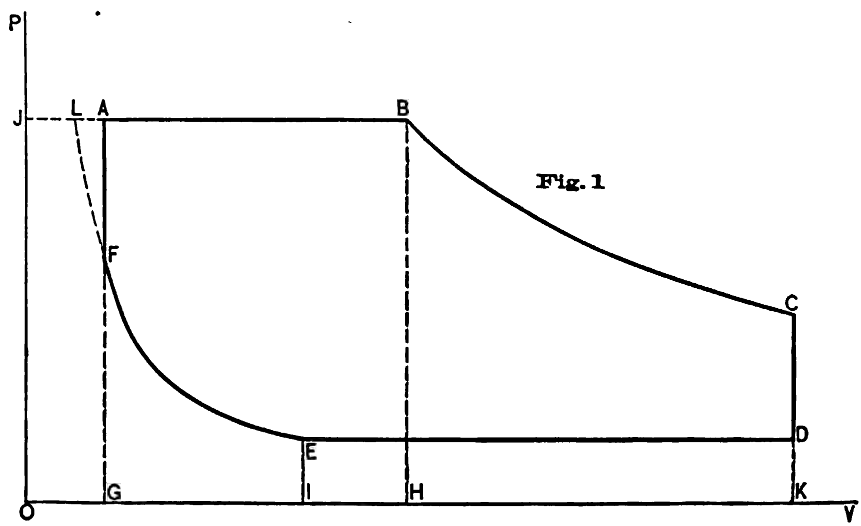


Fig. 1

v_1 = the volume of the clearance, as denoted by OG=JA in the diagram.

v_2 = the combined volume of the cylinder and clearance=OK.

p_1 = the initial absolute pressure=JO.

p_2 = the absolute back pressure during exhaust=DK.

p_3 = the absolute pressure to which steam is cushioned at the end of the stroke=FG.

v_4 = the volume of steam in the cylinder at the instant of closing the exhaust=OI.

c = the ratio of the combined volume of the cylinder and clearance at the instant of exhaust closure to the clearance volume $=\frac{v_4}{v_1}$.

v_3 = the volume of steam contained in the cylinder and clearance space at the instant of cut-off=JB.

r = the ratio of expansion $=\frac{v_2}{v_3}$.

For hyperbolic expansion the equation of the curve BC is, where p and v are respectively the pressure and volume at any point,

$$pv=p_1v_3=p_1\frac{v_2}{r}; \qquad (1)$$

and the equation of the compression curve EF is, where p' and v' are respectively the pressure and volume at any point,

$$p'v'=p_3v_4=p_3cv_1 \qquad (2)$$

From the foregoing conditions we may compute the amount of work realized as expressed by the area of the diagram. Let this area be denoted by A, then,

A=ABCDEF=ABHG+BCKH−EDKI−FEIG.

The area

$$\text{ABHG}=\left(v_3-v_1\right)p_1=\left(\frac{v_2}{r}-v_1\right)p_1;$$

$$\text{BCKH}=\int_{v_3}^{v_2}p\,dv=\int_{\frac{v_2}{r}}^{v_2}p_1\frac{v_2}{r}\frac{dv}{v}=$$

$$\frac{p_1v_2}{r}\text{ hyp. log. }r;$$

and

$$\text{EDKI}=(v_2-v_4)p_2=(v_2-cv_1)p_2;$$

$$\text{FEIG}=\int_{v_1}^{v_4}p'dv'=\int_{v_1}^{cv_1}p_2cv_1\frac{dv'}{v'}$$
$$=p_2cv_1 \text{ hyp. log. } c.$$

Hence

$$\text{A}=\left(\frac{v_2}{r}-v_1\right)p_1+\frac{p_1v_2}{r}\text{ hyp. log. } r$$
$$-(v_2-cv_1)p_2-p_2cv_1 \text{ hyp. log. } c.$$

or

$$\text{A}=\frac{v_1p_2}{r}\left\{\left(\frac{v_2}{v_1}-r\right)\frac{p_1}{p_2}+\frac{v_2p_1}{v_1p_2}\text{ hyp. log. } r\right.$$
$$\left.-\left(\frac{v_2}{v_1}-c\right)r-cr\text{ hyp. log. } c.\right\} \qquad (3)$$

If the volumes be expressed in cubic feet, and the pressures in pounds per square foot, then A represents the energy per stroke in foot-pounds.

In ascertaining the volume of steam used per stroke, it must be borne in mind that the steam due to cushioning in the previous stroke is retained in the cylinder. At the instant of exhaust closure this residual steam is under the exhaust pressure p_2, and it has the volume $v_4=cv_1$. If this steam be compressed until it sustains the pressure p_1, and if the law of compression be hyperbolic, it will have the volume

$$\text{JL}=\frac{cv_1p_2}{p_1}.$$

Let V′ equal the volume of steam used per stroke under the pressure p_1, then

$$\text{V}'=v_2-\frac{cv_1}{p_1}p_2=\frac{v_2}{r}-\frac{cv_1}{p_1}p_2. \qquad (4)$$

The pressure p_1 will vary for different engines, and for the same engine at different times, so that, if the expression is to be perfectly general so as to serve as a ready means of comparison in all cases, this volume must be reduced to a standard pressure.

Let the standard pressure be represented by P, then let V be the volume under this pressure, and we have

$$\text{V}=\frac{\text{V}'}{\text{P}}=\frac{p_1}{\text{P}}\left(\frac{v_2}{r}-\frac{cv_1}{p_1}p_2\right) \qquad (5)$$

This pressure is arbitrary, and it may be given any value. It will simplify the formula somewhat to make P equal to unity. Assigning P this value, and we have

$$\text{V}=\frac{v_1p_2}{r}\left\{\frac{v_2}{v_1}\frac{p_1}{p_2}-cr\right\}. \qquad (6)$$

Let H equal the amount of work realized from a unit volume of steam at the pressure of unity, then

$$\text{H}=\frac{\text{A}}{\text{V}}=$$
$$\frac{\left(\frac{v_2}{v_1}-r\right)\frac{p_1}{p_2}+\frac{v_2}{v_1}\frac{p_1}{p_2}\text{hyp.log.}r-\left(\frac{v_2}{v_1}-c\right)r-cr\text{ hyp.log.}c}{\frac{v_2}{v_1}\frac{p_1}{p_2}-cr} \qquad (7)$$

In employing this formula, since pressures and volumes appear as ratios, we may employ such units as are most convenient, provided both volumes be expressed in the same unit of volume, as cubic foot or cubic inch; and the two pressures in the same unit of pressure, as pounds per square inch or square foot.

Since we have the most efficient use of steam when the ratio H is the greatest possible, and having fixed upon the values that shall be given $\frac{v_2}{v_1}$, $\frac{p_1}{p_2}$, and r, let us find what value of c will, under these imposed conditions, make H a maximum. We shall thus be enabled to determine an expression for the proper point of exhaust closure. Taking the differential coefficient of H with respect to c, and placing it equal to zero, and we have, after reduction,

$$\frac{d\text{H}}{dc}=$$
$$\frac{r\left\{cr-\frac{p_1v_2}{p_2v_1}\text{hyp. log.}\frac{c}{r}+\left(\frac{v_2}{v_1}-r\right)\frac{p_1}{p_2}-r\frac{v_2}{v_1}\right\}}{\left(\frac{p_1}{p_2}\frac{v_2}{v_1}-cr\right)^2}=0;$$

whence,

$$cr-\frac{p_1v_2}{p_2v_1}\text{ hyp. log. } c=$$
$$\frac{v_2}{v_1}\left\{r-\frac{p_1}{p_2}(1+\text{hyp. log.}r)\right\}+\frac{p_1}{p_2}r \qquad (8)$$

This formula is the same as the one arrived at by Prof. Robinson in a paper on "Cushion Adjustment in Engines," published in the Transactions of the American Society of Mechanical Engineers. It is also substantially the same as the one given by Coterell in his work on "The Steam Engine." The equation

is an implicit function of c, and the value of c can only be obtained by a series of approximations which are too tedious to make the formula alone of much practical use. However, a table may be constructed by means of which, cases, that ordinarily occur in practice, may be readily solved. The task of constructing such a table is necessarily a laborious one. Prof. Robinson was the first to make a table of this kind. It was computed by means of a very ingenious method which reduced the labor to a large extent. However, this method was liable to slight inaccuracies. The table given below was computed by the more laborious, but it is thought more accurate method, of a direct numerical solution of the above equation for each particular case. This table, also, while in some respects more limited than that of Prof. Robinson, is, in others, susceptible of a wider application than the latter. In the first column on the left in Table I. is given the ratio of the steam pressure to the exhaust pressure for each case: in the second column are given the corresponding values of the ratio of the combined volume of the cylinder and clearance to the clearance volume. In the columns to the right of these are given the values of c for the above corresponding pressure and volume ratios, and for the ratio of expansion as given at the head of the column in each case.

In equation (8), if r be made equal to $\frac{p_1}{p_2}$, that is, if the expansion be carried to the exhaust pressure, then $c=r=\frac{p_1}{p_2}$.

Therefore, if the expansion be carried to the exhaust pressure, we should cushion to the initial pressure. This is the greatest possible value that c can have. Rankine's general statement that "the most advantageous adjustment of compression takes place when the quantity of steam confined is just sufficient to fill the clearance at the initial pressure," is true for only one particular case.

In practice it is desirable to know the point in the stroke where cushioning should begin, rather than the value of the ratio c. Having determined the value of c, the fractional part of the stroke through which the exhaust port remains open may be easily found. This fraction represents the part of the return stroke passed through before cushioning begins. The volume of the cylinder displacement at the point of exhaust closure$=v_2-v_1c$, and the cylinder volume proper$=v_2-v_1$; therefore, if we let M denote the fraction of the stroke passed through by the piston up to the point of exhaust closure, we have

$$M=\frac{v_2-cv_1}{v_2-v_1}=\frac{\frac{v_2}{v_1}-c}{\frac{v_2}{v_1}-1} \qquad (9)$$

By means of this formula and the table of values for c, the following table was computed giving the corresponding values of M. It is evident that $1-M$ equals the part of the stroke through which cushioning takes place. Table II. is similar in form to Table I., the value of M being given in the body of the table in place of c.

For cases intermediate between those given in the above table, sufficiently accurate results may be obtained by means of interpolation. The equation and tables already given may be applied to all ordinary steam and air engines. If, from any cause, as "wire drawing" at the point of cut-off, early release, or a more rapid fall of pressure in expansion than the hyperbolic law supposes, it is found impracticable to realize the amount of work from a given volume of steam that we have supposed in our theoretical diagram, cushioning should be made to begin slightly later than the table would indicate. While if, from any cause, the amount of energy derived be greater than that heretofore supposed, the point of exhaust closure should be slightly earlier. Or, in more general terms, any cause that increases the ratio of work divided by volume hastens the exhaust closure, while any cause that diminishes this ratio retards it.

Equation (7) affords a convenient means for comparing the efficiency of steam used under various conditions.

Thus, suppose an engine in which $\frac{v_2}{v_1}=30$, $\frac{p_1}{p_2}=6$, and $r=4$, then, by the first table it will be found that for the most advantageous cushion point $c=5.53$; substituting these values in equation (7), and

TABLE I.

Giving the ratio, c, of the maximum pressure due to cushioning to the exhaust pressure.

$\frac{p_1}{p_3}$	$\frac{v_3}{v_1}$	Value of c for the following values of the ratio of expansion.					
		$r=1$.	$r=\frac{3}{2}$.	$r=2$.	$r=4$.	$r=6$.	$r=8$.
4	10	2.02	2.66	3.16	4.00		
4	20	2.07	2.74	3.24	4.00		
4	30	2.08	2.76	3.26	4.00		
4	40	2.09	2.77	3.27	4.00		
6	10	2.16	2.94	3.59	5.34	6.00	
6	20	2.23	3.06	3.75	5.49	6.00	
6	30	2.26	3.10	3.80	5.53	6.00	
6	40	2.27	3.12	3.82	5.54	6.00	
8	10	2.23	3.08	3.82	5.96	7.32	8.00
8	20	2.32	3.23	4.03	6.32	7.59	8.00
8	30	2.35	3.28	4.10	6.42	7.63	8.00
8	40	2.36	3.33	4.12	6.47	7.67	8.00
10	10	2.28	3.17	3.94	6.28	7.88	9.04
10	20	2.36	3.34	4.20	6.86	8.67	9.63
10	30	2.40	3.40	4.30	7.00	8.72	9.69
10	40	2.42	4.43	4.33	7.05	8.76	9.72
14	10	2.33	3.27	4.09	6.62	8.34	9.53
14	20	2.43	3.46	4.40	7.44	9.69	11.40
14	30	2.47	3.53	4.51	7.70	10.05	11.77
14	40	2.48	3.56	4.56	7.82	10.19	11.92

TABLE II.

Giving the fraction of the stroke passed through by the piston up to the point of exhaust closure for maximum efficiency.

$\frac{p_1}{p_3}$	$\frac{v_3}{v_1}$	Fraction of Stroke for the following values of r.					
		$r=1$.	$r=\frac{3}{2}$.	$r=2$.	$r=4$.	$r=6$.	$r=8$.
4	10	.887	.827	.760	.667		
4	20	.944	.909	.884	.842		
4	30	.963	.939	.922	.895		
4	40	.972	.955	.942	.923		
6	10	.871	.785	.712	.518	.444	
6	20	.935	.892	.856	.764	.737	
6	30	.957	.924	.903	.844	.828	
6	40	.967	.946	.928	.883	.872	
8	10	.863	.769	.687	.449	.298	.222
8	20	.931	.883	.841	.720	.653	.632
8	30	.954	.922	.893	813	.772	.759
8	40	.969	.944	.902	.859	.829	.821
10	10	.858	.759	.673	.413	.236	.107
10	20	.849	.877	.832	.692	.596	.546
10	30	.952	.917	.886	.793	.734	.704
10	40	.964	.938	.915	.845	.801	.776
14	10	.852	.748	.657	.364	.184	.052
14	20	925	.871	.802	.661	.516	.453
14	30	.949	.913	.879	.769	.688	.629
14	40	.962	.934	.909	.825	.764	.720

we find that H=1.709. Table second shows that to attain this value of H we should commence to cushion at .844 of the return stroke. Suppose that, under otherwise the same conditions as before, we delay to begin cushioning until .931 of the return stroke is accomplished, c will then equal 3, and H will be found to

be 1.685, which is 1.4 per cent. less than in the first case.

Again, suppose a condensing engine in which $\frac{p_1}{p_2}=14, \frac{v_2}{v_1}=40$, and $r=10$, then, for the best working, $c=11.92$, and the point for beginning compression is .720 of the return stroke. Under these conditions, H=2.4783. Now, let the compression point be varied so that the back pressure at the end of the return stroke shall be four times what it was during exhaust, or, in other words, so that $c=4$, then we find that H=2.4245, which is more than two per cent. less than in the preceding case. Again, suppose an engine in which $\frac{v_2}{v_1}=10, \frac{p_1}{p_2}=6$, and $r=\frac{5}{2}$, then we get 2.7 per cent. more work from the same quantity of steam by closing the exhaust at .785 of the return stroke, the most advantageous point, than we would, should we cushion so as to carry the pressure to the initial.

In order to show the effect of clearance on the efficiency of engines having the exhaust closure properly regulated, the following cases have been assumed, and the value of H worked out for each case. The ratios of pressure and of expansion are the same in each instance, viz., $\frac{p_1}{p_2}=6$ and $r=4$. The ratio $\frac{v_2}{v_1}$ is different for each case, ranging from ten, in the first example, up to infinity in the fifth, where there is no clearance whatever. The values of c and H are given in the second and third columns respectively.

$\frac{v_2}{v_1}$	c.	H.
10	5.34	1.660
20	5.49	1.703
30	5.53	1.709
40	5.54	1.716
No clearance		1.720

The value of H in the fifth case is less than four per cent. greater than in the first. Excluding the value of H in the first instance, there is in no case a difference of one per cent. between any two of the remaining values. The last result shows the slight amount gained by dispensing with clearance entirely.

These examples might be extended much further, but enough has been done to show the use of the formula, and to illustrate how it may be applied to particular cases as they occur.

MEASUREMENT OF FLOW OF WATER IN DITCHES.

By H. N. PIERCE, '85, School of Mines, Columbia College.

Written for VAN NOSTRAND'S MAGAZINE.

THERE appeared in the January number of the Magazine, a valuable paper on the above subject, by Mr. Aug. J. Bowie, Jr.

After a discussion of the "Miner's inch," the author takes up "Flow of water in open channels," and after stating that there is no generally accepted formula for determining the velocity of water in open channels—the older formulæ being based on data which ignore the important factor of the nature of the bed and sides of the channel,—he states that hydraulic engineers are compelled to rely for the correctness of *calculated* results, on a few known laws, in combination with experimental data, which latter, though all important, are restricted, and therefore of uncertain application.

Adopting the Chezy formula for mean velocity

$$v=c\sqrt{rs}$$

he naturally premises

$$Q=ac\sqrt{rs}$$

notating his symbols as follows:—

Q=quantity in cubic feet per second, discharged,

a=effective area of cross-section in sq. ft.

r=hydraulic mean depth in feet,

s=fall of surface in unit of length,

c=coefficient covering all common losses.

It is the object of the rest of the paper to establish by an analysis of the larger ditches on the Pacific coast, a mean value—or approaching limits of value—

for the experimental coefficient, c, and after considering all losses, Mr. Bowie adopted a value ranging from 31 to 45. a

In the April number of the Magazine (page 289) under the same title, is paper by Mr. Chas. E. Emery, Ph. D.

It is a criticism of Mr. Bowie's paper in the January number, and maintains that he (Mr. Bowie) has used r as the average depth of the stream, and *not* as the hydraulic mean depth, in the application of his formula

$$Q = a\,c\,\sqrt{rs}.$$

Mr. Emery then proceeds to apply the formula as he thinks it should be, and obtains values for c, averaging nearly three times as high as those of Mr. Bowie, from which he corrects the final formula to

$$Q = 109 \text{ @ } 116.5\ a\ \sqrt{r\,s}$$

in place of Mr. Bowie's calculation

$$Q = 31 \text{ @ } 45\ a\ \sqrt{r\,s}.$$

Let us take the Texas Creek branch ditch and its flume. It is the only case where Mr. Bowie has noted the discharge per second, the cross-section area of the flow, and the fall per unit of length.

When only the area is given the form of the channel is of some doubt, but we are quite safe in assuming that the sides of the channel slope at 60°.

It is a well-known fact that where the form of a channel is the half of any regular polygon and flowing full, the hydraulic mean depth is one-half the greatest depth, but under no other conditions. If the slopes were carried up at the same angle, the depth might vary from two to four feet, but the hydraulic mean depth would remain practically constant.

Mr. Bowie, however, says that the ditch was running about full, and taking this to mean that the section is a half hexagon, we have for the hydraulic mean depth 1.4 feet, which substituted in the formula gives a value of $c=33.2$.

For the flume in connection with this ditch, the value of r is known with certainty to be .9 feet, giving a value of $c=58.7$.

If these results are taken to the nearest whole number, they agree exactly with Mr. Bowie's figures and are perhaps sufficient to show how seriously Mr. Emery is in error.

Students of the School of Mines take a lively interest in discussions of this character, as it gives us an opportunity (by following out the investigations) to practically test the knowledge we are gaining at the School.

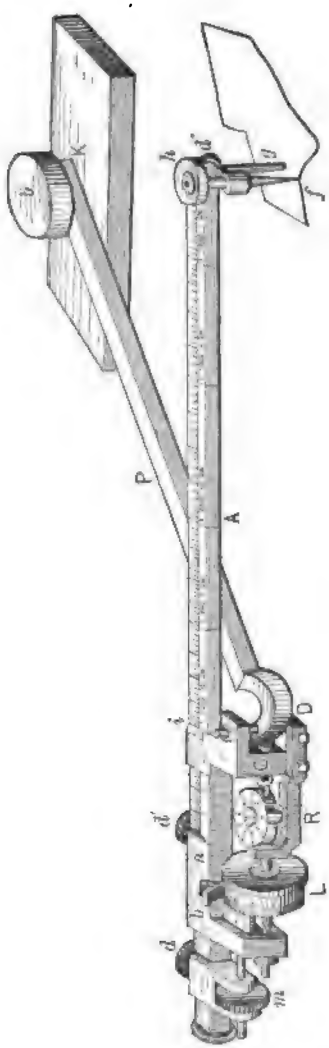
DIRECTIONS AS TO THE USE AND A THOROUGH TESTING OF CORADI'S PLANIMETER.

Translated by J. S. ELLIOTT.

1. Before using your instrument, be certain that it is in good order. The graduated "roller" L must have a barely perceptible play in its bearings. The vernier should not touch the "roller" graduation, and it should not be so far from the same that a good reading becomes difficult.

It is well to run, at times, a thin piece of letter paper between the two, so as to get rid of any dust that may have lodged between them. The register wheel R must revolve easily, and have sufficient play with the worm of the "roller." The axis D of the pole arm P should also turn easily, but on no account should it have the slightest play in its bearings. Make certain of this by placing pole arm and movable arm A parallel, and then try to move the pole arm up and down; further than a certain flexibility, no motion should take place. Both arms, as well as the pencil, should be protected from any bending. Great attention is to be given to the conservation of the "roller" rim, as it is the most important part of the instrument. Avoid all contact of the same with the naked fingers on account of rust specks. If fingered, it should be carefully wiped off in the direction of the axis with a piece of chamois leather. Remove at intervals the old clotted oil from the bearings. To do this, and at the same time avoid taking the instru-

ment to pieces, prop the frame so that the "roller" swings freely, and draw around the bearing of the axis a fine linen thread dipped in petroleum, then use on the bearings a sharpened splinter dipped in the best oil.



2. To find the area of some figure to a certain scale, say 1 : 1200, shove the movable arm in its socket, until it is easy, by means of the micrometer screw *m* to set the vernier *n* at the number (220.6 in this case) found in the table pasted in the instrument box; then clamp the socket with the screw *d'*. The desired areas should be stretched on a smooth drawing board, with no inequalities to obstruct the running of the "roller."

3. Place now the pole slab K near the desired area, and the pole ball in its proper receptacle; a hasty circuit should convince you, that for the chosen position of the pole there will be no obstruction.

4. Place the pencil on an easily distinguished point of the circumference, and to reduce to a minimum the tracing errors, it is well to so choose the starting point that the two arms are about at right angles to each other. You can now either take a note of the reading of register wheel, "roller" and vernier, or by slightly shifting the pole slab, bring the zero points of the "roller" and vernier in line. Let, for instance, the register wheel show 3 and the roller graduation be at zero.

5. Trace carefully around the circumference in the direction of the hands of a watch, until you regain the starting point. The instrument rotates around the "pole," whilst the "roller" acts now with a sliding, then with a back and forward rolling motion. It is well to use a straight edge on all straight lines of the circumference; it does, however, sometimes lead to positive or negative tracing errors, whilst when done freehand, the alternating positive and negative errors to some extent cancel each other. Furthermore, when using a straight edge, care must be taken to have no lateral pressure on the pencil button *h*, for it would result in a tracing error by reason of the flexible movable arm changing its position with regard to the pencil point.

6. The circuit made, take your reading. The vernier reads to tenths, and one complete revolution of the "roller," and one division of the register wheel, are each worth 1,000 vernier units.

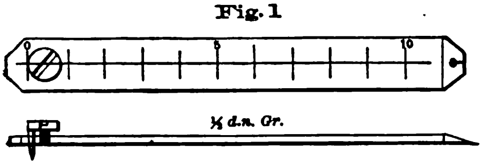
Suppose that the register wheel reads between the 4th and 5th division and the "roller" graduation reads 137. The "roller" has then moved forward by $1000+137=1137$ vernier units. Multiplying this by 10 sq. ms., the value of the vernier unit, the desired area is 11,370 sq. ms. If, before making the circuit, the graduation has not been set at zero, a reading should be taken and subtracted from the second reading, and the difference multiplied by the value of the vernier unit.

7. With large areas whose circuits cannot be traced with one "set up" with the pole slab outside, place the pole slab in the middle, and trace the circuit *against* the hands of a watch.

In this case it is advantageous to set both "roller" and register wheel on zero before making the circuit, as thus, errors are sooner eliminated. The resulting reading must be subtracted from a constant quantity found on the movable arm or in the table, and the difference multiplied by the value of the vernier unit. You wish, for instance, to find the area in feet of some plat: your reading is 1832; subtract this from constant quantity 20,555, and multiply difference by 0.0001 sq. ft., giving $1.\frac{8723}{10000}$ sq. ft. If during the circuit, the register wheel has made one complete revolution forward, 10,000 must be added to the reading.

With large areas, where the angle made by pole slab, roller and pencil point is constantly over 90°, the register wheel will move backwards; in such a case, subtract the reading from 10,000, add remainder to the constant quantity, and the sum of the two multiplied by the vernier unit value will give the desired area.

Fig. 1



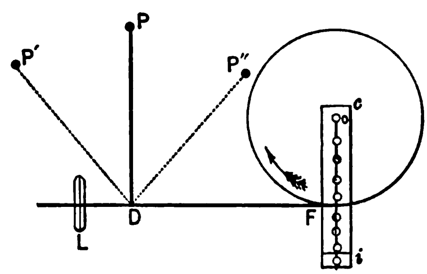
Many tracings of some figure are necessary for a scientific testing of the instrument, and as all tracing errors should be eliminated, mechanical devices may be used with great advantage. Among these the so-called "check-ruler," Fig. 1, deserves especial notice.

It is a small brass scale with ten 1 centimeter divisions. At the intersection of the zero division with the line, the length of the scale is stuck a needle through a small hole; at the other intersections are small indentations to hold the pencil point of the planimeter. One end of the "ruler" is beveled and has an index line to locate the starting point of one of the test circles, shown as it is in the test circle by a produced radius. The use of this device is best understood from Fig. 2.

The "ruler," in fact, is to be used as a means of tracing a perfect circle. Setting the pencil point in any one indentation, means tracing a circle whose area is a function of the distance between said indentation and the needle. The needle stuck in the paper acts as a pivot. As to the objections that the circle is too favorable a figure for the testing of the planimeter, it may be replied that if the examination includes all the requirements given further on, and has stood them all well (a difference of readings of 2 to 2½ vernier units being regarded as permissible), the instrument will give a good and trustworthy value of any and all figures, bounded as they may be, *i. e.*, within the given limits of error.

It is held that mechanical devices eliminate all error. This is altogether wrong, as shown by numerous experiments. If, for instance, when using the check-ruler

Fig. 2



as a radius, the pressure on the pencil is not tangential, a certain bending of the movable arm takes place, although the pencil point cannot leave the periphery. The result is a tracing error of some consequence.

It is therefore recommended to lightly load the pencil, as well as the center of rotation of the "check-ruler," and to guide only with the "check-ruler."

The planimeter should be tested for the following requirements in the given order:

1. The instrument should be in general good order.

2. The graduation of the "roller" should be accurate and concentric.

3. With repeated tracings the readings should be the same, and the graduation should be concentric with the rim of the "roller."

4. The readings for different posi-

Scale Proportions.	Value of the Vernier-Unity.	Readings for one Circuit of the Circles with Radii of from 1 cm. to 10 cm.									
		1 cm.	2 cm.	3 cm.	4 cm.	5 cm.	6 cm.	7 cm.	8 cm.	9 cm.	10 cm.
1 : 500 1 : 1000 1 : 2000 1 : 4000	2.5 10. 40. 160.	0,03,14	0,12,56	0,28,27	0,50,26	0,78,54	1,13,097	1,53,93	2,01,06	2,54,47	3,14,16
1 : 500 1 : 1000 1 : 2500 1 : 5000	2. 8. 50. 200.	0,03,92	0,15,7	0,35,33	0,62,83	0,98,17	1,41,37	1,92,41	2,51,33	3,18,09	3,92,70
1 : 625 1 : 1250 1 : 2500 1 : 5000	2.5 10. 40. 160.	0,04,9	0,19,63	0,44,18	0,78,54	1,22,71	1,76,71	2,40,51	3,14,16	3,97,61	4,90,85
1 : 750 1 : 1500 1 : 3000	5. 20. 80.	0,03,53	0,14,13	0,31,80	0,56,54	0,88,35	1,27,23	1,73,17	2,26,18	2,86,27	3,53,43
1 : 750 1 : 1500 1 : 3000	2.5 10. 40.	0,07,06	0,28,26	0,63,61	1,13,09	1,76,71	2,54,47	3,46,34	4,52,36	5,72,54	7,06,84
1 : 1000 1 : 2000 1 : 4000	5. 20. 80.	0,06,28	0,25,13	0,56,54	1,00,53	1,57,08	2,26,19	3,07,86	4,02,12	5,08,89	6,28,32
1 : 500 1 : 1000 1 : 2500 1 : 5000	1. 4. 25. 100.	0,07,85	0,31,41	0,70,68	1,25,66	1,96,34	2,82,74	3,84,82	5,02,66	6,36,18	7,85,40
1 : 1250 1 : 2500 1 : 5000	5. 20. 80.	0,09,81	0,39 27	0,88,35	1,57,08	2,45,43	3,53,42	4,81,02	6,28,32	7,95,19	9,81,75

1 : 2500	10.	0,19,63	0,78,54	1,76,71	3,14,6	4,90,85	7,06,84	9,62,05	12,56,64	15,90,39	19,63,50
1 : 1000 1 : 2500 1 : 5000	2. 12.5 50.	0,15,70	0,62,83	1,41,37	2,51,33	3,92,70	5,65,48	7,69,65	10,05,31	12,72,35	15,70,8
1 : 1440	20.	0,06,51	0,26,06	0,58,62	1,04,24	1,62,86	2,34,50	3,19,18	4,16,96	5,27,63	6,51,44
1 : 2730	40.	0,05,85	0,23,41	0,52,67	0,93,64	1,46,39	2,10,69	2,86,82	3,74,57	4,74,03	5,85,35
1 : 6250	200.	0,06,13	0,24,54	0,55,22	0,98,17	1,53,36	2,20,88	3,00,63	3,92,7	4,96,98	6,13,56
1 : 1820	20.	0,05,20	0,20,81	0,46,82	0,83,24	1,30,06	1,87,28	2,54,95	3,32,96	4,21,38	5,20,31
1 : 2400	40.	0,04,52	0,18,09	0,40,71	9,72,38	1,13,09	1,62,86	2,21,66	2,89,52	3,66,43	4,52,39

This table gives the values of the vernier units (reduction factors) for the most commonly used scales, as well as the readings, got by tracing around the ten circles, whose radii are given in the "check rules."

tions of the pole slab should be constant.

5. The readings when tracing a figure both ways should be constant.

6. The given value of the vernier unit should be right for the adjusted length of the movable arm.

7. The given constant quantities should be right.

For. 1. See what has been previously said.

For. 2. Test the vernier at every 5th division of the "roller" graduation. If the tests are satisfactory, it may be taken for granted that the graduation is accurate as well as concentric.

For. 3. Choose for this important requirement the greatest length of the movable arm 1 : 1000, and place the instrument in the position shown in Fig. 2 (the two arms at right angles with each other). Take now some radius on the "check-ruler," giving a circle whose repeated tracings will eventually bring every 2d or 3d division in the readings. Chosen, for instance, 6 cms. as radius, you will get 1130.9 as reading. Trace this circle some 60 times, until every other division line has come into the readings. The difference between greatest and least readings should not exceed 2½ vernier units.

If the readings starting from one point, first increase and then decrease in such a way that the least and greatest readings are some 50 "roller" divisions apart, we have a proof that the "roller" rim and graduation are not concentric. If, however, the errors show up irregularly, it points to a malformation of the "roller" rim. These two errors can be removed only in the workshop.

For. 4. Take from the "check-ruler" a radius giving an average-sized circle, and trace it with the pole slab in the 3 positions given in Fig. 2, P P′ and P″. If the reading gives for the nearest pole-slab position too great an area, the right end of the roller axis must be brought nearer to the movable arm by turning the cylinder C, and in the opposite case, should be moved away by the same means, seeing carefully to it in both cases that the play of the register wheel in the worm becomes neither too great nor too small.

Test No. 4 should be repeated for the various lengths of the movable arm; a

deviation for any length shows that the movable arm has been bent, and the error should be removed the same way it arose. It is best for this purpose to begin with the shortest value of the movable arm; set the "roller" axis parallel by turning the cylinder C, so that the readings will be equal for different positions of the pole slab. Test now for the next length of the movable arm, and try to get the same result by carefully binding the movable arm. If your reading is too great for the nearest position of the pole slab, you must bend the right end of movable arm away from the pole slab. With any kind of careful handling, though, a bending of the movable arm should not occur, made as it is of cold-drawn German silver.

For. 5. This error occurs with planimeters whose "rollers" are fixed, and whose pole arm axes are detachable, and is owing to too much play of the "roller" axis, and to a malformation of the "roller" rim. Test for it by taking the longest radius (10 cms.) of the "check-ruler," and trace a complete circle both ways. Both readings should be equal. This error is especially injurious when tracing long narrow figures.

For. 6. First examine the "check-ruler" to see that the graduation is right, and that the indentation as well as the center of rotation are exactly at the intersection of the divisions with the longitudinal line; see also that the needle is perpendicular. Now calculate the areas of the 10 circles traceable with the "check-ruler," and be sure that the resulting readings check with your calculations. Lengthen or shorten your movable arm according as your readings are too great or too small. At the same time the following test may be attempted—a favorable result meaning that the instrument may be used for definite work. Draw accurately a square of known area as large as the planimeter can trace around. Trace the same with different positions of the instrument, and convince yourself that the readings check with the computed area. Divide the square into 2 equal triangles, and trace each with different positions of the instrument; the readings should be equal and their sum equal to reading of the whole. Further subdivisions may also be attempted. This test serves also to determine the individual mean tracing error.

For. 7. For this test use a small mechanism sent with the instrument on request. It fixes movable arm and pole arm at any angle you please, so that circles of any size may be described around the pole slab. Place the pole slab on a line representing the diameter of various sized circles with the pole vertically over the line. Lay off on this line with the pencil point the different diameters, measure them and compute the areas of circles they represent. Let A=computed area of any one circle, and V=value of vernier units, then $\frac{A}{V}$ + reading got by tracing the same circle= constant quantity.

MEASUREMENT AND FLOW OF WATER IN DITCHES.

By CHAS. E. EMERY, Ph.D.

Written for Van Nostrand's Engineering Magazine.

Referring to the article on the above subject, by Mr. Bowie, in the January number of the Magazine, I will state that my note in the April number stands corrected by that of Mr. Browne, in the May number. It appeared clear on the first reading of Mr. Bowie's paper that the expression in regard to the depth of the La Grange ditch, at top of p. 35 of the January number, admitted of but one construction, and a constant was worked backward from the data given, on that basis, but a second reading of the paper, in connection with Mr. Browne's note, shows the intention of the writer.

THE STATE OF TOWN DRAINAGE IN RUSSIA.

By A. v. ABRAMSON.

Translated from "Gesundheits-Ingenieur" for Institution of Civil Engineers.

The question of drainage has hitherto received but little attention in Russia. The principal towns are none of them thickly built over, and only the largest of them — St. Petersburg, Moscow, Odessa and Kiev—can lay claim to a regular water-supply; while with the single exception of Odessa, which is in many respects a model Russian town, they are all ill-paved and badly provided with water.

This matter first attracted notice in respect to St. Petersburg, in which city the mortality is appaling. After a considerable portion of the town had been supplied with Neva water, and when water-closets had been almost universally employed, the evils of cesspools, which had to be emptied by casks carted daily through the streets to the suburbs, became glaringly manifest. Owing to its low-lying marshy situation, and the proximity of the subsoil water to the surface, the question of sewer construction is here one of great difficulty, and the pneumatic system appeared to present important advantages. Very satisfactory trials of Captain Liernur's plan have been carried out, at the experimental station on the Semjonow Square, and this system, in spite of the complaints made respecting it in certain towns in Holland, has many advocates. Bourow, the Russian engineer, is now trying another pneumatic system, which carries away their contents direct from the cesspools by means of a vacuum created in a system of pipes with which the cesspools are placed in connection. His investigations, however, are not yet completed, and the question, so far as St. Petersburg is concerned is still in the experimental stage.

Moscow, on the other hand, has pronounced in favor of the water-carriage system; all the preliminary matters have been arranged, and it is understood that a comprehensive plan of drainage prepared by Herr Hobrecht has been accepted. The execution of the work has been delayed by the proposal of the Governor-General, Prince Dolgoroukou, that the Government should undertake its direction, as a work of such high importance, and that for this purpose a representative committee would be appointed from the town authorities, the Zemstwo, the Government, and the Society of Engineers.

Odessa also enjoys the advantage of a water-carriage system, which is not yet so complete as it might be in the outlying suburbs. Householders are not compelled to connect with the sewers; each one is free to do as he chooses; and a further defect of the scheme of drainage is that the main outfall discharges into the center of the harbor, close to the public bathing-establishments.

In Kiev, the oldest town in Russia, this matter has come to the front from the discovery that, in spite of its naturally healthy position, on the summit of lofty hills, below which flows a large and rapid river, the Dneiper, the death-rate here averages thirty-eight per thousand. A commission of engineers and physicians was appointed to inquire into this subject in 1879, and in their report they attributed the above state of things to the insanitary condition of the town, and recommended the immediate adoption of the water-carriage system, at an estimated cost of 2½ millions of roubles (about £400,000), or from £2 10s. to £3 per head of the population. Their views were adopted, and the necessary preliminary works were ordered.

There is a law in Russia which prohibits the pollution of public water-courses, and the Kiev authorities are endeavoring to obtain permission to discharge the sewage at a sufficient distance from the town, into the river Dneiper, which at the point proposed has a velocity of 0.75 meter (29.5 inches) per second, and a flow of over 200 cubic meters (44,000 gallons) per second. To take the water to a point where it could be profitably employed in irrigation, would necessitate a lift of 80 meters (262.4 feet), and would so increase the

expense as to render this method of drainage impracticable: the success of the scheme depends, therefore, on the permission being granted to drain into the river.

At Warsaw a water-carriage system, designed by Mr. W. Lindley, M. Inst. C. E., is well advanced towards completion; and though the policy of taking all the sewage into such a small stream as the Vistula has been disputed, the town will shortly be in possession of a thorough system of drainage.

In addition to the towns already mentioned, the subject is receiving attention in several places of lesser importance, as, for instance, Charkow, though there the matter has scarcely got beyond the phase of discussion, at the meetings of the town council.

This momentous question is, however, being widely considered, and efforts are being made to extend the knowledge of its importance throughout Russia. As one who has taken the lead in drawing attention to the subject, the author follows with careful interest all that is taking place in neighboring countries.

SPONTANEOUS DECOMPOSITION OF EXPLOSIVE GELATINE.

By CHARLES E. MUNROE, S.B. (Harv.)

From the Journal of the American Chemical Society.

Several instances of the decomposition of explosive gelatine on keeping, or after long exposure to moderate temperatures, have been reported, but I have yet met with but one of these cases in which the products of the decomposition have been stated. Gen. H. L. Abbot, in a prefatory note to Addendum I, Report on Submarine Mines, states that "all the samples of the explosive gelatine remaining on hand after the trials detailed in the Report, have undergone spontaneous decomposition, separating into cellulose and free nitro-glycerine with the copious evolutions of nitrous fumes. This change occurred during the winter and spring of the current year (1881–1882), and was not caused by any exposure to high temperatures while in store."

A case of spontaneous decomposition of a small amount stored freely exposed to the air, in a room of fairly even temperature and dryness, has occurred under my own observation. The camphorated explosive gelatine was wrapped in paraffine paper, and then in light brown Manilla paper, and laid on the shelf. After something more than one year's exposure it was found, in the early winter, to be giving off nitrous fumes (which had stained and attacked the wrapping paper), and to have shrunk considerably in volume, while the outside of the paper was covered with congeries of fine crystals. The odor of camphor was still quite strong. The mass was immediately put into a vessel of water. It was found to be friable, and, after a short immersion, disintegrated. The camphor odor soon disappeared, and the water became of a straw color, gave a strong acid reaction, and showed traces of nitrous acid, but no nitric acid. On evaporation of the filtered liquid, oxalic acid crystallized out in quantity, and on evaporation of the "mother liquor" on the water bath, a sugar-like mass was obtained, which gave the glucose reaction with Fehling's solution. The paraffine was regained unchanged, and the paper was recovered, but in a flocculent condition, and with the color bleached from the brown. Careful search failed to reveal the presence of glycerine, nitro-glycerine, or gun-cotton.

The cellulose from the gun-cotton could not well be detected (if it existed) in the presence of so much flocculent cellulose from the paper. In reporting these observations I am not unmindful of the fact that some changes may have taken place during immersion, but it can easily be understood why I preferred it in that position.

The results obtained by de Luca in his "Researches on the Spontaneous Decomposition of Gun-Cotton," *Comptes Rendus* 59,487, *Sept.* 12, 1847, are inter-

esting in this connection. Gun-cotton decomposes most rapidly when heated to 50° on a water bath, next by direct sunlight, more slowly by diffused light, and very slowly in darkness. The gun-cotton first shrinks to one-tenth of its original volume, next it begins to become gum-like and sticky, then it swells; during all these phases it gives off nitrous fumes, but especially during the last. For the fourth phase the gas ceases to be evolved, and the mass becomes brittle, and of a light color like sugar. The products are nitrous compounds, with formic and acetic acids in the state of a gas, and an amorphous, porous, sugar-like body, almost entirely soluble in water, and containing an abundance of glucose, gummy matter, oxalic acid, a small quantity of formic acid, and a new acid of which he obtained the lead and silver salts for later examination. From 100 grains of gun-cotton he obtained about 14 grains of glucose.

In discussing the stability of nitro-glycerine (which is the other component of explosive gelatine), A. Brull, in *Etudes sur la Nitro-glycerine et la Dynamite*, *Fig.* 26, 1875, says: "Nitro-glycerine which retains traces of acid is not stable. In general the decomposition is extremely slow and tranquil. It disengages at first nitrous fumes, and the liquid takes a greenish color; then it generates nitrogen protoxide, carbon dioxide and crystals of oxalic acid, and after some months the entire mass is found to be converted into a greenish, gelatinous mass, composed of oxalic acid, water and ammonia. Sometimes, if the temperature is quite high—if, for example, the nitro-glycerine is heated by the sun, the decomposition is more active. Very rarely it causes an explosion."

The source of difficulty, then, seems to be in the presence of free acid, and this will probably be found in the gun-cotton used, for it is difficult to purify soluble gun-cotton completely.

THE FLOW THROUGH SUBMERGED OUTLETS.

BY ALFRED SALLES.

Translated from "Annales des Ponts et Chaussées."

THE delivery through submerged sluices, those in which the sill is below the water on the down stream side, may be calculated by the aid of two formulas; one of them due to M. Mary, and the other to M. Lesbros. The writer has had occasion to apply them in case of the Bazacle Dam upon the Garonne at Toulouse, and the object of the present note is to record the results obtained.

In 1860 M. Mary published in his *Cours de Navigation Interieure* a formula which may be written

$$Q=m\,L\,H'\sqrt{2g(H-H'+h)}$$

in which L is the length of the sluice, H the height of the water on the up stream side above the sill of the sluice, H′ the height of the surface on the down stream side above the same level, and h is the height due to the velocity of the current on the up-stream side. Of the coefficient m the author says, although the value of m has not been determined by any known experiments, we may assume it to be about 0.8.

According to the accounts collected by the *Service des inundations*, notably by Messrs. Lanteires, Endres and Gros, the flow on June 23d, 1875, at the Bazacle Dam was from 6,000 to 7,000 cubic meters per second. It had been determined by the delivery of the Garonne above, and they found fH a value of $5^m.95$ to 6 meters, and for H′ a value of 5 meters. In adopting the mean value of 6,500 cubic meters for Q the writer finds that the velocity of the river above should be $3^m.40$ per second, which gives for the value of $h=0^m.59$. The length of the dam is 283 meters. This for the value of m

$$m=\frac{6500}{283\times5\sqrt{2g\,(6-5+0.59)}}=0.82$$

which will accord with the estimate of M. Mary.

Taking Q=7000 cubic meters, we get for the velocity u=3.6 and for $h=0^m.66$.

The corresponding value of m is 0.87. If Q=6000 cubic meters, u becomes =3.20 and $h=0^m.52$. The value of m then falls to 0.77

M. Lesbros' formula is

$$Q=m'\,L\,H\,\sqrt{2g\,(H-H')}$$

H is the height of water level in the upstream side measured above the sill of the sluice and measured to the surface of still water. H' is the height of the water level down stream above the sill of the sluice, but measured to the lowest point of the hollow of the liquid vein. m' is a coefficient varying between 0.430 0.605 according to the value of $\frac{H-H'}{H}$.

The above considerations indicate that the formula of M. Lesbros, although serviceable for sluices for water wheels and irrigation canals, should not be applied without examination to the submerged dams of rivers. The heights of 6 meters and 5 meters realized at Toulouse during the inundation of 1875, doubless fail to answer to the letter of the prescribed conditions, but they nevertheless deviate but little, and for

$$\frac{H-H'}{H}=\frac{6-5}{6}=\frac{1}{6}$$

the table of M. Lesbros gives a value of $m'=0.511$. If this were the true value, the quantity delivered at the Bazacle Dam would have been only

$$0.511\times 283\times 6\times\sqrt{2g\,(6-5)}=3843$$

cubic meters instead of 6000 or 7000.

In order to obtain a mean delivery of 6500 cubic meters m' should have a value of

$$\frac{6500}{283\times 6\sqrt{2g\,(6-5)}}=0.86$$

which is not far from the value determined above for m for the same delivery.

The writer believes that the formulas of Mary and Lesbros should be applied to the submerged outlets with the coefficient of about 0.80, conformably to the estimate of M. Mary, whose formula, perhaps too often forgotten at the present day, has been meanwhile confirmed by the experiments at the Bazacle Dam during the great flood of the Garonne in June, 1875.

THE SCOPE AND METHOD OF PETROGRAPHY.*

By J. J. H. TEALE, M.A., F.G.S.

From "Nature."

In considering the history of geology we are struck by the fact that towards the close of the last and during the commencement of the present century, when the science was taking rank as an important branch of human knowledge, petrography occupied a much higher position than it has at any subsequent period.

Werner, whose influence was almost unrivaled at the time to which I have referred, was a mineralogist, and his formations were therefore naturally based on the mineralogical characters of the different rocks. His observations were limited for the most part to his own district of Saxony, but he regarded his formations as sediments or precipitates from a universal ocean, and his numerous pupils, fired by his love of science and his intense enthusiasm, rejoiced in extending his classification to the districts with which they were severally acquainted.

The magnificent work of those who devoted special attention to the organic remains in the sedimentary deposits, and especially that of William Smith, the "Father of British Geology," had the effect of deposing petrography from the position which it held under the influence of Werner and his followers. It was clearly shown that the fossil contents of the strata were far more reliable as evidence of chronological relations than their lithological characters, and as soon as this became generally recognized, the reduction of the fossil-bearing rocks all over the world to something like definite order followed as a natural consequence.

The principle that strata may be identified by means of their fossil contents has

* Lecture delivered in the Woodwardian Museum, Cambridge.

not only proved applicable to the Secondary and Tertiary formations to which it was originally applied by Smith, Cuvier, and others, but it has been extended by Murchison, Sedgwick, Barrande, and others to the older rocks. Speaking broadly, there can be no doubt that over large areas the succession of the forms of marine life has been remarkably uniform from the Cambrian times down to the present, so that we have in the fossil contents of the different strata by far the most reliable means of determining chronological relations.

It is not surprising, then, that petrography has been comparatively neglected by geologists, for their main object during the present century has been to classify the stratified rocks which form so large a portion of the existing land surfaces.

At the present time, however, we are witnessing a great revival of interest in petrography, not only in this country, but all over the globe. This is due in part, no doubt, to the introduction of new methods of research; but it seems to me that there are other and more general causes. The clear recognition of the great principle with which the name of William Smith is so indissolubly united at once made it possible for a host of observers to do excellent work in every quarter of the globe. The interest awakened by the study of the geological structure of the most densely populated regions was akin to that which is felt by the geographical explorer of unknown lands. Until the main features of the geology of fossiliferous regions were described, it was not to be expected that observers would turn aside from a field of research in which they were certain to meet with success for the purpose of attacking problems which, after all, might prove to be insoluble. As time went on, the unexplored tracts in which fossiliferous rocks occur became more and more restricted, or more and more inaccessible, and when the old chaos of Grauwacke fell into order before the brilliant researches of Sedgwick, Murchison, and Barrande, geologists were placed in an entirely new position. They had conquered that portion of the world which was open to their special method of attack. A number of fortresses still held out, it is true, and many of these remain unsubdued at the present day. They will doubtless occupy the attention of those who are most skilled in the old methods of warfare for many years to come. At the same time I think it will be admitted on all hands that the brilliant successes of the old generals have left a large portion of the army with little to do. We must, therefore, look for other worlds to conquer.

Now, on taking a general survey of the subject-matter of geology it will be seen at once that we are profoundly ignorant on questions relating to the origin and sequence of volcanic rocks, the cause or causes of volcanic action, the mode of formation of the crystalline schists, and the origin of mountains. That these questions are really unsolved is proved by the difference of opinion which exists between competent observers. Another point which strikes one is, that if a solution of these problems be ever realized, it will be due in a great measure to the combination of field geology and petrography. This, it seems to me, will explain the great interest which is taken in the latter branch of science at the present day. If I am right in my opinion as to the present state of things, then we may safely predict that petrography will occupy as prominent a position in the immediate future of geology as palæontology has done in the past. In making this statement I trust it will not be thought that I am claiming too high a position for that branch of geology with which I am most intimately acquainted.

Let us turn now to a more detailed consideration of the scope and method of petrography. The rocks of the earth's crust form the subject-matter of the science. Now, these may be studied from two more or less distinct points of view—the descriptive and the ætiological. We may set to work to describe the rocks, that is, to ascertain and record every possible fact with regard to them; or we may endeavor to trace the succession of events which has culminated in the state of things which we actually observe. It is perfectly obvious that we cannot hope to attain any considerable success in the second branch of the subject until we have devoted a considerable amount of attention to the first.

Descriptive petrography then concerns itself with the chemical, mineralogical, and physical character of the individual rocks, and also with the distribution and mu-

tual relations of the different varieties. The last-mentioned branch of the subject occupies the same position in petrography as comparative anatomy does in zoology. It may therefore be termed comparative petrography.

When the history of the science comes to be written, it will be recognized that it is to the Germans we are especially indebted for our knowledge of descriptive petrography. The amount of work which has been done in Germany is immeasurably greater than that produced by other nations. For years past they have been steadily improving their methods of observation, as well as observing and recording facts. Moreover, they have been training petrographers who are now scattered all over the world. The Americans especially have availed themselves of the laboratories of Rosenbusch and Zirkel, and almost every Annual Report of the American Survey now bears witness to the influence of Germany, from a teaching point of view, on the growth of petrographical science. In this sketch, of course, I am only calling attention to the broad facts of history as far as regards the special branch of descriptive petrography. Many observers in France, England, and America have done independent work of the very highest order, and to England especially belongs the credit of having, in the person of Sorby, determined to a very large extent the introduction of the modern methods of microscopical research.

Consider now what is involved in the description of any particular rock, and take, for example, a specimen of the Whin Sill, that mass of basic igneous rock which plays such an important part in the Carboniferous region of the North of England.

The rock is dark gray or bluish-gray when freshly exposed. In texture it varies from compact to coarsely crystalline, the most common variety being one in which the individual constituents are just recognizable by the naked eye. Its specific gravity varies from 2.90 to 2.95. Its chemical composition is shown on this table. (Table referred to.) We have now to consider its mineralogical composition. In the determination of minerals in rocks we use physical and chemical methods. Color, general appearance, hardness, cleavages, specific gravity, crystalline form, fusibility, and flame coloration are some of the most important physical properties available for the determination of minerals in rocks when they can be examined microscopically. In thin sections we can use color, general appearance, cleavages, form, and also the many properties which are brought out by the use of parallel and convergent rays of polarized light. Chemical tests may be applied both to microscopically recognizable minerals and also to those which can only be observed by the use of thin sections or minute particles and the microscope. The latter are generally referred to as micro-chemical tests.

By applying these methods, some of which will be more fully explained in the subsequent lectures, we can prove that the rock of the Whin Sill is composed of felspar, pyroxene, titaniferous magnetic iron ore, quartz in the form of grains and also as a constituent of micro-pegmatite, apatite, pyrite, brown hornblende, mica, and some green decomposition products. Apatite, pyrite, hornblende, and mica occur only in very small quantity, and cannot be said to form any appreciable portion of the rock.

In order to give a complete petrographical description, however, it is necessary that we should not only know what minerals are present, but also that we should know the precise composition of each and the relative abundance of the different species. Information on these points can only be obtained by isolating the different constituents of a rock and analyzing them separately. Methods of isolation will be described in subsequent lectures. The most important are those which depend on the use of heavy solutions, the magnet and electro-magnet, and various chemical re-agents, especially hydrochloric and hydrofluoric acids. The chemical composition of each of the three principal constituents of the Whin Sill is represented on these tables. (Tables referred to.) Now, having obtained a knowledge of the composition of the principal constituents of the rock, it becomes possible to determine with a very fair amount of accuracy the relative proportions of these constituents by calculations based on the bulk analysis of the rock.

There is yet another point of great importance to which attention should be paid in subjecting a rock to an exhaustive

examination. Owing to the brilliant researches of Sandberger, it is beginning to be recognized that many of the heavy and so-called rare metals are present in ordinary rocks in minute quantities. Until recently we have been disposed to regard these substances as occurring only in mineral veins and in the deeper portions of the earth from which the mineral veins have been supposed to derive their supply of material. Now it is beginning to be clearly recognized that these substances are very widely distributed, even in the superficial crust of the globe. As an illustration of the interest and practical importance of the subject above referred to, I may call attention to the important work by Dr. Becker, on the "Geology of the Comstock Lode," recently published by the U. S. Geological Survey. This lode, the yield of which is supposed to have sensibly affected the bullion markets of the world, occurs in a region which is remarkable for the extreme development of igneous rocks (diabase, diorite, andesites, &c.), and for the widespread alteration to which these rocks have been subjected. The bisilicates, especially, have been affected by this alteration, and for immense distances they have been entirely replaced by a green chloritic mineral.

Most careful assays have been executed, under the supervision of Dr. Becker, for the purpose of determining the amount of bullion in the fresh and unaltered rocks, and the relative amounts of gold and silver. He says: "By comparison of the different assays it appears that decomposed diabase carries somewhat less than half as much silver as the fresh rock. When the decomposed rocks are pyritous the experiments made do not indicate any essential diminution of the gold contents. This fact, however, is quite possibly due to irregularity in distribution and the minuteness of the quantities of gold to be determined. As the decomposition of the rock in question has proceeded at a great depth beneath the surface, it is highly unlikely that silver should have been extracted unaccompanied by gold. Much of the decomposed rock, too, is nearly free from pyrite, and had the gold contents of such specimens been determined, a smaller percentage would probably have been found. The omission (to select specimens free from pyrite) was not detected until it was too late to resume the investigation. So far as quantitative relations are concerned, the silver only can be relied on, though the qualitative detection of gold as well is both interesting and important."

Another point of great interest was determined by Dr. Becker. He isolated the felspar and the augite of the diabase and tested both from silver. He found that for equal weights the augite was eight times as rich as the felspar substance, and as the latter contained some augite, this appears to furnish substantial proof that the silver is a constituent of the augite.

Having subjected a rock to exhaustive chemical and mineralogical examination, it next becomes necessary to compare it with other rocks, and to give it a name. The subject of nomenclature is a very difficult one. It is much to be regretted that notwithstanding all that has been done in descriptive and comparative petrography, we are still far from having any system of classification which is capable of general acceptance. Indeed, we are not agreed as to the first principles on which a classification of rocks should be based. The German petrographers, in most cases, adopt at the outset a principle which we cannot accept. They divide igneous rocks into older and younger: the former including all those which they regard as pre-Tertiary, the latter all those which are of post-Cretaceous age. The division is based, of course, on the assumption that the conditions of eruption in pre-Tertiary times were essentially different from those which have prevailed since. There seems, so far as we can judge, little or no ground for this assumption. The few facts which do at first sight lend support to it appear to be equally capable of explanation on the other hypothesis. The typical pre-Tertiary rocks of the German system are the granites, diorites, gabbros, diabases, and syenites. Now there is reason to believe that these are all plutonic rocks; in other words, that they are the result of slow consolidation beneath the surface, and therefore under great pressure. If this view be correct, then their exposure at the surface can only occur long after their formation, and the fact that the majority of those known to us should be of pre-

Tertiary age, as Lyell long ago pointed out, need occasion no surprise.

Again, it must be remembered that the mere association of eruptive rocks with pre-Tertiary deposits is no proof in itself that the former are of pre-Tertiary age, and also that many competent observers believe that these are clear cases of Tertiary granite, diorite, diabase, and gabbro.

The igneous rocks, which are regarded by the German petrographers as especially characteristic of the post-Cretaceous period, are the basalts, andesites, trachytes, and rhyolites; in other words, the surface-products of volcanic action. That these should be mainly Tertiary, and that they should differ to a certain extent from their pre-Tertiary equivalents in consequence of alteration, is only what might be naturally expected. This, however, is not sufficient to justify the refusal to give the same name to different specimens of one and the same rock merely because they have been produced at different periods; and the work of Allport, Bonney, Geikie, and others has proved that there are basalts, andesites, and rhyolites of Palæozoic age which are identical in structure, composition, and mode of occurrence with modern rocks.

In the absence of any generally recognized system of nomenclature it becomes difficult to assign a name to the rock of the Whin Sill. It is a holo-crystalline rock composed essentially of plagioclase, pyroxene, titaniferous and magnetic iron ore. In sections the felspar occurs in lath-shaped forms. To such a rock, provided it be of pre-Tertiary age, Rosenbusch would give the name diabase. We are inclined, on the other hand, to call the rock a dolerite. The important point for the student to remember, however, is that in the present unsettled state of nomenclature his primary duty is to make himself familiar with the structure and composition of the various rock types. The question of names is, after all, only of secondary importance, provided we remember that in looking at the facts through the medium of an unphilosophical nomenclature we may so distort them as to fail to realize their true forms and relations.

Consider now the ætiological aspect of petrography. Most of us are so constituted that we cannot rest satisfied with a mere description of facts; we almost instinctively endeavor to discover what we call the origin of things. This, after all, merely consists in tracing back as far as possible the chain of events of which the object or phenomenon in question represents the last link. The search after causal relations in the organic world has led to the introduction of a principle which is now recognized as one of the greatest importance in almost every branch of human knowledge. Changes in the characters of organisms are now admitted to be determined by two factors —the inherent properties of the organism and the influence of surrounding circumstances. A very little consideration will serve to show that the changes which occur during and subsequent to the development of minerals and rocks are determined by two allied factors.

Take the case of crystallogenesis. It is not difficult to see in a general kind of way that the characters which a crystal possesses have been determined (1) by the inherent properties of the crystallizing substance, and (2) by the influence of surrounding circumstances—of the environment. When we examine thin sections of rocks, furnace-slags, or the refuse products of glass-works, we frequently find a number of bodies which are intermediate as it were between glass and true crystals. These have been carefully examined and admirably described by Herrmann Vogelsang, who has also succeeded in producing many of them by artificial means. As they serve to illustrate in a very striking way the principle above referred to, a short description of Vogelsang's experiments will not be out of place.

The crystallizing substance finally selected by Vogelsang for the purpose of his experiments was sulphur. This substance is readily soluble in bisulphide of carbon, out of which it crystallizes in the rhombic form. If the process of crystallization be followed under the microscope, nothing definite as to the nature of crystalline growth can be made out. The first objects which appear are definite crystals, and these grow by accretion. If, however, the solution of sulphur be thickened with Canada balsam then, provided the proper proportions of the different substances have been employed, some very interesting phenomena may be ob-

served by the aid of the microscope as the bisulphide of carbon evaporates. Minute fluid spheres arise in the medium and grow by mutual absorption. They finally consolidate as clear, transparent, isotropic bodies, and to them Vogelsang has applied the term globulites. It is impossible to determine absolutely the composition of these globulites, but there seems no reason to doubt the conclusion of Vogelsang that they are portions of the Canada balsam which are richer in sulphur than the surrounding mass, and that they arise in consequence of the attempt of sulphur to crystallize under unfavorable circumstances. Similar bodies may be observed in certain rocks, slags, and blow-pipe beads, although the crystallizing compounds must be very different in the different cases.

Under certain circumstances the mass of sulphur and Canada balsam solidifies with the formation of globulites, but under other circumstances additional phenomena may be observed. When the resistance of the medium is too great to prevent the union of the globulites, but not too great to prevent their approach, they become united into various more or less definite forms. The mode of union depends partly on the way in which the globulites attract each other, and partly on the movements in the mass. A linear arrangement of the glubulites is very common, and to the form arising in this way Vogelsang has given the name margarite. A rectangular grouping is also not uncommon. From a study of the various forms arising in consequence of the union of globulites, Vogelsang concludes that in the case of sulphur there are in each globulite, as it were, three directions at right angles to each other, in which the attraction is considerable, and that in one of these the attraction is decidedly greater than in the other two.

The building up of the compound forms naturally leaves the surrounding space free from globulites.

Under certain circumstances the globulites become fused, as it were, at the points of contact. This occurs when the resistance is sufficient to prevent the assumption of the spherical form, but not sufficient to resist the destruction of the pellicle at the point of contact. In this way rod-like bodies, termed longulites, arise.

It must be remembered that all these forms are strictly isotropic. They are not, therefore, in any sense of the word, crystals. The moment a true crystal of sulphur appears, it can be recognized by its doubly refracting properties. They have been termed crystallites, wherever they occur, by Vogelsang, and they evidently arise in consequence of the attempts of some definite chemical compound to crystallize under conditions which do not admit of the free approach of the molecules.

Between crystallites and perfect crystals showing definite external faces there are numerous intermediate forms, such as microlites and skeleton crystals. As further illustrations of the influence of the environment we have only to consider the facts that no two crystals of the same substance are precisely alike in all their characters, and that some substances, like sulphur and carbonate of lime, may be made to crystallize in two different systems by varying the conditions under which the crystallization is effected.

There can be no doubt, then, that two factors are involved in the determination of the properties which crystals present: the inherent forces of the crystallizing substance and the influence of surrounding substances.

So far, we have referred only to the birth and growth of crystals. But the history of a crystal does not cease with its formation. With a change in the surrounding circumstances the crystal may be modified or destroyed. Thus we see that crystals have a kind of life-history; they are born, they grow in size by accretion, and finally they cease to exist as distinct individuals.

As an illustration of the influence of a change of physical condition on the character of a crystal, consider the case of leucite. At ordinary temperature this mineral is generally regarded as tetragonal, and it certainly shows double refraction in thin sections. Klein has shown that by heating leucite to a point far short of its fusibility it may be rendered perfectly isotropic, and hence follows the conclusion that leucite is really isotropic when subject to the conditions under which it is formed. That crystals should be in a state of stable equilibrium, so far as molecular forces are concerned, when subject to the physical conditions under

which they are formed, is exactly what we should expect, and that this equilibrium should be disturbed by a change in these conditions is also very easy to understand.

As further illustrations of the principle here referred to, consider the various cases of paramorphosis, such as the change from arragonite to calcite, or from calcite to arragonite; or, again, the corresponding changes in sulphur.

Illustrations of the changes which arise in crystals in consequence of changes in the chemical conditions to which they are subjected, need not here be referred to in detail; the destruction of crystalline rocks by denudation is, of course, a consequence of these changes.

Consider, now, the rocks of which the earth's crust is composed. They also have a life-history. They are formed and destroyed, and it is the business of the petrographer not only to describe and classify them, but also to trace out the cycle of change. For the purpose of illustrating this branch of petrography let us consider certain facts with reference to the genesis of crystalline igneous rocks. It will be admitted on all hands that such rocks as granite, syenite, diabase, rhyolite, trachyte, andesite, and basalt have originated by the consolidation of an intensely heated silicate magma under different conditions as to temperature and pressure. The consolidation has been accompanied—except in those cases where the magma has consolidated as a homogeneous glass, and these will be left out of account for the present—by the development of crystals. If, then, we would understand the manner in which crystalline igneous rocks have been formed, we must consider the subject of crystallogenesis in silicate-magmas. Numberless facts which need not here be referred to prove that the process of consolidation is not a sudden one. As the surrounding circumstances (environment) become more and more favorable to crystallization, the minerals separate out one after the other, and at last the whole mass becomes solid, and the rock is formed. The temperature at which any given mineral forms is not determined by its own fusibility. The laws of the formation of minerals in igneous rocks are analogous to those which determine the formation of salts from concentrated aqueous solutions. Cooling influences the separation of the different minerals only in so far as it affects the solubility of the constituents of the minerals in the silicate-magma. The point at which a mineral forms from a silicate solution has, then, no more connection with its fusibility than the point at which graphite forms in molten iron has with *its* fusibility.

Another point of very great importance is this—the differentiation of crystals in an originally homogeneous magma must necessarily be accompanied by a variation in the composition of that magma. It becomes, then, of great interest to determine the general order of the formation of crystals in igneous magmas. On this subject we have a most valuable and suggestive paper by Rosenbusch, entitled "Ueber das Wesen der körnigen und porphyrischen Structur bei Massengesteinen" (*Neues Jahrbuch*, 1882, ii., p. 1). Before proceeding to give an account of the portion of this paper which bears more particularly on the subject we are now discussing, it may be well to call attention to the methods available for the purpose of determining the order of the formation of the minerals in a rock. There are two. In the first place we may observe the phenomena of inclusions, and in the second place we may observe the extent to which crystalline form has been developed. If one mineral is seen to be included in another, then we may safely infer, subject to certain precautions, that the included mineral is the earlier of the two, and if one mineral shows a more perfect form than another with which it is associated, then we may infer—again subject to certain precautions—that the mineral with the more perfect form is the earlier.

Now, in the paper above referred to, Rosenbusch divides the constituents of igneous rocks into four groups:

(1) The ores and accessory constituents (magnetite, hematite, ilmenite, apatite, zircon, spinel, sphene).

(2) The ferro-magnesian silicates (biotite, amphibole, pyroxene, olivine).

(3) The felspathic constituents (felspar, nepheline, leucite, melilite, sodalite, haüyn.

(4) Free quartz.

He then calls attention to the contrast which is presented by the granites and syenites on the one hand, and the diabases

on the other. In the former the following law is adhered to with a very great amount of persistence: The order of formation is that of increasing basicity; the ores and accessory constituents are first formed, and the quartz is the final product of consolidation. In the diabases and gabros there is apparently an exception to this law of increasing basicity, the augite consolidating after the felspar. Rosenbusch proposes to divide the granular holo-crystalline rocks into two classes: (1) Those in which the minerals of the second group in the above classification consolidate before those of the third, and (2) those in which the reverse relation holds. He then calls attention to cases illustrative of the law of increasing basicity which are furnished by the order of separation in the individual groups. Thus, in the ferro-magnesian group, olivine is older than biotite, amphibole and pyroxene; and biotite is older than the bisilicates. In the felspathic group, triclinic felspars are older than monoclinic [there are exceptions to this rule, as, for instance, in the porphyroid of Mairus in the Ardennes, where orthoclase crystals are seen to be surrounded by a narrow zone of oligoclase], and the basic triclinic felspars are older than those which contain a large percentage of silica.

The views of Rosenbusch are based on the assumption that the order of formation of crystals in igneous magmas is determined solely by chemical conditions. That these conditions are the more potent seems quite clear, but there are facts which appear to show that physical conditions are not altogether without influence on the result.

The law of increasing basicity may be accepted without hesitation as expressing in a general way the truth as regards the order of separation of the different constituents of igneous rocks.

Now, a very interesting conclusion follows as a natural consequence of this law. The effect of progressive crystallization in a magma must be to increase the percentage of silica, to decrease the amount of lime, iron, and magnesia, to increase the total amount of alkalies, and to increase the potash relatively to the soda in the part which remains liquid. It is always satisfactory to find independent evidence confirmatory of any conclusion at which one may have arrived. Now, I think we have confirmatory evidence of this kind in the present case. It will be admitted on all hands that the crystals in porphyritic rocks, such as hypersthene-andesite, have been formed in a magma, the composition of which is represented by the bulk analysis of the rock. If, then, we compare the bulk analysis with the analysis of the ground-mass deprived of its crystals, we ought to find confirmation of the above conclusion.

Dr. Petersen has isolated and analyzed the ground-masses of two of the Cheviot porphyritic rocks, and by comparing these with the bulk analysis of the rock, the truth of the conclusion is most strikingly illustrated. The effect of progressive crystallization in the andesitic magma has led unquestionably to an increase in the amount of silica, a decrease in the amount of lime, iron, and magnesia, an increase in the amount of alkalies generally, and an increase in the potash relatively to the soda. In the rock itself, soda is in excess of potash; in the ground-mass, potash is in excess of soda.

There is yet another piece of independent confirmatory evidence. Every geologist is familiar with the phenomenon of contemporaneous veins. The general view held with regard to them is that they represent portions of material which remained fluid after consolidation had progressed to a considerable extent. If this view be correct, then they should hold the same chemical relation to the main mass of the rock as the ground mass of the Cheviot andesite does to the main mass of the andesite. Mr. Waller has recently analyzed certain contemporaneous veins which occur in the bronzite diabase of the Penmænmawr. He finds that they contain about 7 per cent. more silica than the normal rock, less lime and magnesia, more alkalies, and more potash than soda, although in the normal rock soda is in excess. Contemporaneous veins in the Rowley rag dolerite have also been investigated by Mr. Waller, with the same result as far as increase in silica and total alkalies is concerned. The relation of potash to soda has not yet been determined.

I believe it is admitted to be a general rule that contemporaneous veins con-

tain more silica than the rock with which they are associated. It will be seen that there is abundant evidence of an independent character to confirm the general truth of the conclusion which follows from a consideration of the facts brought forward by Rosenbusch.

I should not have treated this subject at such length did it not appear to have an important bearing on the origin and sequence of volcanic rocks. I can best explain this by referring to the Cheviot district, with which I am slightly acquainted.

Andesitic lavas and tuffs cover large tracts of this district. These are unquestionably the products of surface volcanic action. In the central portion of the volcanic area there is a mass of augitic granite. A consideration of the mineralogical composition of this granite shows that it cannot belong to the acid group of rocks, and this conclusion is confirmed by an examination of the chemical composition of allied rocks from the Vosges. So far as we can judge, in the absence of analyses, there appears to be a very close connection beween the composition of the plutonic and that of the volcanic rocks of the Cheviot district, and we therefore seem justified in concluding that the plutonic masses differ in character from the andesitic lavas merely in consequence of difference in the conditions of consolidation. The plutonic rocks represent the consolidation of the andesitic magma beneath the surface, and therefore under great pressure; the lavas and tuffs represent the consolidation of the same magma at the surface.

I now come to the point to which I wish to direct special attention. The andesitic lavas and tuffs are traversed by quartz-felsite dykes in such a way as to show that a magma of rhyolitic composition must have been erupted by the Cheviot volcanoes subsequently to the period characterized by the eruption of the andesitic magma. Contemporaneous veins, similar in character to the quartz felsite dykes also occur in the plutonic rocks. Again an analysis of one of the quartz-felsite dykes by Mr. Waller agrees almost exactly with the analysis of the ground-mass of the hypersthene-andesite by Dr. Petersen.

Putting all these facts together, we conclude that the eruption of an andesitic magma was followed, in the history of the Cheviot volcanos, by that of a rhyolitic magma in consequence of progressive crystallization in the deep-seated plutonic source. The rhyolitic magma is, so to speak, the mother liquor out of which various basic minerals have crystallized. Suppose a half consolidated plutonic mass, originally of andesitic composition, to become subjected to a powerful crush, such as that which unquestionably arises in the earth's crust under certain circumstances. The mother liquor will be squeezed out of the mass, like whey out of cheese, and it may finally consolidate as contemporaneous veins in the plutonic rock, as dykes in the surrounding volcanic rocks, or as rhyolitic lavas and tuffs at the surface. The ideas here thrown out appear to me to be capable of extension to other volcanic regions; but as the sequence in these regions is generally complicated by the coming in of basic rocks during the latter phases of volcanic activity, it will not be advisable to enter more fully into the subject on the present occasion.

The special characters which igneous rocks present, then, are to be traced to the chemical and physical properties of the original magma, and to the influence of surrounding circumstances. Rocks, like minerals, are in a state of stable equilibrium when subjected to the conditions of their formation. When subjected to other conditions, whether physical or chemical, they usually undergo a change. The destruction and disintegration of igneous rocks by the various agents of denudation are familiar to every student of geology, and need not therefore be described on the present occasion.

I trust I have now said sufficient to show that the science of petography is one of the greatest importance to the geologist of the present day. The remarks on ætiological petrography are, of course, only intended to illustrate the nature of this branch of the subject, and to show that conclusions of the greatest theoretical interest may be expected to follow from a careful consideration of the facts acquired by work in the other branch of the science.

OPTICAL REFINEMENTS IN ARCHITECTURE.

From "The Building News."

The Greeks and Romans had a keener sense of the apparent weakness of certain lines in architecture than have modern artists. We all know, on the authority of Vitruvius, but more particularly from the careful investigations made by Mr. F. C. Penrose, M. A., into the refinements of Greek architecture, what attention was paid by the Greek architects of the time of Pericles to the proportions and lines of their edifices. Their extremely sensitive and educated eyes perceived various objectionable effects in the horizontal and vertical lines of their buildings. Thus the effects of straight lines in the entablatures and steps of their temples, and of the uncorrected axes of their columns, especially those at the angle, were early noticed, and in the Parthenon we find a decided curvature of the one and a considerable declination from the perpendicular of the other. Thus Mr. Penrose discovered that in each front and flank of that great temple all the axes of the columns were parallel to each other, except the axes of the angle columns, where the inclination on both fronts is found to be 1 in 106. The object of this inclination inwards in the angle columns is obvious; it was to correct the ocular impression of the portico spreading out at the top, just for the same reason that the *entasis* of a column was given to correct the impression of concavity in the sides, or the convexity of horizontal lines was given to make them appear to the eye perfectly level instead of deflecting towards the center. Vitruvius refers to these illusions, such as the unpleasant effect produced on the horizontal cornice by the raking lines of the pediment; and there is a treatise on optics, written by Heliodorus, which proves beyond question that architects in his day studied optical illusions, for he says: "The business of the architect is to make his work harmonize with appearances, and when anything tends to be misrepresented to the eye he must find contrivances to guard against it, providing for the apparent, and not for the real, equality and symmetry." As Mr. Penrose's results are published, it is unnecessary here to dwell at any length on them. He has proved beyond doubt that the horizontal lines of the Parthenon are curved to a perceptible degree, that the lines of the front are lifted more than those of the flanks—namely, .228 on the steps and .171 on the entablature, represented by the fractions $\frac{1}{450}$ and $\frac{1}{600}$ respectively; while in the flanks they are lifted .340 and .306 or about $\frac{1}{700}$ and $\frac{1}{750}$ respectively. Mr. John Pennethorne first pointed out the curvature in horizontal lines of Greek buildings nearly 50 years ago, and Mr. Watkiss Lloyd, who has devoted considerable study to the subject, has also pointed to certain harmonies of proportion in Greek architecture, upon which basis all buildings were designed. Mr. Lloyd's theory is that a leading proportion was first chosen for the principal components or the length of the upper step of the temple measured in front and flank, which in the Parthenon has the ratio of 4 to 9, the other parts being regulated by a series in which in every term the numerator and denominator differ by the same number as they do in 4 to 9. In other words, as Mr. Lloyd says, the parts were proportioned to one another by simple ratios by building up from the whole to a part, and from one part to another with which it had organic dependence. Thus, the proportions of the temple were adjusted to certain ratios. Taking the angle columns, their size was determined by making the abacus one-fifth of the height of the column; the distribution of the columns was eustyle, or in the proportion of 4 to 9, while the diameter of columns and other parts were determined by the application of similar ratios. Again, the term $\frac{7}{12}$ gave the height from step to apex of cymatium. In one great modern building of glass and iron, the Crystal Palace, a unit of 8ft. governs the plan, every horizontal measurement being a multiple of 8ft. The effect is certainly agreeable. Perhaps few architects would care to follow such analogies in the proportions of modern buildings, though

they are worth a careful study. Optical refinements are, however, useful, and means of correcting the illusions caused by horizontal members, straight lines, and the connection of oblique to perpendicular lines, ought to be devised in all important buildings. Even in such practical matters as the finishings of rooms, the effect of cornices ought to be considered. In "Wightwick's Hints," the effect produced by a heavy cornice, in making a room of given height and size appear low, is pointed out by diagrams. If height is desired, the cornice is better placed chiefly or wholly in the ceiling; if lowness, then on the wall. The intervention of a cove may be found useful in modifying the apparent height of a room or a corridor, and in such a manner the architect can diminish the apparent height of the inclosed space. Upon an analogous principle the eye estimates a dimension, say a height, by any easily-perceived unit, such as the courses of brick or stonework, the joints forming a kind of scale by which the eye judges the height, on the same principle that a figure near a building becomes a measuring unit. To the architect the value of a scale is all important, as he can by its means impress the beholder with the magnitude of his work. The measuring unit of the human figure has at all times served this purpose. Those who have visited St. Peter's at Rome can only realize the vastness of its dimensions by this means, as the proportions of the sculpture serve to reduce its real size, a fault which it has in common with many edifices in which single orders of columns are employed. We here see the value of a proper scale of details. If apparent size is required, it is easy for the architect to proportion his details to that end. He can reduce the size of his cornice or increase the number of members, and subdivide his lengths by pilasters or pillars, or by arbitrary divisions of the wall-space, or his heights in the same manner by the superimposition of orders, or the division into parts by string courses or stories. In surface decoration scale can be readily obtained by the selection of a proper design or a pattern, for the greater the subdivision, or the smaller the pattern, the larger the space becomes, as everyone knows who has tried the effect of different wallpapers. These are well-understood matters on which we need not dwell any longer. Our object is to point out a few less-understood and more recondite sources of delusion of a purely optical kind.

The apparent weakness of a straight lintel or a large square-headed opening may not affect much our sense of beauty in small apertures such as windows, though in the case of a long lintel or a girder the effect on the eye is disagreeable. The long straight tie-beams of roofs, whether of wood or iron, produce a weak effect; they invariably appear deflected instead of straight to the eye. Few architects, however, trouble themselves about the matter, or care to apply any curve, such as the catenary, to make the correction, and the same may be said with regard to the application of curvature to the bounding lines of columns and spires, which is not often made, though very easy. The Greeks used an arc of one of the conic sections for the purpose. Mr. Penrose says the hyperbola was chosen for the entasis of the columns of the Parthenon because that curve offers considerable variety of curvature in so slight an arc, the deflection of which did not exceed one six-hundredth part of the length of the column. We all know that the conic sections were used in the mouldings, as in the corona of the cornice. One cause assigned for the weakness apparent in straight-lined and perpendicular columns, due to their apparent spreading outwards when viewed in front, is that of irradiation, an optical principle which causes an object to seem to spread by the light cast upon it. Thus the columns of the Parthenon were exposed to a strong light on their upper part, which were seen against a shaded background, the result being to make them appear expanded at the top, and to give, in a straight-lined column, an attenuated or concave effect in the central portion of the shaft. The inclination given to the axes of the angle columns before alluded to, the diminution of the columns, as well as their entasis or curvature, were all expedients more or less intended to correct the illusive effects due to the sudden contrast in direction of lines and irradiation of light.

Let us see how the same principles of correction may be applied to features and

details of buildings of the present day, taking the case of uncorrected horizontal and vertical lines, especially such as are in juxtaposition or combined with other lines. A gable or pediment furnishes a good instance of the effect upon a horizontal line of the raking cornices which spring from the ends. The cornice in such a feature always looks sunken in the center, the opposition of the oblique lines of pediment helping to aggravate the appearance of deflection. A like effect happens if the pediment, instead of being straight-sided, is curved. This sunken appearance is evidently greater than would have been if no such upper contrasting lines were visible. Here we have a very obvious case of the illusion caused by contrast of lines having different degrees of inclination. The raking cornices on the same principle appear to drop in the centers of their length. A very noticeable instance of contrast or opposition of lines is observed when an arch, especially of flat curve, springs from or abuts upon vertical piers or walls, as when we see a segmental or obtuse pointed arch abutting against side piers. The latter appear to be thrown out of the perpendicular or to spread at the top. Many of the chancel arches of churches illustrate in a remarkable manner the illusive effect here mentioned. The lofty central piers of most cathedrals, which carry the tower, look to diverge from the vertical at the springing of the arch; the obtuser the latter is, the more divergent the pier lines become. Now, there is a good reason for a correction to the piers to counteract this effect; even an inch or two on each side would correct the visible distortion due to the contrast of lines. Many arches look weak from this cause; the more abrupt the angle between the arch curve and the vertical line, the greater the distortion. A more remarkable illustration of the illusion caused by opposing lines may be observed when two arches at different levels spring from the same pier, as in the case of a chancel or tower arch over the nave and a lower aisle arch. The optical effect produced is to make the pier look crooked. At the junction of the lower arch there is a perceptible curvature of the pier inwards, the lines of the two arches on different levels making the pier bend inwards in the center and outwards at the top, producing a crippled effect. In an arcade of equal and level arches the effect is counterbalanced and neutralized. Other instances often occur showing the influence of contrast, as when a level string course in passing over an arch causes the horizontal line to bend, apparently—a similar effect to the raking and horizontal lines of a pediment. In these cases there is a strong reason for applying a correction to the straight lines. Flat surfaces suffer from a similar sagging when in juxtaposition to contrasting lines or surfaces. Thus a flat ceiling, having a sharply curved cove, invariably appears to bend downwards, the rounded corners of a room make the straight sides, especially at the cornice, look distorted at the meeting of the curve and straight line. A little easing at the junction would be sufficient to counteract the unpleasing crippled effect at these points. Different degrees of illumination on a surface, as we have already remarked, is a cause of distortion, and may tend to increase the illusion of contrasting lines and surfaces. The drops under the tænia are made conical to counteract the apparent diminution caused by their ends being seen against a bright surface, the effect of light being to reduce their width, as when a column is seen against a shaded background at the top. From what we have said the reader will see the importance of attention to details and outlines, differently juxtaposed and illuminated. Contrast, as we have shown, tends to increase the difference between things. The difference of direction between two lines, as in those forming an angle, always appears greater than it is. Contrasts of light and shade, and of color, exhibit the same law, as shadow always makes the luminous object appear the brighter, and light the shadow the deeper. Green emphasizes red, which appears redder by the contrast, and so of all contrasting colors, shades and tints.

What has been observed with respect to right lines applies with equal force to curves. Thus æsthetically we have a preference for curves of contrary flexure to those which are of unvarying curvature, such as circular ones. The conic sections are more interesting than those formed of circular arcs, because of their varying degrees of curvature. But the

principle of contrast has something to do with the preference. Hogarth's "line of beauty" is a curve of contrary flexure—that is to say, the two curves forming it are in opposite directions, one is counteracted by the other, and a highly agreeable curve is the result. Let us suppose a curve joined to a straight line, the effect is displeasing; a distorted appearance at the junction is the result, and this is more aggravated as the curvature is sudden. Thus in a flattened Gothic arch, such as the Tudor, where the upper arcs are too straight, a crippled effect invariably occurs; the upper portion of the arch appears to sink at the haunches where the lower and upper segments unite.

When a perfectly straight line is substituted with a sharp curve at the springing, examples of which are not uncommon, the illusion of depression becomes very marked and disagreeable to the eye. We have here a good lesson of the value of studying ocular impressions, of easing the union between curves of different degrees of convexity, or of arcs of different radii, for when the difference is great, a disagreeable effect is sure to be the result. Architects, in drawing arches of four centers, or in designing window tracery, are not often sufficiently careful; the selection of a curve to please the eye is not so much a question with them as a convenient center or length of radius. Our observations on curvilinear forms might be extended indefinitely. The subject is exhaustless, and has not been adequately dealt with by writers upon architecture. Luckily, nature supplies a corrective to lines of unvarying curvature. In perspective the circle becomes an ellipse; and as we view it in different directions, so the curvature is ever changing with the eye from the elliptical to the hyperbolic. Through this beneficent law of perspective the circle is never seen in but one position; so the vaults and domes of our buildings present ever varying lines, and become a charm to the eye. In these cases the effects produced are more æsthetic than optical, though they equally arise from the pleasure we experience in tracing curved lines, which are less tiring to the muscles and nerves of the eye than those of right lines and angles.

The laws of perspective not only produce a charming variety in curves, but apply a corrective to straight lines and the boundaries of objects viewed by the eye. Although we design in elevation we see in perspective. Those who have studied the principles of binocular vision know that all right lines appear curved when we stand in the center of a front of a long building. The summits or cornice lines, if level, drop down on each side to the points of distance or the vanishing points of the picture, so the same holds good of planes above or below the level of our eye; or, in other words, all planes parallel to the horizontal or vanishing plane at the level of the eye will, when extende , approach the latter. The higher the plane is above our heads the more concave it will appear. These planes, by a natural law, regulate the solid appearances of all objects. Now, the law we have referred to offers another reason for making our straight level lines convex; and though it is perfectly true that this law of perspective applies its own remedy to our right lines and planes, we have still to make compensation for the effects produced by contrasting perpendicular and inclined lines, like those of the angles of buildings and of raking pediments. In short, we never see right lines straight any more than we can see a circle in perspective. These are truths which appear as mathematical truisms, but they are constantly and oddly overlooked by designers. Nature, as she "abhors a vacuum," equally abhors a straight line and a level plane. They are mathematical abstractions which never enter into our views of natural objects. We may look at an extended facade from a distance from a central point of view, but its lines and features grow gradually smaller and dimmer as they recede on either side of us. We cast our eyes upward at a lofty tower whose sides are parallel; but their parallelism vanishes, and we see the tower ascending, but lessening in bulk—bounded, in fact, by lines which, according to nature's own perspective, are gentle and imperceptible curves; so that, if we would follow nature, we should apply an analogous law to our lines whenever they appear weak or unite harshly.

THE SCIENCE OF MECHANICS AS APPLIED TO THE FINE ARTS.

By GEORGE SIMONDS.

Papers of the Civil and Mechanical Engineers.

I have often been asked how it happens that I, who am an artist, am at the same time occupied with mechanical pursuits, and whether there is not some incongruity in the union of the two professions.

To this I reply, that whilst I am before all else an artist, I have nevertheless remarked that art without a sufficient knowledge of mechanics is weak and helpless; and that, on the other hand, it has in all ages reached the highest perfection in the hands of those artists who have possessed the greatest mechanical knowledge.

To convince ourselves thoroughly of this fact, we need only to glance at the artistic achievements of savages in the present day, of whom we can with certainty say, that they have very small mechanical knowledge.

We find that their decorative art is either confined to very simple geometric patterns, or if it aspires to represent living forms is not able to rise above the grotesque; whilst architecture amongst them is confined to the construction of huts or wigwams.

Architecture, of course, more than the two sister, or daughter, arts, as they should more properly be called, is dependent for its development on mechanical science. This fact is so evident that it is needless further to insist on it. We do not expect a savage people to have any architecture. It is on this account that we are so astonished by the ancient monuments found in various parts of the world, such as rocking stones, druidical remains &c.; and yet these do not really prove any great mechanical knowledge, but are monuments of patient labor, aided only by the most simple appliances, and cannot, save by courtesy, be considered as architecture.

Painting and sculpture can only find proper development amongst nations who have arrived at some knowledge of mechanical science, because unless we have architecture of some pretension beyond that of a mere shelter from the weather, we have no need of, and indeed no place for the decorative arts.

In the infancy of the human race, the mere struggle for existence would preclude any attempt at art. Man's needs had to be satisfied before he had leisure to think of aught else. Food, shelter and clothing would occupy all his time and all his energy. To obtain the first he would require a tool to dig the ground, a weapon to kill his prey. For shelter a cave would suffice, and for clothing, the skins of the beasts he had killed.

But as man by practice became more skillful in finding his food, he would have more time on his hands, and be able to consider the means of providing himself with a more suitable and convenient residence. Thus the hut or wigwam would be invented, and being enabled to place his domicile in the locality best suited to supply his material wants, he would again find himself with time to spare. This would be employed in perfecting and embellishing the implements and utensils of which he was already possessed, and also in improving his own personal appearance. As the hut or wigwam would be his first architectural effort, so his first attempt at painting would probably be to daub his own skin with some pigment to render him either attractive to his friends, or terrible to his enemies; whilst his sculpture would consist of rude patterns incised on the handles of his tools or weapons.

The discovery of the plastic properties of clay, and subsequently of the art of pottery, would give an enormous development to the arts of Sculpture and Painting, especially after the invention of the potter's wheel, which was doubtless one of the most ancient if not actually the first mechanical appliance used in connecnection with the fine arts.

To appreciate the immense value of this contrivance to the artists of far-off ages we need only visit the British Museum, where we find vases, cups, bottles and all kinds of earthenware goods which were

evidently spun upon the potter's wheel and afterwards profusely decorated, not merely with conventional patterns, but with figures and groups of men and animals, sometimes colored, but more frequently in red on black ground, or black on red. The Etruscans and Greeks particularly excelled in the production of these pots and vases, which in their hands became valuable works of art, models of style both in drawing and in composition to the artists of all ages.

It was not, however, in the earliest ages that this was the case; on the contrary, the earliest examples of vase painting are, as we should naturally expect, rude and lacking in correctness of proportion and of outline.

The Egyptians seem to have been the first to recognize the importance of regulating the fine arts on scientific and mechanical principles. In the earliest stages of Egyptian art, this does not seem to have been the case, and the oldest work of art we know, a wooden portrait statue in the Museum at Boulak, is thoroughly unconventional, not to say naturalistic in treatment. Doubtless, however, it was found that the absence of school or tradition led to extravagances, and it became needful to lay down fixed mechanical rules for the guidance of artists. As all the science and mechanical knowledge of Egypt was vested in the priesthood, it was they who laid down the rules of construction, proportion, and composition which governed the arts in that country. Little has come down to us of what those laws were, nevertheless, there have been found incompleted works mapped out according to a particular scale or Norma.

The one idea which was ever present to the Egyptian mind seems to have been Eternity. This they strove to express in all their works of art, sculptural as well as architectural, by their leading qualities, stability and durability.

These qualities they insured by making their monuments of vast size, and of the hardest and most lasting materials, as well as by the solidity and compactness of the general design.

The erection of Pyramids and Obelisks by a people who had neither steam power nor hydraulic machinery at their command, excites our just admiration; but although it argues a certain mechanical knowledge, it does not prove an acquaintance with more than the simplest mechanical appliances, set in motion by a boundless supply of brute force. I do not therefore insist on these works, wonderful as they are, as showing any great influence of mechanical science upon art; but with Egyptian sculpture the case is different, and I would submit that without mathematical and mechanical knowledge of a high order, it would have been quite impossible for any people to have erected such colossal works as the Sphynx, the Memnon, and many others. The methods employed by them for working the hardest and most intractable materials have been, and to a great extent are still, matters of conjecture; although it has recently been discovered that for certain purposes they made use of a tool, in its action exactly resembling a modern diamond rock drill. To me, however, it seems less wonderful that they should have been able to master the hard material, than that they should have succeeded in giving pleasing expression and accurate proportions to works of such vast size. It is evident that they must have been hewn out by the united action of many men working at once; no one man could have achieved such a task. There must, therefore, have been a design for them to work to. And to produce a satisfactory result mathematical principles must have been skillfully applied to the enlargement of the small model.

In the British Museum there is a small stone lion of Egyptian origin marked over with lines intersecting each other at various angles, which I think was in all probability a small model from which to construct a similar lion of much greater dimensions.

The art of Egypt probably derived, for a time at least, great benefit from the rules and restrictions imposed on it by the priesthood; nevertheless the system must have had its drawbacks, for if it prevented extravagance and absurdity, it also effectually precluded progress even of a legitimate kind. The same observations apply with equal force to Assyrian art, from which, but for such restrictions, as great results might have been expected as were afterwards obtained in Greece.

With the Greeks the case was of course

very different. It is true that they derived their knowledge of the arts and sciences from Egypt, but they had not the same religion, and not being governed by a priesthood they had no reason for observing the rules and traditions of Egyptian art for one moment longer than they found it convenient to do so. Thus we find that even in the early days of Greek art, when its character is quite archaic, the figures are full of movement and energy, and before long the faces only seemed to retain traces of Egyptian influence, as, for instance, in the Ægina Marbles now in the Museum at Munich, which, although carved only about 40 years before the time of Phidias, when Greek art suddenly rose to its highest, nevertheless, in some little peculiarities of form and feature show their Egyptian descent.

Amongst the Greeks about this time, art and science seem to have made most rapid progress; and sculpture and architecture must have received great assistance from the schools of Euclid and other mathematicians of that day, whose teaching would be invaluable to an artist, in assisting him to understand the laws governing the construction, movements, and balance of so complicated a piece of mechanism as the human form. And it needs, I think, little argument to show that, other things being equal, the man who will make the best machine drawing is he who best understands the machine it represents. So it is with the representation of the human figure, more especially in sculpture, for in painting, color and chiaroscuro are like charity, which often covers a multitude of sins. In sculpture, however, color cannot be made to gloss over faulty and unmechanical construction; and though many sculptors at the present day, of the so-called naturalistic school, endeavor to give an air of reality to their works by imitating the texture of the skin, and other minute details, they often grossly neglect the correct mechanical construction of their figures, so that even if they were suddenly to come to life they could not move, their machinery being ill-designed and badly fitted, even though to the unmechanical eye it may look well enough and be very highly finished.

Construction, movement and balance, the three great points to be observed in the artistic representation of living forms, are all dependent on definite mechanical laws; and we have abundant proof that the Greeks were quite alive to this fact, and closely studied the laws that led in their case to such brilliant results.

Unfortunately, but little has come down to us of the formula they used. Something, however, we have; not much, indeed, but just enough to show us how much we have lost. I refer to the Norma or Canon set forth by the sculptor Polycletes, a very defective account of which has been handed down to us by Vitruvius, who himself does not seem to have been very clear about it.

In the early part of the Christian era, both art and science fell to a very low ebb. Art, indeed, was so poor that when the Romans wished to build a triumphal arch in honor of Constantine, they were obliged to despoil other pre-existing monuments in order to adorn the arch with sculptures which they had not the skill to execute themselves.

It was not until the revival of art and science in the middle ages that we have anything of note to record; but in the 14th and succeeding centuries we are met with a multitude of names of highest fame. In those days we find men who were like Leonardo da Vinci, engineers, both civil and mechanical, as well as sculptors, painters, architects, and who yet found time for the study of poetry, music and astronomy.

Previously to this great revival, the mechanical process of art had been utterly lost; and even at the present day we know but little of the systems whereby the ancients produced their masterpieces. Curiously enough, we have but few unfinished works of antiquity, and these few throw very little light on the system of measurement they employed. We know indeed from the many works in terra-cotta that have survived, that they were skillful modelers, and we know also that they were acquainted with the properties of plaster of Paris, as it is commonly called, and that they were skilful moulders, but we do not know how they reproduced in marble the forms they had designed in plastic material; we do not know whether they were in the habit of making full-sized models of their statues and copying them in marble, or whether

they usually made small sketches in clay or plaster, and enlarged from these. As far as their tools are concerned, we know from the marks that they have left that they were almost identical with those that are in use to-day. They used a point tool for roughing down, which was simply a piece of rod drawn down to a square point, with which they wedged off lumps of marble with a hammer. Having reduced the block to a rough shape, they then brought it still nearer to size with a flat chisel, the edge of which was serrated; and the work was finished with ordinary chisels of various sizes, and with rasps. All these tools were doubtless precisely the same as those used in the middle ages, and at the present day. They also used a drill for removing marble in the undercutting of draperies, and in other parts where it would have been difficult or dangerous to use the hammer and chisel. There is nothing to show how the motive power was applied to these drills, but there is at least a probability that it was a reciprocating motion produced by a cord, wound round the shaft of the drill, and kept tight by a bow, and worked by the carver himself; or else the two ends of the cord may have been worked by an assistant, whilst the carver guided the drill. This latter is the method usually employed in Italy at the present day, as it leaves more freedom to the carver than when the bow is used. Having both hands at liberty, the carver can use his drill as a slotting tool, cutting curves as easily as straight lines. I have, however, never seen any evidence that the ancients were in the habit of so using this tool; on the contrary, wherever I have seen the drill marks they have been clear, distinctly separate marks, with no indication whatever of a side traverse. In the middle ages they do not seem at first to have had any very clear system either of proportion or of measurement, and the mechanism of the human figure was very imperfectly understood. As a natural consequence the figures produced at that time were grotesque, not to say incorrect in action, poor in form and bad in their proportions. As, however, the laws of mechanics were more studied, and the art of construction was so well understood that it became possible to erect such structures as the bell tower of Giotto and later Bruneleschian Dome, the art of sculpture made corresponding strides. The discovery of fragments of antique art gave an impetus to the study of the beautiful, and the proportions of the figure were reduced to a truer standard, whilst the study of anatomy, although not permitted, was ardently practiced by some few artists, and resulted in correct mechanical construction and harmonious movement. The parade of anatomical knowledge has often been made a reproach to Michael Angelo by those who were too ignorant to understand him; and indeed to a superficial observer there would seem to be some reason for censure. Yet there is not one of his figures that is not full of life and energy. The carving is often very rough, the figures are rarely, if if ever, finished in all parts, but one feels that the mechanical construction of the figure is correct. It may be exaggerated, the man may be a man of forty horse-power, but nevertheless he is a man with limbs and joints of a man, correctly articulated, with possible, not impossible muscles and tendons—muscles which need only the vital spark to contract and set the limbs in motion. These figures possess in an eminent degree that highest of artistic qualities, potentiality of motion.

The accounts we have of Michael Angelo's method of working are not as ample as we could desire. We have, however, a diagram from his own hand for calculating the proportions of the human figure, and we have in the writings of Benvenuto Cellini, who was himself acquainted with the great master, what professes to be an account of his system. We have also a vast number of small models and studies in wax and in terra-cotta for large statues, but we have no full-sized models; from which we may, I think, conclude that he made small-sized models when the work was to be executed in marble, and full-sized ones only when it was to be cast in bronze when, as the "cire perdue," or lost wax system of casting was the one he employed, the model would be destroyed in the process. According to Cellini, his plan was to sketch with charcoal on one side of the block the outline of the figure he wished to carve, and that then he at once attacked the block, and, working always from the same side, produced the figure first as a low relief, then in ever-increas-

ing relief, until it stood out freely in the round. To any one who knows the practical difficulties of sculpture this sounds very wonderful, and although I am far from saying that it is impossible, yet knowing Cellini's love for the marvelous, and his hero worship of Michael Angelo, I am inclined to take this account with some reservation. That Michael Angelo could carve a statue without mechanical aids I will not dispute, but that he should have found it convenient to do so is impossible. A scale for proportional measurement was invented by him for the use of sculptors, and is still the most convenient known, and the most simple.

The system of measurement which has been general in Italy for a vast number of years, though there is no record of when it was first used, is a system of triangulation. A model is produced of the same size as the proposed statue in marble. On this model three principal points are determined: two on the base as far apart as possible, and one as near the top of the statue as practicable. A sort of T-square of wood is then constructed, which has a steel point at the end of each of its three arms, and is of such proportions that the steel points each rest on one of the three principal points on the model. This T-square is then transferred to the block of marble, and the three principal points are assumed where the three steel points rest on the block. It is obvious that with this arrangement, any point on the model can be easily obtained on the marble by the use of ordinary compasses; for if we measure the distance of the required point from each of the three principal points on the model, and also measure the depth in, from a point assumed on the T-square, and transfer these measurements to the marble, we shall be able by carefully cutting away the marble, and repeated trials, to find with extreme accuracy a point which will coincide with all the four measurements, and which is the point required. In this way point after point can be found all over the figure, and a replica in marble of the original model is produced with mathematical accuracy. The introduction of this system was obviously of the highest importance to art, as it enabled the artist to depute to his assistants the roughing out and preparing of the work in marble, thus economizing his own time and labor. This system, however, perfect as it is in the hands of careful and skillful workmen, is not altogether without drawbacks. Mistakes may arise from a careless workman taking one pair of compasses for another, or from want of accuracy in measuring from point to point; also, there is a great loss of time, from the fact that each point requires at least three measures to be taken. To obviate this inconvenience, what is known as the "scale-stone instrument" was invented, I believe in England, where alone it obtained any considerable use. The instrument consisted of two blocks of stone, one and sometimes more sides of which where squared up true with the surface. An iron bar formed like a strap passed horizontally along the trued-up side. Between this strap and the side fitted an upright, held in place by a wedge. An arm moving on a universal joint was fastened to this upright, sliding up and down it on a sleeve. At the extreme end of this arm was another universal joint, with a short arm bearing a pointer or needle. All of these joints could be clamped immovably. The model being fixed on the scale-stone, and the block of marble on the other, the upright was now placed in the strap and tightened up by the wedge. The arms were now moved until the pointer rested exactly on the point to be transferred to the marble; all the joints were then clamped up except the needle, which had a little stop put on it and was then withdrawn, it being fitted in a slide. The wedge was then loosened, and the instrument transferred to the other scale-stone, wedged up, and the pointer slid forward; the marble being cut away to allow its motion, until it came against the stop, its point touching the marble at the same time indicated the exact point required. This instrument had many good qualities, but the use of scale-stones was highly inconvenient, and although it was popular in England, the foreign artists would not use it, as they found that the saving of time was more than counterbalanced by the inconvenience of being unable to move the work, and, moreover, they accused the instrument of being inaccurate.

Within the last seven or eight years a

great improvement has been made in pointing instruments, and before long the old instruments will have entirely disappeared. The new instrument, in its latest and most perfect form, consists of two metal tubes, one about 15 in. long, the other about 2 ft. 6 in. The longer one has a strong, steel hook at the one end, the other passes through a sleeve on the middle of the other tube, and can be clamped up tight. The short tube has near each end a sleeve which bears a steel point. This part of the instrument is neither more nor less than the T-square I have already mentioned as used in pointing by triangulation. The steel hook can be removed and replaced by a straight point when required. As a rule, it is hooked on to a point assumed on the head of the statue. On the long bar of this T slides a sleeve, to which is fixed another sleeve at right angles across it. In this second sleeve slides another rod or tube, which in its turn carries another couple of sleeves like those already mentioned, so that the right-angle one is smaller than the other, and carries a smaller rod. At the end of this smaller rod is a ball and socket joint, bearing the sliding needle or pointer described in the scale-stone instrument. This instrument is wonderfully light and handy, and very accurate, and can be used with the work in any position. There is one great drawback, however, to these instruments, they will not work proportionally. You can only use them to produce a work on the same scale as the model. But it is often of great convenience for an artist to be able to enlarge from a smaller design; and much ingenuity has been bestowed on the invention of methods of enlarging and reducing. Every draughtsman is acquainted with the usual old and tedious methods of enlarging and reducing, and the Pantograph is so well known that I need not waste time in describing it. Photography has also helped us somewhat, and is quite convenient for making reductions, but not so for enlargements on any considerable scale. For this latter I have recently seen an arrangement which seemed so satisfactory that I asked the permission of the inventor, Mr. Henry Holiday, the well-known artist, to mention it. It consists of a dark room, about 7 ft. square and 10 or 12 ft. high. In the front of this room is an opening closed by a canvas and paper bellows tapering in form, which can be drawn out several feet long, the smaller and movable end of which is closed with a camera lens. This lens, and its bellows, is supported on a frame which travels backwards or forwards on guides on the floor according to the distance required. Opposite to the lens a screen is placed, and very strongly illuminated. The sketch or drawing to be enlarged is placed against this screen, but inverted. The bellows is drawn in or out, and the proper focus obtained according to the proportion required by the artist, who then finds his sketch inside the dark room projected right side up on a paper screen of the size required, and he has only to trace it off. Mr. Holiday showed me several most interesting experiments with this instrument; amongst other things we enlarged some small photographs of sculpture to full life size, and the same with some photos and drawings from nature, and the result was highly satisfactory. The enlargement of drawings, however, important as it is, is not so serious an affair as enlarging sculpture. This is a subject which has attracted the attention of some of our ablest men. Scores of patents have been taken out for this purpose, and Watt and Chantrey rre said to have joined forces to effect it, but do not seem to have been altogether successful. The most usual and best approved plan has been to work with the three compasses, as in ordinary pointing by triangulation, and transferring the measures taken with the compasses on the small model to the larger measures required by means of the scale of Michael Angelo. This scale is formed as follows: Given a statue of any size to be enlarged to whatever you like, draw on the wall, or any other convenient place, a vertical line, mark off on that line the exact length of the enlarged statue. Then take in your compasses the measure of the small statue, and placing one point of the compass on the point which marks the base of your statue on the large scale, draw a circle with that radius, then draw from the point which marks the top of your large figure a line tangential to that circle, and you have the scale of Michael Angelo. If, now, you wish to transfer any intermediate measures, you take such measure in the compasses, place one

point on your vertical line, and run it along until you find a point where the other arm of the compass will just touch the opposite line without crossing it. From the point of your compass on the vertical line to the apex of the angle formed by the two lines of the scale, is the measure required. This system of enlarging is, in my opinion, the usual plan pursued by Michael Angelo, and may probably have been invented by him, which would account for the scale bearing his name. But the danger of error is very considerable, for a careless workman taking inaccurately any measure on the small model, transfers this initial error to the large scale plus any further inaccuracy that may creep in through elasticity of compasses or other causes, and a very slight initial error becomes a very serious one at the last on the large scale. To get over this difficulty many attempts have been made to produce a pointing instrument that should allow no chance of error to the workman, and it was even proposed that he should not even be allowed to cut away the marble, but that the machine should do all. The difficulties in the way of producing a machine capable of turning out a large marble statue from a small highly finished model were so great, that for artistic purposes, the attempt has been abandoned. The difficulties were elasticity of material, and wear and tear of tools, and unequal expansion and contraction of parts. These causes were ever at work producing ever-varying results. Still, although these machines would not enlarge satisfactorily, some of them would reduce very well indeed, and when made on a small scale, turned out very beautiful work in ivory or similar material. The Colas machine is the best example of this type; I will therefore describe it. It consists of a cast iron bed on which are two revolving tables. These tables can be placed at varying distances from each other as may be required. They are revolved at equal angular velocity by means of an endless chain. At one end of the bed there is fixed a universal joint from which proceeds a pantograph suspended by cords and weights from a frame above. This pantograph being free to move in any plane, and being counterpoised as just mentioned, is used as follows:—The figure to be copied is placed on the table farthest from the universal joint and securely chucked.

The pantograph is set to the proportion required, and the other table is brought into position opposite the reducing point of the pantograph, the chain is tightened up by means of an idle wheel, and the instrument is in order. A block of plaster is now chucked on the reducing table, and the operator takes in his hand the reducing point, which is a cutting point of hard steel, and with it cuts or rather scratches away the surface of the plaster block, until the other, or enlarging point, strikes the surface of the model. He continues this process until there is no more plaster within reach of his cutting point, and then he makes the tables revolve so as to bring fresh material within reach, and so on, until no more remains to be cut away. This seems simple enough, but as he is obliged to work this pantograph in very different planes, in order to get at the different parts of the work, the strains on the instrument are constantly varying, and materially affect the accuracy of the work. Of course the larger the instrument the greater would be this variation. To get over this difficulty it has been found necessary to restrict very materially the size of the instrument, and also to cut the models in pieces and make the reduction of each piece separately, in order to avoid as much as possible the necessity of working in widely differing planes. Yet in spite of all this, the result is not altogether satisfactory, even for reductions. Some errors are sure to creep in in joining up the various parts of the model, and these are almost as objectionable as errors due to the machine itself. I believe this machine to be the best extant, and it has maintained its reputation against all of similar construction for nearly thirty years, but I speak of it from experience. About fourteen years ago I desired to have a statue I had just completed reduced to half-size for bronze-casting. I accordingly sent a plaster cast of this statue to Paris, to the patentees, keeping a cast out of the same mould for myself. In due course the reduction was sent to me, but it did not look quite right. I did not know what was wrong, but I was not satisfied. I called in a brother sculptor, and he also noticed the difference. We held a consultation, and decided to test this reduc-

tion by the scale of Michael Angelo. We accordingly projected the scale on a slate table, and found that the scale of the reduction was not only not 1 to 2 as ordered, but that the small model did not agree exactly to any particular scale, in short it was as full of small errors as any fairly good free hand copy would have been. Of course under these circumstances I declined to pay more than the ordinary value of a hand copy, and the result was that I paid that amount, and they took it, and did not fight, knowing the result would be to depreciate the value of their machine if they brought the case before the courts. Two or three years ago, some Frenchmen had an instrument in Regent Street, which was merely a large Colas machine, neither more nor less, which they were endeavoring to use for enlarging sculpture, and were desirous of getting up a company for the purpose. The project was absurd, and the machine quite incapable of doing very accurate work. There is no existing machine that does. But although it is very difficult to make a machine that will carve on a large scale, it has always seemed to me that it would be very possible to produce an instrument that would take all the responsibility of measurement off the hands of the workman—an instrument which could be set to any scale, and which should tell the workman exactly where he had to cut away the marble, and exactly how much. I often talked over this project when I resided in Rome with my friend Mr. Cardwell, the sculptor, a very able mechanic, and he made some attempts at the construction of an instrument of this description, but was not very successful, except in so far as to show the difficulties as well as the possibilities of the undertaking. This was about eighteen years ago. Five or six years ago I again turned my attention to this subject, and at last succeeded in producing an instrument which I named the Inconograph, but which my workmen christened Polly, a name which has stuck to it.

It is obvious that in a short paper like this it is impossible even to enumerate the half of the ingenious mechanical contrivances which have been invented for the benefit of the arts, or even the less numerous names of those engineers who have also been artists. The subject would require a good sized volume, and would, I think, prove interesting reading. I have merely attempted to skim over the surface of the subject; but if I have not proved that engineers ought to be artists, at least I trust I have shown that artists have much to gain by studying the science of mechanics, and by frequenting the society, and cultivating the friendship of civil and mechanical engineers.

THE ROCKNER-ROTH PROCESS FOR THE PURIFICATION OF SEWAGE AND WASTE WATER FROM FACTORIES.

From "Gesundheits-Ingenieur" for Abstracts of the Institution of Civil Engineers.

This process, which has been in operation in Dortmund, furnishes a ready means for the purification of manufacturing waste liquids. It is both mechanical and chemical in its action. The chemicals employed vary in accordance with the composition of the water to be dealt with, but it is claimed that the utilization of these compounds is so complete that a relatively small quantity suffices, entailing only a moderate cost. After the addition of the chemicals a separation from the liquid takes place of the suspended and dissolved impurities and the matters resulting from the chemical reaction. The subsequent mechanical treatment consists in the introduction of the mixture into a simple apparatus, in the form of an upright cylinder about 7 meters (22.96 feet) high, whose diameter is regulated by the volume of liquid to be treated in a given time. This vessel has certain pipes connecting it with a small air-pump, the dimensions of which for the treatment of 200 cubic meters (44,000 gallons) per diem were as follows: cylinder, 75 millimeters (2.95 inches) in diameter; stroke, 210 millimeters (8.26 inches). It was found practicable in 8 minutes with this pump to exhaust the

air entering the receiver in the course of 5 hours; so that a similar pump, used continuously, would suffice for the daily purification of upwards of 7,000 cubic meters of sewage. Owing to the vacuum produced in the receiver, the external atmospheric pressure slowly forces up the sewage water, together with the precipitate, and this mixture undergoes complete separation of the liquids from the solids. Then by a siphon-like contrivance which is entirely self-acting and continuous in its operation, the purified liquid is caused eventually to flow away at the top of the vessel. Arrangements are made for the escape of oil and the specifically lighter matters at the upper part of the siphon.

By the action of the apparatus, the necessary slow upward movement of the liquid is effected, and by this means also the specifically heavier, suspended matters, and the precipitate resulting from the chemical treatment are deposited at the bottom. Layers of sludge are thus formed in the lower part of the receiver and in the basin beneath it, from which latter the deposited matters may, either continuously, or from time to time, be withdrawn, mingled with a small proportion of the liquid. The presence of this layer of sludge is of much importance, as it acts as a species of filter for the subsequent volumes of sewage, and avoids the waste of chemicals, which in former cases, and under ordinary methods of precipitation at once fall to the bottom of the tank and no longer act on the liquid under treatment. The following are among the advantages of the process set forth by Dr. Kaysser, of Dortmund: Absolute separation of the suspended, and to a great extent also of the dissolved mineral impurities, and the production of a clear, colorless effluent, free from smell. The removal from the water also of the gaseous admixtures, thereby rendering it incapable of supporting the life of the lower organisms; the water remaining slightly alkaline after the treatment, no free sulphuretted hydrogen can be present. The sludge contains all the matters of use to the agriculturist, in the form of a dense and readily utilizable mud; and moreover the purification is effected in an enclosed space, and can therefore give rise to no pestilential effluvium.

G. R. R.

THE DISTRIBUTION OF ELECTRICAL ENERGY BY SECONDARY GENERATORS.

By J. DIXON GIBBS.

From "Iron."

The remarkable results obtained during the last few years from the production of electrical energy and its application to lighting purposes as well as to the transmission of mechanical power, have naturally brought into prominence the great problem of the distribution of electricity. In a complete system of electrical distribution, it is necessary that electrical energy in all its forms should be at the disposal of individual householders, whatever the service they may require it to perform; that is to say, if light is desired, currents should be available at will for feeding every type of lamp, whether arc or incandescent. If mechanical power is required, motors should supply it; whilst currents suitable for electro-chemical purposes should be obtainable with equal facility. The distribution should be over a large area, central stations being preferably situated in the outskirts of towns at a distance from the area to be supplied. Where water power exists it should be utilized, or, if steam power is used, a site should be chosen in proximity with water so as to secure the economy in fuel effected by the employment of condensing apparatus.

It is scarcely necessary to remark that nothing hitherto done in the way of street lighting or of lighting large establishments, such as theaters, hotels, and public buildings, by means of machinery on or near the premises, can be claimed to constitute a distribution of electrical energy.

The same may be said of the transmission of a given force to a single point. This is not a distribution of mechanical power. Gas and water companies do not set up separate works for the supply of special consumers, but from central stations distribute to all consumers in conformity with their several requirements. In order to arrive at the results just described as necessary to a complete system of electrical distribution, the following conditions are essential:—

1. Every receiving apparatus must be supplied with its proportion of electrical energy, so that it may act independently of the others and without affecting them.

2. The regulation must be automatic, instantaneous in its action, and require no attention.

3. The regulation must be of such a nature that the generating dynamo machine shall produce each moment the exact amount of electricity necessary to supply all the apparatus in action.

The chief difficulty in realizing these conditions has been that the intensity and E. M. F. of an electric current being always exactly determined, the uses to which the current can be put are necessarily limited to feeding apparatus of a given resistance and of an electrical capacity, in harmony with the quantity of available electricity, from which it results, that by means of a given current it is only possible to employ apparatus of consumption of identical construction, that is to say, connected together under certain conditions of resistance that must be maintained constant.

The author need not review the various systems, more or less ingenious, which have been invented during the last few years with the result, not of solving, but of going round, the difficulty, for in the interesting lectures recently delivered by Professor Forbes before the Society of Arts, the mechanism of all these combinations has been ably explained. It seems, however, to have resulted from the facts adduced in these lectures:—

1. That the future of electrical distribution lies in the direction of the employment of currents of high potential and small quantity, requiring conductors of small diameter.

2. That the employment of an apparatus, by means of which the factors of the initial energy can be transformed to suit the requirements of each consumer, is essential to the solution of the problem which we are discussing this evening, and the secondary generators under review are such transformers. They are known as the Gaulard-Gibbs secondary generators. The phenomena of induction which have immortalized the name of Faraday are utilized in these instruments. Numerous predecessors have certainly conceived the idea of utilizing secondary currents localized and of different kinds, but coming later in this path of research, the inventors of the secondary generators have labored under more propitious circumstances, because results already arrived at have enabled them to produce these phenomena under such conditions that their employment has been rendered absolutely practical and economical. This consideration certainly inspired them with the courage to pursue with perseverance those researches to which their predecessors had given but passing attention.

However this may be, the present inventors have regarded the employment of these phenomena from a purely industrial point of view. Their first thought after having experimentally verified the actual transformation of primary electrical energy into electrical currents of different kinds, and capable of being applied to every practical purpose, was to make a careful analysis of the phenomema observed. They were thus able to determine the special conditions under which the primary and secondary circuits would yield the highest effective and most economical return for the energy expended; they arrived at the conclusion that the two circuits, inducing and induced, must have the same mass of metal and a position absolutely symmetrical with the common magnetic field. Since it is upon the determination of these conditions that the invention of the secondary generator is based, it may be interesting to know that these conditions, which are a *sina qua non* of an economical return, have never been previously determined.

But it was not sufficient to determine philosophically what the industrial conditions should be, it was also necessary to realize them practically. The inventors accordingly constructed their apparatus with a sufficient number of spirals to produce the required practical E. M. F

by means of a cable formed of an inducing circuit of low resistance surrounded parallelly to its axis by forty-eight wires composing the induced circuit, the sum of the sections of which was equal to the section of the inducing circuit. By means of this arrangement the theoretical conditions already alluded to were approximately fulfilled, since the mean distances of the induced circuits from the magnetic field were equal to the distance of the inducing circuit from the same magnetic field. Further, it was easy to group the extremities of the secondary wires so as to give to the factors E. I. of energy the different values required according to the work to be done. These apparatus served, during five consecutive months, without interruption, to light with arc and incandescent lamps, five stations of the Metropolitan Railway, one apparatus being placed at each station. The primary circuit in which they were placed was composed of a single wire 15 miles in length and $\frac{1}{8}$ of an inch in diameter. This primary circuit was metallically closed throughout its entire length with the terminals of the dynamo machine at Edgware Road. The results as regards effective work formed the subject of a report by Dr. Hopkinson, the conclusions of which were perfectly satisfactory, and are too well known to require repetition.

The anticipations of an economical return having been thus fulfilled, the next step was to seek the most simple and practical methods of applying economically the principle upon which the construction of the apparatus reposed. These researches led to the formation of the inducing and induced circuits by means of copper discs superposed and furnished with ear pieces for the purpose of connecting them together. This arrangement, which allows of the juxtaposition of the two circuits, has also the advantage of permitting the employment of any insulating material that may be found to give the best results. The simplicity of this method of construction is obvious; the weight and size of the apparatus are remarkably small in relation to the work it is capable of performing.

The apparatus is identical in form with the generators exhibited at Turin.

It is worthy of remark that in these apparatus the actual resistances of the inducing and induced circuits are kept as low as possible, so as to render the work expended in the interior circuits very small in proportion to the work available in the exterior circuits. As an example, take the instrument we have before us: it is intended to supply in the exterior circuit an effective work of 750 Watts under the influence of a primary current of 12 amperes—the total resistance of its two circuits induced and inducing is $\frac{3}{10}$ of an ohm, so that $12^2 \times \frac{3}{10}$ Watts represents the work absorbed by the apparatus, and, consequently, useless; thus the theoretical loss of energy resulting from the interposition of these apparatus is $\frac{43.2}{750}$, or $5\frac{1}{2}$ per cent. Nevertheless, when the inventors announced an effective return of 90 per cent., many doubts were expressed in consequence of the unfavorable results hitherto obtained from researches in the same direction—these doubts were really testimonies to the novelty of the results—it remained only to demonstrate conclusively the truth of these results. The authority of an eminent electrician had been insufficient to carry conviction to every mind. An exceptional circumstance, however, enabled the inventors to determine definitely, without room for further question, the accuracy of their assertion.

In the month of January, 1884, the Italian government offered a grand prize of 10,000 fr., to be competed for internationally for the most important advance made in the transport of electrical energy to a distance, and invited other governments to name representatives who should constitute a jury for deciding the question. The Italian government was probably influenced in taking this step by the conviction that the industrial development of Italy would be largely aided by the prompt utilization of the vast natural forces in which the country abounds. The jury was composed of: M. Tresca, membre de l'Institut de France, honorary president; Professor Ferraris, acting president; M. Wattmann, rector of the University of Geneva; Professor Voit, of Munich; Professor Webber, of Zurich; Professor Roiti, of Florence, member of the International Commission for the determination of the ohm; Professor Kittler, of Darmstadt; Professor Cossa, of the School of Engineers at Turin; Professor Farini, of Milan, &c. Profes-

sors Voit and Kittler were the gentlemen deputed to take electrical measurements at the Vienna and Munich Electrical Exhibitions.

Practical experiments of the distribution of electrical energy by means of the secondary generators were made under conditions which M. Tresca, in the name of the international jury of the Turin Exhibition, communicated to the Académie des Sciences de Paris, in terms of which the author will read a translation. The original was published in the *Lumiere Electrique* of October 18, 1884. It runs as follows: "An International Electrical Exhibition is now being held at Turin, in connection with which an important prize is offered by the Italian government and the town. I am charged by my colleagues of the jury of this exhibition to bring to the notice of the Académie the following facts: Messrs. Gaulard and Gibbs have established at the exhibition, the station of Lanzo, and the intermediate stations, a circuit whose length, including return, is 80 kilometers by means of a bronze chrome wire 3.7 millimeters in diameter without covering. This wire carries an alternating current produced by a Siemens electro-dynamic machine of the 60 horse-power type in such a way that the current can be simultaneously utilized for different modes of lighting, whether at the exhibition, or at the Turin station, or at the Lanzo station, or at the intermediate stations, by its transformation at each point of the two factors constituting its energy by means of the secondary generators of the new type, shown by Messrs. Gaulard & Gibbs. On September 25, we verified the simultaneous regular working.

"1. At the exhibition of the following apparatus, which had to be necessarily supplied with very different potentials—nine Bernstein lamps, one Sun lamp, one Siemens lamp, nine Swan Lamps, and five other Bernstein lamps situated at a small distance.

"2. At the Turin Lanzo station, 10 kilometers away, thirty-four Edison lamps of sixteen candles, forty-eight of eight candles, and a Siemens arc lamp.

"On September 29, the experiments were still more conclusive, the system being extended to the Lanzo station, 40 kilometers distant, by the perfectly regular action of twenty-four Swan lamps of 100 volts. The numerous transformations required by the variety of these different methods of lighting are effected with accuracy, and, although we are not able to give the exact figures, it is perfectly demonstrated that the secondary generators may be considered, at all events within certain limits, as transformers giving a relatively large return of the energy of alternating currents. The actions of lighting and extinction are effected without any disturbance (of the other lights) and by means of simple commutators. The principal object of this communication is limited, however, to testifying to the complete success of a distribution of different modes of lighting over an (effective) distance of 40 kilometers. The importance of the realized fact alone demands that it should be fixed by a precise date, but it should be borne in mind that we are not dealing here with the transport of mechanical power."

More than 300 Italian engineers and architects who had witnessed the experiments, assembled for the 1884 congress, passed a resolution of which the author will read a translation. "The fifth congress of Italian Engineers and Architects cannot ignore the great importance of the experiments now being made at Turin by means of the Gaulard and Gibbs secondary generator, and, having examined the working of such a system of distribution, record the hope that the government, the corporations of towns, and manufacturers will patronize this system, and that the expectations which five months of trial on the Metropolitan Railway, the experiments at Turin, and the sound scientific conceptions upon which the system is based have raised in the field of science and industry, may thus be realized."

The measurements taken by means of the electrometer of Mascart of the effective return of the secondary generators are shown by the curves on the diagram, fig. 2. In taking for abscissæ the resistances introduced in the secondary circuit, and for ordinates the primary and secondary work, it will be seen that the progression, at first increasing, arrives at its maximum between the resistances of 6 and 10 ohms, which are the resistances under which the apparatus works normally, but these measurements having been considered by some members of the

jury not absolutely free from theoretical objections, a commission was appointed by the jury, composed of Professors Webber, Voit, Roiti, and Ferraris, to prepare a calorimeter, by means of which Professor Ferraris carried on his experiments during seven consecutive days, and arrived at the conclusions which formed the subject of a special report, which is very voluminous. The conclusions of this report are condensed in the table of which the following is a copy, and which gives an average practical return (column (N)) of 94 per cent. when the apparatus worked under the conditions for which they were constructed.

In the annexed table are shown the theoretical and practical co efficients of the return from a secondary generator coupled in tension, calculated in the following manner, for a series of resistances of the secondary circuit. In column R the values of the total resistances of the secondary circuit vary from 0.28 to 40.0 ohms. In column M are shown the theoretical values of the coefficient of the total return. In column N are shown the values of the coefficient of the exterior theoretical return, and in column (N) the values of the coefficient of the exterior practical or effective return.

R.	M.	N.	(N.)
0.28	0.500	0 000	0.00
2	0.876	0.753	0.74
4	0.933	0.867	0.86
6	0.956	0.911	0.90
8	0.962	0.928	0.92
10	0 967	0.940	0.93
12	0.971	0.948	0.94
14	0.973	0.954	0.94
16	0.974	0.957	0.95
18	0.975	0.959	0.95
20	0.975	0.961	0.95
22	0.976	0.963	0.95
24	0.976	0.964	0 95
26	0.975	0.964	0.95
28	0.975	0.965	0.95
30	0.975	0.966	0.96
32	0.974	0.966	0.96
34	0.973	0.965	0.95
36	0.973	0.965	0.95
38	0.972	0.965	0.95
40	0.971	0.965	0.95

Now that the secondary generator has been absolutely demonstrated to be a perfect transformer of the energy of alternating currents, it only remains to the author to examine whether the conditions under which these instruments act are in perfect accord with the conditions laid down at the commencement of this paper for the solution of the problem. In order to thoroughly understand that this is so, let us suppose that we have to distribute 10,000 glow lamps, 200 arc lamps, and 200 mechanical horsepower in varying proportions, over a circuit 15 miles in length. The author adopts these figures because they represent somewhere about the average requirements of the future. With the aid of the secondary generators this distribution would be effected in the following manner:—

The initial electrical work would be produced by four alternating-current dynamo machines, of the Siemens model, for example, supplying 100 amperes and 3,000 volts each. This work would be distributed over four distinct circuits, metallically closed with the terminals of each dynamo, and formed of a cable having a diameter of one centimeter only, connected with the secondary generators, one of which would be placed in the house of every consumer. The form and size of these secondary generators would necessarily be proportioned to the quantity of work required of each one respectively. It may be remarked here that the secondary generators, fed by an electrical quantity which is constant, develop on the current which feeds them a counter electro-motive force which is proportionate to the work they develop in their external or secondary circuits. From this it follows that the quantity or ampere value of the primary current must remain fixed, whatever may be the number of secondary generators to be fed, and that the E. M. F. only of the primary current will vary according to the sum of the resistances set up by the number, more or less important, of the generators in action on the circuit. This result is automatically obtained by means of a regulator of intensity, which, placed in the primary circuit, acts upon the derivation of the exciting machine, by introducing variable resistances, so as to proportion the intensity of the magnetic field of the generating dynamo machine to the E. M. F., which it must develop in order to overcome the resistance opposed by the secondary generators in action.

It is necessary to remember that the work developed by the secondary generators depends absolutely upon the number of spirals of which each is composed, and the form of energy developed depends on the manner of grouping these spirals. From this it follows that each consumer may, as it pleases him, put his apparatus in action, and cause it to produce the special form of energy he wishes to employ without troubling himself about his neighbors. The most absolute independence of each apparatus, and an automatic proportioning of the work produced to the work expended, permit the realization by this system of the essential conditions already indicated for enabling a distribution of every alternating form of electrical energy in currents resulting from the phenomena of induction, which produce always, and necessarily, the alternating form of current.

The old ideas, attributing special danger to the employment of alternating currents, have been ably corrected by Professor Forbes, Dr. Hopkinson, and others; but it is worthy of remark that in an installation under the system the author is discussing—whatever may be the E. M. F. of the primary current, which, it must be remembered, circulates always in a closed circuit—the difference of potential between the terminals of the secondary generators will never be greater than that necessary for the lamps fed by them—that is to say 100 volts or 50 volts, as the case may be. Nothing, therefore, short of culpable carelessness could possibly give rise to a condition of things presenting any danger whatever to the public.

If consumers required electrical energy only for producing light, a solution brought to this point would be as complete as possible; but the applications of continuous currents are too numerous not to render their distribution also desirable, and that with the same facility which, it has been shown, attends the distribution of alternating currents.

It has been already pointed out that the *sine qua non* of the practical and economical transport and distribution of electrical energy to a great distance is to give to the energy to be transported the form of small quantity and high tension, or E. M. F. But although the known types of alternating-current dynamos adapt themselves with the greatest facility to the production of currents of the highest E. M. F., this is, unfortunately, not the case in regard to the collection of continuous currents of a higher E. M. F. than 2,000 volts. On this account the inventors of the secondary generators sought and found a means of redressing the alternating currents produced by their secondary generators. These currents, it will be remembered, have already, by transformation, a low E. M. F., that is to say, they are in the form most readily utilizable. On November 16 last, Professor Ferraris, president of the International Jury of the Turin Exhibition, witnessed the perfect redressing of a current produced by a secondary generator—this current had 16,000 changes of direction per minute.

The instrument for redressing an alternating current is composed of several electro magnets coupled in series and fixed on a cast iron frame, a similar number of electro magnets attached to a movable frame turning on its axis, a redressing commutator fixed on the same axis, having as many changes of polarity as bobbins, and lastly of collectors receiving the currents. The alternating current enters by the fixed bobbins traversing them in series; the point where the current leaves is attached to the brush which communicates with the commutator; the opposite pole communicates directly with the other brush. These brushes are so arranged that they can never come into contact with the same metallic pieces. If the apparatus is at rest, the movable electro magnets are also traversed by the alternating currents; but as soon as the apparatus begins to work, the commutator inverts the poles of the movable electro magnets. When the speed has reached the synchronism of the alternations, the current becomes continuous in the movable bobbins, and maintains the synchronism. It is necessary only to take a derivation on the collectors to have a continuous current.

This instrument has, then, the property of taking, under the influence of a very small alternating current, a speed which is synchronic with the changes of direction of the current which feeds it. Thus, then, until it has been found possible to do important work with continuous cur-

rents of high E. M. F., the secondary generators will enable the distribution in all its forms, whether alternating or direct, whether of high or low potential of electrical energy, over distances sufficiently great to enable the practical utilization of natural forces. The value of the progress thus realized has been officially recognized in Italy by a grand prize of 10,000 francs.

Besides installations in Italy and elsewhere, there is one now in course of preparation, of which the central station is situated in New Bond Street, which will doubtless be studied with interest. This installation will consist of the distribution of more than 5,000 lamps of various systems—the steam engines now being placed in position are of over 600 horse-power indicated, and have been manufactured by Messrs. Marshall & Company, of Gainsborough. The dynamos to be used are by Messrs. Siemens Brothers, and are the largest yet made by that firm; they have already been tested, and give most satisfactory results. An excavation of 6,000 square feet has been made under the Grosvenor Gallery, and in this space the machinery is being placed.

It may be interesting to mention that, pending the laying down of the large engines, a temporary engine of 30 horse-power nominal is driving two Siemens W′ dynamos coupled in parallel, which give a current of 24 amperes and 800 volts. This current traverses sixteen secondary generators of 2 horse-power each, which supply currents for 300 glow lamps distributed in the library and club at the Grosvenor Gallery, and in two adjoining establishments in Bond Street. When the satisfactory working of the permanent installation shall have been demonstrated, it is not unreasonable to expect that a wide extension of the application of electricity for the purpose of house-to-house lighting upon the principle described by the author will take place.

HYDRAULIC TABLES BASED ON THE FORMULÆ OF D'ARCY AND KUTTER.

By P. J. FLYNN, Mem. Tech. Society.

From Transactions of the Technical Society of the Pacific Coast.

THE object of this paper is to show how the work of computation required for the solution of problems in hydraulic engineering is very much diminished by the use of the following tables. It is believed that these tables will be of material help to hydraulic engineers who make it a practice to use the formulæ of D'Arcy and Kutter in the solution of problems relating to pipes for the flow of water.

In the following formulæ let—

$V =$ velocity in feet per second.

$Q =$ discharge in cubic feet per second.

$C =$ coefficient of mean velocity.

$S =$ fall of water surface (h), in any distance (l), divided by that distance $= \frac{h}{l} =$ sine of slope.

$a =$ area of cross section of pipe or conduit in square feet.

$p =$ wetted perimeter of pipe or conduit in lineal feet.

$r =$ hydraulic mean depth in feet $=$ area of cross-section of pipe or conduit in square feet (a) divided by its wetted perimeter in lineal ft. $p = \frac{a}{p}$

$d =$ diameter of pipe or conduit.

$n =$ the natural coefficient, the value of which depends on the nature and condition of the bed of the channel through which the water flows, or in other words, its degree of roughness.

The plan on which the tables are constructed will be briefly stated here and their use will be more fully explained at the end of the paper.

Chezy's general form of formula for velocity is—

$$V = c\sqrt{rs} = c\sqrt{r} \times \sqrt{s}$$

therefore

$$Q=ac\sqrt{rs}=ac\sqrt{r}\times\sqrt{s}$$

The factors on the right hand side of the equations are tabulated, $c\sqrt{r}$ and $ac\sqrt{r}$ for diameters usually adopted in practice, and $\sqrt{s}$ for several slopes.

Now to find the velocity, the diameter and slope being given: Look out and note down the number representing $c\sqrt{r}$ in its column and opposite the given diameter; also look out rnd note down the number respresenting $\sqrt{s}$ opposite the given slope. The product of these two numbers will give the required velocity. Again, given the slopeand velocity in feet per second to find the diameter. From the equation

$$V=c\sqrt{r}\times\sqrt{s}$$

we have

$$c\sqrt{r}=\frac{V}{\sqrt{s}}$$

Look out the value of $\sqrt{s}$ corresponding to the given slope and divide the velocity by it. The quotient will be the value of $c\sqrt{r}$. In the column of $c\sqrt{r}$ look out the nearest number to the value of $c\sqrt{r}$ so found, and opposite to it in the same line will be the diameter required. At the same time the area and hydraulic mean depth can be found on the same line and the discharge can be found by looking out the value of $ac\sqrt{r}$ and dividing it by $\sqrt{s}$. In fact by inspection of the tables and the multiplication or division of two numbers, problems can be rapidly and accurately solved, which, by the use of any one of the formulæ, would be a tedious and troublesome operation. When the value of $c\sqrt{r}$ or $ac\sqrt{r}$ is found, the diameter can at once be found by inspection.

When the slope and velocity are given and the diameter is required it is not found directly. The value of $c\sqrt{r}$ is first computed by formula (10) and in the same line with this value in the tables will be found the required diameter. In a similar way the slope and discharge being given and the diameter required. The value of $ac\sqrt{r}$ is first computed by formula (15) and in the same line with this value in the tables will be found the required diameter.

The columns of $c\sqrt{r}$ and $ac\sqrt{r}$ can be used to compare velocities and discharges of pipes with equal slopes, and this can be done even when the channels have different degrees of roughness if the tables have been prepared from the same formula.

For instance, a pipe say of 3 feet diameter has a discharge of 20 cubic feet per second by D'Arcy's formula, and it is required to find the diameter of a pipe which shall discharge 30 cubic feet per second, that is an increase of 50 per cent., the slope being the same in both pipes. Find in Table 1 the value of $ac\sqrt{r}$ opposite 3 feet diameter and it is $=674.09$, and this increased by 50 per cent. $=1011.1=$ the value of $ac\sqrt{r}$ corresponding to the required diameter. By inspection of Table 1 the nearest value of $ac\sqrt{r}$ to this is found to be 1021.1 opposite a diameter of 3 feet 6 inches, which is the required diameter. In a similar manner velocities can be compared by the use of the column giving the values of $c\sqrt{r}$.

For feet measures D'Arcy's formula is

$$V=\left\{\frac{rs}{.00007726+\frac{.00000162}{r}}\right\}^{\frac{1}{2}} \qquad (1)$$

and from this we have

$$S=\left(.00007726+\frac{.00000162}{r}\right)\frac{V^2}{r} \qquad (2)$$

In order to simplify, ***substitute for r in feet the diameter d in inches***, and we have

$$S=\left(.00007726+\frac{.00000162\times 48}{d}\right)\frac{48\ V^2}{d}$$

$$\therefore\ S=\left(.00370848\,d+.00373248\right)\frac{V^2}{d^2}$$

As the change will not materially affect the result, Mr. J. B. Francis, C. E., simplifies this into the form

$$S=.00371\left(d+1\right)\frac{V^2}{d^2} \qquad (3)$$

$$\therefore\ V=\left\{\frac{S\,d^2}{.00371\,(+1)}\right\}^{\frac{1}{2}} \qquad (4)$$

In order, however, to further simplify the equation into the Chezy form of formula, which is the form required for the preparation and use of the table adopted by the writer, and given at the end of this paper, let equation (3) be transformed

Table 1.—Circular Pipes, Conduits, etc., flowing under pressure. D'Arcy's formula for clean pipes.

Table giving the values of a and r, and also the values of the factors $c\sqrt{r}$ and $ac\sqrt{r}$ for use in the formulæ.

$$V=c\sqrt{r}\times\sqrt{s} \qquad Q=ac\sqrt{r}\times\sqrt{s}$$

These factors are to be used only for clean pipes under pressure.

d=diameter in ft.	ins.	a= area in square feet.	r=hydraulic mean depth in feet.	For velocity $c\sqrt{r}$.	For discharge $ac\sqrt{r}$.
0	$\frac{3}{8}$	.00077	.0078	5.251	.00403
0	$\frac{1}{2}$	.00136	.0104	6.702	.00914
0	$\frac{3}{4}$	.00307	.0156	9.309	.02855
0	1	.00545	.0208	11.61	.06334
0	$1\frac{1}{4}$	.00852	.0260	13.68	.11659
0	$1\frac{1}{2}$	.01227	.0312	15.58	.19115
0	$1\frac{3}{4}$	.01670	.0364	17.82	.28936
0	2	.02186	.0417	18.96	.41357
0	$2\frac{1}{2}$	0.0341	0.052	21.94	.74786
0	3	0.0491	0.063	24.63	1.2089
0	4	0.0873	0.084	29.37	2.5630
0	5	0.136	0.104	33.54	4.5610
0	6	0.196	0.125	37.28	7.3068
0	7	0.267	0.146	40.65	10.852
0	8	0.349	0.167	43.75	15.270
0	9	0.442	0.187	46.73	20.652
0	10	0.545	0.208	49.45	26.952
0	11	0.660	0.229	52.16	34.428
1	0	0.785	0.250	54.65	42.918
1	2	1.069	0.292	59.34	63.435
1	4	1.396	0.333	63.67	88.886
1	6	1.767	0.375	67.75	119.72
1	8	2.182	0.417	71.71	156.46
1	10	2.640	0.458	75.32	198.83
2	0	3.142	0.500	78.80	247.57
2	3	3.976	0.562	83.77	333.08
2	6	4.909	0.625	88.39	433.92
2	9	5 939	0.687	92.90	551.72
3	0	7.068	0.750	97.17	686.76
3	3	8.295	0.812	101.2	839.38
3	6	9.621	0.875	105.1	1011.2
3	9	11.044	0.937	108.9	1202.7
4	0	12.566	1.000	112.6	1414.7
4	3	14.186	1.062	116.1	1647.6
4	6	15.904	1.125	119.6	1901.9
5	0	19.635	1.250	126.1	2476.4
5	6	23.758	1.375	132.4	3146.3
6	0	28 274	1.500	138.4	3912.8
6	6	33.183	1.625	144.1	4782.1
7	0	38.485	1.750	149.6	5757.5
8	0	50.266	2.000	160.0	8043.0
9	0	63 617	2.250	169.8	10804.
10	0	78.540	2.500	179.1	14066.

into one with the *diameter d in feet* and it becomes

$$S=.00371\left(12\,d+1\right)\frac{V^2}{144\,d^2}$$

Table 2.—Circular Pipes, conduits, etc., flowing full. Kutter's formula with n=.011.

Table giving the values of a and r and also the values of the factors $c\sqrt{r}$ and $ac\sqrt{r}$ for use in the formulæ,

$$V=c\sqrt{r}\times\sqrt{s} \qquad Q=ac\sqrt{r}\times\sqrt{s}$$

These factors are to be used only where the value of n, that is the coefficient of roughness of lining of channel, =.011, as for surfaces carefully plastered with cement with one-third sand in good condition, also for iron, cement and terra-cotta pipes, well jointed and in best order, and also surfaces of other material equally rough.

d=diameter in ft.	ins.	a= area in square feet.	r=hydraulic mean depth in feet.	For velocity $c\sqrt{r}$.	For discharge $ac\sqrt{r}$.
0	5	0.136	0.104	26.76	3.6398
0	6	0.196	0.125	30.93	6.0627
0	7	0.267	0 146	34.94	9.3294
0	8	0.349	0.167	38.77	13.531
0	9	0.442	0.187	42.40	18.742
0	10	0.545	0.208	45.85	24.976
0	11	0.660	0.229	49.46	32.644
1	0	0.785	0.250	52.85	41.487
1	2	1.069	0.292	59.13	63.210
1	4	1.396	0.333	65.21	91.037
1	6	1.767	0.375	71.08	125.60
1	8	2.182	0.417	76.76	167.50
1	10	2.640	0 458	82.11	216.76
2	0	3.142	0.500	87.36	274.50
2	3	3.976	0.562	94.84	377.07
2	6	4 909	0.625	102.0	500.78
2	9	5.939	0.687	109.0	647.18
3	0	7.068	0.750	115.7	817.50
3	3	8.295	0 812	122.1	1013.1
3	6	9.621	0.875	128.3	1234.4
3	9	11.044	0 937	134.4	1484.2
4	0	12.566	1.000	140.4	1764.3
4	3	14.186	1.062	146.1	2072.7
4	6	15.904	1.125	151.7	2413.3
5	0	19.635	1.250	162.6	3191.8
5	6	23 758	1.375	173.1	4111.9
6	0	28.274	1.500	183 1	5176.3
6	6	33.183	1.625	192.7	6394.9
7	0	38.485	1.750	202.0	7774.3
8	0	50.266	2.000	219 7	11044.
9	0	63.617	2 250	236.6	15049.
10	0	78.540	2.500	252.5	19834.

$$\therefore\quad V=\left\{\frac{144\,d^2\,S}{.00371\,(12\,d+1)}\right\}^{\frac{1}{2}}$$

but $d^2=16\,r^2=16\,r\times r=4\,d\times r$ substitute this value for d^2 in the last equation, and

TABLE 3.—Circular Pipes, conduits, etc.' flowing full. Kutter's formula with $n=.013$.

Table giving the values of a and r, and also the values of factors $c\sqrt{r}$ and $ac\sqrt{r}$ for use in the formulæ,

$$V=c\sqrt{r}\times\sqrt{s} \qquad Q=ac\sqrt{r}\times\sqrt{s}.$$

These factors are to be used only where the value of n, that is the coefficient of roughness of lining of channel $=.013$ as in ashlar and well laid brickwork, ordinary metal, earthenware and stoneware pipe, but not new, cement and terra cotta pipe not well jointed nor in perfect order, plaster and planed wood in imperfect or inferior condition, and also surfaces of other material equally rough.

d=diameter in ft.	ins.	a=area in square feet.	r=hydraulic mean depth in feet.	For velocity $c\sqrt{r}$.	For discharge $ac\sqrt{r}$.
0	5	0.136	0.104	21.20	2.8839
0	6	0.196	0.125	24.60	4.8216
0	7	0.267	0.146	27.87	7.4425
0	8	0.349	0 167	31.00	10.822
0	9	0.442	0.187	34.00	15.029
0	10	0.545	0.208	36.87	20.095
0	11	0.660	0.229	39.84	26.296
1	0	0.785	0.250	42.65	33.497
1	2	1.069	0 292	47.85	51.157
1	4	1.396	0.333	52.90	73.855
1	6	1.767	0.375	57.80	102.14
1	8	2.182	0.417	62.58	136.54
1	10	2.640	0.458	67.07	177.07
2	0	3.142	0.500	71.49	224.63
2	3	3.976	0.562	77 77	309.23
2	6	4.909	0.625	83 82	411.27
2	9	5.939	0.687	89.71	532.76
3	0	7.068	0.750	95.37	674.09
3	3	8 295	0.812	100.9	836.69
3	6	9.621	0.875	106.1	1021.1
3	9	11.044	0.937	111.3	1229.7
4	0	12.566	1.000	116.5	1463.9
4	3	14.186	1.062	121.4	1722.0
4	6	15.904	1.125	126.2	2007.0
5	0	19.635	1.250	136.4	2659.0
5	6	23.758	1.375	144.3	3429.2
6	0	28.274	1.500	152.9	4322.9
6	6	33.183	1.625	161.2	5339.7
7	0	38.485	1.750	169.2	6510.6
8	0	50.266	2 000	184.5	9272.6
9	0	63.617	2.250	199.1	12663.0
10	0	78.540	2.500	212.8	16709.0

$$V=\left\{\frac{144\times4\,d\times r\times S}{.00371\,(12\,d+1)}\right\}^{\frac{1}{2}}$$

therefore, *for feet measures* D'Arcy's formula for velocity is simplified into

$$V=\left(\frac{155256\,d}{12\,d+1}\right)^{\frac{1}{2}}\times\sqrt{rs} \qquad (5)$$

and putting the first factor on the right-hand side of the equation $=c$, we have

$$V=c\sqrt{rs}=c\sqrt{r}\times\sqrt{s}$$

Kutter's formula for feet measure is

$$V=\left[\frac{41.6+\frac{1.811}{n}+\frac{.00281}{s}}{1+\left(41.6+\frac{.00281}{s}\right)\frac{n}{\sqrt{r}}}\right]\times\sqrt{rs} \ (6)$$

and putting the first factor on the right-hand side of the equation $=c$ we have

$$V=c\sqrt{rs}=c\sqrt{r}\times\sqrt{s}$$

In the solution of problems relating to pipes, of diameters different from those given in the tables, Kutter's formula (6) can be simplified in form by adopting it to a slope of 1 in 1,000. It then becomes when $n=.013$

$$V=\left\{\frac{183.72}{1+\left(44.41\times\frac{n}{\sqrt{r}}\right)}\right\}\times\sqrt{rs} \qquad (7)$$

and when $n=.011$

$$V=\left\{\frac{209.05}{1+\left(44.41\times\frac{n}{\sqrt{r}}\right)}\right\}\times\sqrt{rs} \qquad (8)$$

The velocity by these formulæ will be near enough to the true velocity for all practical purposes. The difference between the result found and the true velocity will be about the same amount for the same diameter as that shown in tables 7 and 8.

As shown above, the general form of the formulæ of D'Arcy and Kutter is

$$V=c\sqrt{r}\times\sqrt{s} \qquad (9)$$

$$\therefore\ c\sqrt{r}=\frac{V}{\sqrt{s}} \qquad (10)$$

$$\sqrt{s}=\frac{V}{c\sqrt{r}} \qquad (11)$$

$$S=\left(\frac{V}{c\sqrt{r}}\right)^2 \qquad (12)$$

Now

$$Q=a\,V=ac\sqrt{r}\times\sqrt{s} \qquad (13)$$

$$\therefore\ a=\frac{Q}{v} \qquad (14)$$

TABLE 4.—Giving fall in feet per mile; the distance corresponding to a fall of one foot, and also the values of s and $\sqrt{s}$.

$s=\frac{h}{l}=$ sine of slope $=$ fall of water surface (h) in any distance (l) divided by that distance.

Fall in feet per mile.	Slope, one in	s	$\sqrt{s}$.
1	5280.	.000189393	.013762
2	2640.	.000378787	.019463
3	1760.	.000568182	.023836
4	1320.	.000757576	.027524
5	1056.	.000946969	.030773
6	880.0	.001136364	.033710
7	754.3	.001325797	.036411
8	660.0	.001515151	.038925
9	586.6	.001704445	.041286
10	528.0	.001893939	.043519
11	443.6	.002083333	.045643
12	440.0	.002272727	.047673
13	406.1	.002462121	.049620
14	377.1	.002651515	.051493
15	352.0	.002840909	.053300
16	330.0	.003030303	.055048
17	310.6	.003219696	.056742
18	293.3	.003409090	.058388
19	277.9	.003598484	.059988
20	264.0	.003787878	.061546
21	251.4	.003977272	.063066
22	240.0	.004166667	.064549
23	229.6	.004356060	.066000
24	220.0	.004545454	.067419
25	211.2	.004734848	.068810
26	203.1	.004924242	.070173
27	195.2	.005113636	.071510
28	188 6	.005303030	.072822
29	182.1	.005492424	.074111
30	176.0	.005681818	.075378
31	170.3	.005871219	.076624
32	165.0	.006060606	.077850
33	160.0	.006250000	.079057
34	155.3	.006439393	.080246
35	150.9	.006628788	.081417
36	146 6	.006818181	.082572
37	142.7	.007007575	.083711
38	139.0	.007196969	.084835
39	135.4	.007386363	.085944
40	132.0	.007575757	.087039
41	128.8	.007765151	.088120
42	125.7	.007954545	.089188
43	122.8	.008143939	.090244
44	120.0	.008333333	.091287
45	117.3	.008522727	.092319
46	114.8	.008712121	.093339
47	112.3	.008901515	.094348
48	110.0	.009090909	.095346
49	107.7	.009280303	.096334
50	105.6	.009469696	.097312
51	103.5	.009659090	.098281
52	101.5	.009848484	.099241
53	99.60	.010037871	.100189
54	97.77	.010227273	.101130
55	96.00	.010416667	.102060
56	94.28	.010606060	.102983
57	92.63	.010795454	.103901
58	91.03	.010984848	.104809
59	89.49	.011174242	.105708
60	88.00	.011363636	.106600
65	81.23	.012310606	.110953
70	75.43	.013257575	.115141
80	66.00	.015151515	.123091
90	58.66	.017045454	.130559
100	52.80	.018939393	.137620
120	44.00	.022727272	.150756
140	37.71	.026515151	.162835
160	33.00	.030303030	.174077
180	29.33	.034090909	.184637
200	26.40	.037878787	.194625
240	22.00	.041666667	.213200
280	18.86	.053030303	.230283
320	16.50	.060606060	.246183
360	14.66	.068181818	.261116
400	13.20	.075757575	.275241
450	11.73	.085227272	.291937
500	10.56	.094696969	.307729
600	8.800	.113636363	.337100
700	7.543	.132575757	.364109
800	6.660	.151515151	.389249
900	5.866	.170454545	.412861
1000	5.280	.189393939	.435194
1500	3.520	.284090909	.532925

$$ac\sqrt{r}=\frac{Q}{\sqrt{s}} \tag{15}$$

$$\sqrt{s}=\frac{Q}{ac\sqrt{r}} \tag{16}$$

$$\left(\frac{Q}{ac\sqrt{r}}\right) \tag{17}$$

By the use of the tables of factors as applied to the solution of the six formulæ (9) (10) (11) (13) (15) (16) all the information relative to pipes, such as velocity, slope, etc., can be obtained.

Kutter gives for the values of the natural coefficient of surface:

$n=.010$ Plaster in pure cement.

$n=.011$ Plaster in cement with one-third sand.

$n=.013$ Ashlar and brickwork.

Mr. Lewis D'A. Jackson, C. E., who first translated the work of Kutter into English, and who in his *Hydraulic Manual*, and other contributions to the literature of hydraulic engineering, has done so much to introduce the new and improved hydraulic formulæ into English and American practice, extends the

range of surfaces to which the above values of n apply.

In his *Hydraulic Manual* he states that:

"A coefficient of roughness $n=.010$ has been assumed as applicable to glazed or enameled pipes, and one of .013 for ordinary metal or earthenware or stoneware pipes under ordinary conditions, but not new; and there is every reason to believe that these assumptions are generally correct; if we compare the smoothness of surface of a glazed pipe with that of very smooth plaster in cement, and that of an ordinary pipe, in average condition with that of ashlar or good brickwork; in addition to this, such few partial and limited experimental data as are available support the assumption."

Mr. R. Hering, C.E., in a paper read before the Am. Soc. C. E., in 1868, gave a list of pipes of different materials whose surfaces closely approximate in roughness to the surfaces included by Kutter's coefficients $n=.011$, and $n=.013$.

In this paper the coefficients of Jackson and Hering have been adopted, and table 2 gives $n=.011$ for surfaces carefully plastered with cement with one-third sand in good condition, also for iron, cement and terra-cotta pipes, well jointed, and in best order, and also surfaces of other material equally rough. Table 3 assumes that $n=.013$ for ashlar, well laid brickwork, ordinary metal, earthenware and stoneware pipes, but not new, cement and terra-cotta pipes not well jointed nor in perfect order, plaster and planed wood in imperfect or inferior condition, and also surfaces of other material equally rough.

In Table 5 the coefficients of D'Arcy, and Kutter with $n=.011$ for smooth pipes are placed in parallel columns for purposes of comparison. It will be seen that beginning with the small pipes D'Arcy's coefficients have, for the same diameter, a greater value than Kutter's, but that as the diameters increase, the value of the coefficients approach nearer to each other, until at 14 inches diameter they are equal. From this point as the diameter's increase, Kutter's coefficients are the greater, the difference increasing with the increased diameter of pipes. For diameters greater than 10 feet D'Arcy's coefficient is almost constant. It

TABLE 5.—Comparison of coefficients (c) in the formula.

$$V=c\sqrt{rs}.$$

D'Arcy's coefficients for clean pipes under pressure.—Kutter's coefficients for pipes flowing full with $n=.011$ and $s=.001$.

Diameter. Ft.	Ins.	D'Arcy's Co-efficients for clean pipes.	Kutter's Co-efficient, $n=.011$ $s=.001$
0	5	103.8	82.9
0	6	105.3	87.4
0	7	106.4	91.5
0	8	107.2	95.0
0	9	107.9	97.9
0	10	108.5	100.5
0	11	108.9	103.3
1	0	109.3	105.7
1	2	109.9	109.5
1	4	110.4	113.0
1	6	110.7	116.2
1	8	111.0	118.8
1	10	111.3	121.3
2	0	111.5	123.6
2	3	111.7	126.5
2	6	111.9	129.1
2	9	112.1	131.5
3	0	112.2	133.6
3	3	112.3	135.6
3	6	112.4	137.2
3	9	112.5	138.8
4	0	112.6	140.4
4	3	112.7	141.7
4	6	112.7	143.0
5	0	112.8	145.4
5	6	112.9	147.6
6	0	113.0	149.5
6	6	113.0	151.2
7	0	113.1	152.7
8	0	113 2	155.4
9	0	113.2	157.7
10	0	113.3	159.7

increases very little more than 113.5 even for a diameter of 16 feet or more, but Kutter's coefficient continues to increase until such a diameter is reached as is never likely to be required in practice.

Now, the experiments on which D'Arcy's formula is based were made on clean pipes, of the diameters usually adopted in practice, flowing under pressure and under conditions somewhat similar to pipes in actual use, and therefore, as the experiments were conducted with great accuracy, the results are entitled to the confidence of engineers. D'Arcy's experiments did not, however, include pipes of a very large hydraulic mean radius.

TABLE 6.—Of coefficients (c) from the formulæ of D'Arcy, Kutter and Fanning for small pipes below 5 inches in diameter.

$$V=c\sqrt{rs}.$$

Diameter in inches	(c) D'Arcy's Co-efficient for clean pipes.	(c) Kutter's Co-efficient from formula $n=.011$ $s=.001$	(c) Kutter's Co-efficient recommended by L. D. A. Jackson.	(c) Fanning's Co-efficient for clean iron pipes.
0 ⅜	59.4	33.0	82.9	
½	65.7	36.1	82.9	
¾	74.5	42.6	82.9	
1	80.4	47.4	82.9	80.4
1¼	84.8	51.9	82.9	
1½	88.1	55.4	82.9	88.0
1¾	90.7	58.8	82.9	92.5
2	92.9	61.5	82.9	94.8
2½	96.1	66.0	82.9	
3	98.5	70.1	82.9	96.6
4	101.7	77.4	82.9	103.4
5	103.8	82.9	82.9	

TABLE. 7—Showing the velocity in feet per second in pipes, by Kutter's formula (6) and also by the tables, the value of n being .011.

Diameter, Feet.	Inches.	Slope, one in	Velocity, by Kutter's formula.	Velocity, by Flynn's tables.
1	0	66	6.62	6.51
1	0	2640	1.02	1.03
2	0	66	10.89	10.75
2	0	2640	1.67	1.70
4	0	66	17.52	17.28
4	0	2640	2.70	2.73
6	0	66	22.63	22.54
6	0	2640	3.54	3.56

TABLE 8.—Showing the velocity in feet per second in pipes, by Kutter's formula (6) and also by the tables, the value of n being .013.

Diameter, Feet.	Inches	Slope, one in	Velocity, by Kutter's formula.	Velocity, by Flynn's tables.
1	0	66	5.34	5.25
1	0	2640	0.81	0.83
2	0	66	8.91	8.80
2	0	2640	1.36	1.39
4	0	66	14.44	14.34
4	0	2640	2.24	2.27
6	0	66	18.91	18.82
6	0	2640	2.94	2.98

Kutter's formula is derived not only from experiments made on channels with small hydraulic radius, but also on channels with large hydraulic radius, and his coefficients for very large pipes are therefore more likely to agree with the actual discharge than D'Arcy's constant coefficient of 113.5 for very large pipes. But again, Kutter's formula is open to the objection that it is based on experiments made on open channels. I may here remark, although it is only remotely connected with pipe discharge, that Major Allan Cunningham states, as the result of his extensive experiments for four years on the Ganges Canal, that Kutter's formula alone, of all those tried by him, was found generally applicable to all conditions of discharge, and that it gave nearer results to the actual velocity than any of the other formulæ tried by him. It gave results with a difference from the actual velocity seldom exceeding 7½ per cent., and usually much less than that. When we contrast the wide divergence of the old formulæ under varying flow from the actual velocity, with the results obtained by Kutter's formula, it will be seen that the latter is the most accurate formula for channels with large hydraulic mean radius.

In Tables 2 and 3 the values of the factors of Kutter's formula are not given for diameters less than 5 inches. Mr. L. D'A. Jackson, C. E., in his *Hydraulic Manual*, states:

"For the present, and until further experiments have thrown more light on the subject, it may be assumed that the coefficient of discharge for all full cylindrical pipes, having a diameter less than 0.4 feet, will be the same as those of that diameter."

Although Mr. Jackson's opinion is entitled to great weight, still the facts all tend to prove that the coefficients of diameters below 5 inches should diminish with the diminution of diameter. The smaller the diameter the more effect will the roughness of the surface have in diminishing the discharge. Table 5 shows that Kutter's coefficient for 5 inches diameter with $n=.011$ is 82.9, and therefore, according to Mr. Jackson, all the diameters from 5 inches to ⅜ inch should have a coefficient of 82.9. This is contrary to the principle of Kutter's formula, the accuracy of which is due to

the fact that, other things being equal, its coefficients vary with the diameter. The following proofs are given in support of the opinion that coefficients of diameters below 5 inches should diminish according to the diminution of diameter.

1. In Table 6 the coefficients of D'Arcy's formula are seen to diminish. At 5 inches diameter the coefficient is 103.8, and at $\frac{3}{8}$ inch diameter 59.4.

2. In Table 6 the coefficients of Fanning's formula diminish from 4 inches diameter with a coefficient of 103.4 to one inch diameter with a coefficient of 80.4.

These coefficients are derived from the mean velocities in clean pipes with a slope of 1 in 125 given in Fanning's tables.

3. In Table 6 the coefficients, as found by Kutter's formula with a slope of 1 in 1,000, and $n = .011$, are for 5 inches diameter, 82.9, and for $\frac{3}{8}$ inch diameter, 32.0.

The facts, therefore, show that the coefficients diminish from a diameter of 5 inches to smaller diameters, and it is a safer plan to adopt coefficients varying with the diameter than a constant coefficient. No opinion is advanced as to what coefficients should be used with Kutter's formula for small diameters. The facts are simply stated, giving the results of well-known authors.

As the coefficients of D'Arcy's formula vary only with the diameter, the values of the factors $c\sqrt{r}$ and $a\,c\sqrt{r}$ given in Table 1 for D'Arcy's formula are practically the exact values for all diameters and slopes given, and the results found by the use of the tables will be the same as the results found by using the formula.

Kutters factors as given in the tables, are, however, absolutely exact for only one slope, that of 1 in 1,000; but for the slopes given in actual practice to pipes, sewers, conduits, etc., the use of the factors gives results differing so little from the results derived from the use of the complicated formula (6) that the values of the factors $c\sqrt{r}$ and $ac\sqrt{r}$ given in the tables may, for all practical purposes, be accepted as sufficiently accurate.

In tables 2 and 3 the values of $c\sqrt{r}$ and $a\,c\sqrt{r}$ for Kutter's formula differ by a small quantity from the actual values as found by the use of formula (6). These values by Kutter's formula depend not only on r, but also on n and s, so that a change in any of these three quantities causes a change in the values of $c\sqrt{r}$ and $a\,c\sqrt{r}$. It is found, however, that the slope of 1 in 1,000 will give coefficients which practically differ very little from the coefficients derived from the slopes usually given to lines of pipe.

The values of the coefficients from Kutter's formula given in the tables have been computed for a slope of 1 in 1,000, and they give values of $c\sqrt{r}$ and $a\,c\sqrt{r}$ near enough for practical work.

The two tables 7 and 8 show how small is the difference between the velocity found by the tables of factors and the velocity found by formula (6). The difference seldom reaches more than three per cent., and it is generally less than one per cent. In most cases this degree of accuracy will be deemed sufficient, but should the engineer prefer to use formula (6), even then the tables of factors will give a ready means of checking the computations.

EXPLANATION AND USE OF THE TABLES.

The velocity mentioned below means velocity in feet per second. The discharge mentioned below means the discharge in cubic feet per second.

1. What is the velocity and discharge by Kutter's formula of an iron pipe of 2 feet diameter, and with a fall of 9 feet per mile, the value of n being assumed equal to .013.

By formula (9) $\quad V = c\sqrt{r} \times \sqrt{s}$

and by formula (13) $\quad Q = ac\sqrt{r} \times \sqrt{s}$

In Table 4 look out the value of $\sqrt{s}$ corresponding to a fall of 9 feet per mile, and it is found $=.041286$. Look out also in Table 3 the value of $c\sqrt{r}$ and $ac\sqrt{r}$ opposite a diameter of 2 feet, and they will be found to be respectively equal to 71.49 and 224.63—substituting the values so found in equations (9) and (13), and we have

$V = 71.49 \times .041286 = 2.95$ feet per second velocity.

$Q = 224.63 \times .041286 = 9.27$ c. feet per second discharge.

If the velocity and discharge are found by Kutter's formula (6) it will be seen that a very great saving of work is effected by the use of the tables.

2. An iron pipe one foot six inches in diameter, whose natural coefficient of roughness is assumed =.011, is to have a velocity not to exceed 3 feet per second. What should its slope be by the use of Kutter's formula?

By formula (11)

$$\sqrt{s}=\frac{V}{c\sqrt{r}}$$

Find by inspection in Table 2 the value of $c\sqrt{r}$ opposite 1 foot 6 inches. It is equal to 71.08. Substitute this value and also the given velocity in equation (11) and we have

$$\sqrt{s}=\frac{3}{71.08}=.042206$$

Look out the nearest value of $\sqrt{s}$ to this in Table 4, and it will be found to be .043419 opposite a slope of 10 feet per mile, which is the slope required.

3. A 3 feet 6 inch old iron pipe whose natural coefficient is assumed = .013 is to be replaced by a new pipe capable of discharging double that of the old pipe, the slope remaining unchanged. What is the diameter by Kutter's formula of the new pipe, its natural coefficient being assumed=.011.

Find by inspection in Table 3 the value of $ac\sqrt{r}$ opposite 3 feet 6 inches diameter. It is found equal to 1021.1. Then $1021.1\times2=2042.2$. As the value of n for the new pipe = .011, look out in Table 2 the value of $ac\sqrt{r}$ nearest to 2042.2, and it is found to be 2072.7 opposite a diameter of 4 feet 3 inches, which is the diameter required.

Look up the values of $c\sqrt{r}$ for each pipe, and it will be seen that the velocity in the new pipe is to that in the old as 146 : 106.

4. This example is taken from Weisbach's *Mechanics of Engineering*.

A system of pipes consisting of one main and two branches is required to discharge by one branch 15, and by another 24 cubic feet of water per minute. The levels show the main pipe to have a fall of 4 feet in 1,000, the first branch 3 feet in 600, and the other branch 1 foot in 200. What diameter should the pipes have?

For the solution of this example table 1, derived from D'Arcy's formula, will be used.

The main is to discharge 39 cubic feet per minute, equal to 0.65 cubic feet per second with a slope of 1 in 250. One branch 15 cubic feet per minute, equal to 0.25 cubic feet per second, with a fall of 1 in 200, and the other branch, 24 cubic feet per minute = 0.4 cubic feet per second, with a fall of 1 in 200.

By inspection find in Table 4 the value of $\sqrt{s}$ nearest to 1 in 250 (21 feet per mile) and it is found to be=.063066, and also find the value of $\sqrt{s}$ nearest to 1 in 200 (26 feet per mile). It is found = .070173. By formula (15).

$$ac\sqrt{r}=\frac{Q}{\sqrt{s}}$$

$$\therefore \text{ for main pipe } ac\sqrt{r}=\frac{0.65}{.063066}=10.307$$

the nearest value of $ac\sqrt{r}$ to this in Table 1 is 10.852, opposite which is the diameter, 7 inches.

In the same manner for the first branch,

$$ac\sqrt{r}=\frac{0.25}{.070173}=3.562$$

and the nearest value of $ac\sqrt{r}$ to this, in Table 1, is 4.561, corresponding to a diameter of 5 inches.

For the second branch,

$$ac\sqrt{r}=\frac{0.4}{.070173}=5.7$$

and the nearest value of $ac\sqrt{r}$ to this, in Table 1, is 7.3 opposite a diameter of 6 inches. The required diameters are therefore: for the main pipe 7 inches, for the first branch 5 inches, and for the second branch 6 inches.

Although the explanation of this example, in the use of the tables may appear somewhat long, still the actual work can be done very rapidly and with little trouble. If a comparison is made of the work required for the solution of this example, as given above by the tables, with the work required for its solution by the method of approximation as given in Weisbach's *Mechanics of Engineering*, from which the example is extracted, it will be seen that there is a great saving of labor effected by the use of the tables.

A more extensive table of slopes and $\sqrt{s}$ than Table 4, and also table of factors for circular and egg-shaped brick sewers with $n=.015$ are given in "Hydraulic Tables" in No. 67 of *Van Nostrand's Science Series* by the writer of this paper.

[No. 67 of Van Nostrand's Science Series contains similar tables for circular and egg-shaped sewers.]

PROOF TESTS OF IRON AND STEEL.

From "The Engineer."

The extent to which iron or steel should be strained by testing it, when made up into a boiler, a small girder, a bolt, &c., has from time to time formed matter for debate amongst the most competent experts for a very long period, and still remains so to some extent, although a sort of general agreement has been arrived at that the test stress ought not to be less than one-third or more than one-half the ultimate strength of the *matériel*. Cases occur now and then, however, where a member of a structure tested before fitting in place to half the estimated ultimate strength, fails soon after it is put to work at, at all events, an estimated stress much below that to which it was exposed in testing. Such cases, though comparatively rare, cause, when they occur, perplexity in the minds of engineers, and shake their faith in the utility of proof tests. Such tests, in common with many other sources of information on things mundane, are valuable in proportion to the intelligence and soundness of judgment with which their results are considered. Because the subsequent behavior of the material tested may, in stray instances, contradict the testimony of the test, it should not be concluded that tests are therefore altogether valueless. Such tests are of the highest importance, and much of the research devoted to the investigation of the chemistry, the molecular formation, and the effects of working upon iron and steel, has been brought about not only by the results of tests, but also by even the contradictions apparently visible about such tests. To paraphrase Euclid, conditions cannot be the same and yet be different one from another; and it is owing to the operation of this law that many of the seeming contradictions shown between the behavior of iron in the testing machine and its subsequent performance in actual work are really, when intelligently considered, no contradictions at all. Theory is one thing, practice another—very much another frequently. The trial of a piece of iron or steel in a machine made with mathematical precision, the piece itself prepared with the greatest care, and the strain applied under very special conditions, constitutes, perhaps, the nearest known approach of practice to theory. The iron from which the sample has been taken is probably most identical in quality with it, yet when worked into a girder, a crane jib, the rib or stringer of a ship, or other structure, it fails at what seems to be a much lower stress than should have sufficed to fracture it, simply because at the time other conditions are present.

The real value of proof tests, as dealt with in commercial transactions or engineering work, is two-fold. First, they enable the engineer, by the process of analogous reasoning, to estimate chances pretty closely; to calculate from past experience even of the percentage of seemingly inexplicable failures, and to specify for a particular class of metal. He may specify on either of two systems—he may stipulate that certain mixtures of ores are to be used, that they are to be worked with certain fuel, and in a particular way, knowing by experience that a certain process of manufacture will give material of corresponding nature; or he may take the simple course of demanding material that shall endure a certain proof strain. Unless the engineer be what many otherwise eminent men are not, an expert practical iron worker by training, it is best for him to adopt the latter course; the adage that "a little knowledge is a

dangerous thing" is essentially applicable to iron or steel making. Those who mean to make good iron must devote all their time to the study of that subject alone. If an imperfectly informed person draw a specification stating a particular method of making his iron, and that it disappoints his expectations when brought to the testing machine, he cannot blame any but himself, he must abide by his bargain. Having taken the premises of the transaction into his own hands, obviously he must stand or fall by the results. Where he leaves the premises to the iron maker, simply demanding a certain result, the maker has to bear the risks.

The second use of tests is really to enable business to be done at all. We have not yet reached the stage where the iron maker is called on to warrant the working endurance—the life insurance in effect—of the stuff embodied in a roof, bridge, or ship, steam boiler, or railway axle. Perhaps, by-and bye, a system of life insurance, at least for railway axles, whether plain or cranked, may come into practice, and its introduction might have good results ; but all that is in the future. If an engineer wants iron, it is obvious that some intelligible and simple definition of quality must be used, and the subsequent behavior of the material is another matter. The real point for the users of iron or steel to consider is how best to prolong the life of either; and the method in which either is called upon to perform its work enters as intimately into the question as do the laws of health and sanitation into that of the duration of animal existence. Iron will, it may be almost certainly assumed, have a very prolonged life if exposed to statical strains alone well within its elastic resistance. In a long-continued state of repose under stress it perhaps gradually becomes crystalline, but with nothing like the same rapidity as when exposed to vibration. That the natural tendency of iron is to pass from a fibrous to a crystalline state is well known, and the process of transition is rapid in proportion to the amount of vibration brought to bear upon it. Fairbairn's famous experiments on rods and bars exposed to exceedingly moderate stress while at the same time struck rapidly by a revolving cam, with the resulting speedy fracture of the bars, are familiar to most men who have given any thought or attention to the subject. These experiments are borne out every day, especially on railways.

In order to remedy a defect its cause must first be sought. We will take axles of railway wagons and propeller shafts as two examples. In both these examples we have vibration attending on their use, though different in nature; in the axle the vibrations are short, sharp, and incessant, in kind not very unlike that caused by Fairbairn's cams; the result is also the same, though longer deferred. In respect to securing the endurance of axles, two parties are obviously concerned—the maker of the material and the user of the axle—and both are responsible in the matter. We must candidly express the opinion that the axle makers have done far more to give the public immunity from accidents due to broken axles than have axle users. Of the two horses drawing the coach, one does, we will not say more than his share; he does his utmost, however, and no stone is left unturned, no expense spared, no device neglected by our iron and steel makers, to produce unbreakable axles; but we may, and do say, that axle users, and especially private wagon owners, have done nothing on their side of the pole to draw the coach. Wagon axles are fitted now as they were forty years since; no attempt at improvement, no effort to diminish vibration has been, or is being, made. The springs, the only deadeners of shock attempted to be used, are little better than a name. They are too rigid. They do not possess enough elasticity to be long-lived even themselves, and their preservative influence upon axles is almost *nil*. What we have said of axles applies equally to propeller shafts. Makers of these use every effort to produce sound shafts, but marine engineers, like railway men, stir not to improve the conditions of the workings of these shafts. Railway men, indeed, go through the form of introducing shock deadeners, but marine engineers do not even this; and it is idle to make tests, obtain certain material, and then put that material to work under conditions that daily experiences prove to be faulty in the extreme.

YACHT MEASUREMENT AND TIME ALLOWANCE.

From the "Nautical Magazine."

During the past winter, the question of time allowance for yachts has been again under the consideration of the Yacht Racing Association, and a step has been taken by them which it is hoped may promote competitions between cruising yachts and racers. The developments of the last few years, it has been pretty generally admitted, have had a prejudicial effect upon sport. Competitions, it is said, are practically limited to the latest-built racing machines, which cost very large sums of money, have poor accommodation, and are only adapted to win cups for a few seasons, after which they are out of date as racers and not fit for cruisers. It has been sought to adopt some principle of distinction by which racers and cruisers might compete on something like equal terms; but, of course, the old difficulty has cropped up, and it is first necessary to establish some rule by which to determine what is a racer and what a cruiser. There is, further, the class of yachts to be considered which may be called ex-racers, and whose capabilities for winning prizes are of an intermediate character. The Association have decided upon a scheme for the classing of yachts in three divisions, called A, B, and C, in which for purposes of racing, class A would be rated at four times their tonnage, class B at twice their tonnage, and class C at their nominal tonnage. The means of distinguishing between the classes is by sail area, or rather, by the area of the mainsail and topsail. A maximum area is prescribed for classes B and C, but of course in A it is unlimited. It is not expected that this will be of very much benefit to old-fashioned cruisers, but it will, certainly, where it is carried out, give an opportunity for ex-racers to compete on equal terms with racers of the present type. It will be remembered that the tonnage rule of the Yacht Racing Association is

$$\frac{(\text{Length and Breadth})^2 \times \text{Breadth}}{1730}$$

but an alternative measurement is based upon sail area as follows:

$$\frac{\text{sail area} \times \text{Length}}{7000}$$

It would appear that where the latter rule be adopted, in combination with the new classification, the racers will, through having to pay a double price for their large sail area, be at a considerable disadvantage in comparison with vessels in class B. There is every probability that this new arrangement will enlarge the area of competition, and so promote genuine sport, but it will have this distinct disadvantage that vessels in classes B and C will not be able to use the sails which are suited to their size and stability, but will have to adopt, where possible, just about the area allowed them as their maximum. It is, however, absolutely impossible to place all vessels on an equal footing for racing, and, if this could be done, it would not, we think, promote the interest of genuine sport. If we could measure every point which makes for speed, assess it, and then give time allowance for the whole, yacht racing would become merely a trial of skill in sailing, not what it should be—a competition between yachts.

In an able paper, read before the Institution of Naval Architects, at their recent session, Captain Tuxen has attempted to do this, but we do not think, for the reasons stated above, that his, or any similar proposal, is likely to find much favor with yachtsmen. Captain Tuxen, it should be stated, is Chief Constructor of the Danish Royal Navy, and was a student at the Greenwich School of Naval Architecture. He has been induced to turn his attention to the subject by request from a Copenhagen Yacht Club. Captain Tuxen's scheme is thus stated by himself:—

(1). The dimensions to be taxed are L, length of water-line (with probably some addition for yachts with excessive rake of stem and counter); B, largest breadth; H, draught of water.

(2). Of the total sailing power which a yacht possesses, a certain percentage is due to her length, a certain percentage

to her breadth, and the remainder is due to her draught.

(3). The three dimensions are to be taxed the same relative proportion as their influence upon the sailing power of the yacht.

(4). By increasing any one of the dimensions the tonnage of the yacht is to increase proportionally to the increase of that part of her sailing power which is due to this dimension.

To satisfy these requirements the following system is worked out, viz. :—

Yacht tonnage =

$$\frac{C_1 L^2 + C_2 B^2 + C_3 H^2 + C_4 L + B + H.}{C_5.}$$

The coefficients C_1 to C_5 have to be determined so that the above conditions are satisfied, and should this not be possible more terms must be added, but no doubt it will be found that the number given is sufficient. The coefficients have been determined to suit several yachts of great variety of form which have sailed so much together that their relative sailing power is known, and the following result has been obtained :—

Yacht tonnage =

$$\frac{L^2 + 10 B^2 + 30 H^2 + 20 L + B + H.}{11000.}$$

The following table shows the relative influence of the three dimensions on the sailing power of some different forms of yachts according to this system :—

Proportions of Yacht. L.	B.	H.	Perc'tage of sail'g power due to: Length.	Breadth.	Draught.
1	$\frac{1}{3}$	$\frac{1}{8}$	56.5	28.5	16.
1	$\frac{1}{4}$	$\frac{1}{6}$	60	20.5	19.5
1	$\frac{1}{6}$	$\frac{1}{5}$	63	13.5	23.5

If this system should be adopted in any place, and should be found not to tax the three dimensions according to the experience at that place, the coefficients are easily altered. If any dimension is found to be taxed too severely, the corresponding coefficient has to be diminished, and *vice versa.* If extreme proportions of yachts are found to be taxed too much, the coefficient of the fourth term has to be increased, and *vice versa.*

Captain Tuxen's rule it will be seen, is an attempt to make such allowances as will bring all yachts on an equality, both as regards sailing power and also resistance, in fact, as we have said, if his rule were successful in its operation, yacht races would be reduced to competitions between sailing masters. What then is it advisable to measure in a yacht? If power, her stability would be the best measure on which to base time allowance. A rule of this character was suggested by Mr. Dixon Kempt in the *Field* of 11th of February, 1882. He proposed that the yacht should be inclined as ships are to find the center of gravity. The yacht should be in the condition in which she was to race with all weights in their proper places. Then if a weight W be moved across the deck through a distance *k* and the angle of inclination, thus produced, noted, which call A, then the formula for sail power will be :—

$$W \times k \times \text{ contangent } A.$$

He proposed to multiply this quantity by the length and to divide by some appropriate divisor, say 300, to reduce the figures to something like actual tonnage.

Another measurement based upon power of propulsion, is the sail-area multiplied by length, which we have already spoken of as the alternative rule of the Yacht Racing Association. The chief disadvantage attending it is that it attaches equal importance to all the sails, whereas the mainsail is decidedly of much greater relative importance than is due to its excess in area.

The existing tonnage rule of the Association is based on dimensions; we have before expressed the opinion in these pages that its chief fault is the leaving draught of water out of the calculation. When half of two-thirds of the total weight of a yacht is ballast, the depth of that ballast is a most important factor, and a rule which leaves depth, or rather draught, out of the question, is, as a well-known yachtsman said in the *Field* a few months ago, like a bushel measure with no bottom to it. It has appeared to us that a rule like the old Thames rule but in which instead of half-breadth, draught should be used, in fact for

$$\frac{(L - B) \times B \times \frac{B}{2}}{94}$$

substitute

$$\frac{L \times B \times \text{Draught}}{100}$$

would be a fair measure of size, but if it be thought that length is relatively of

greater importance than the other dimensions it might take the form of

$$\frac{L^{\frac{3}{2}}\times(B\times D)^{\frac{3}{4}}}{C.}$$

where C is a constant divisor to be ascertained.

Captain Tuxen, in his paper, also deals with the question of time allowance, and, remarking that experience shows that a smaller yacht is by the present system taxed too severely relatively to a larger one when there is a strong wind, and too little when there is a slight wind, suggests that the principles regulating allowance should be:—

(1). The allowance of time given by a larger to a smaller yacht should be directly proportional to the distance sailed and the velocity of the wind, and inversely proportional to the size of the yachts.

(2). It is supposed that a yacht double the size of another can allow the latter 2.6 seconds per knot sailed through, and per knot velocity of wind.

He proposes to measure the average velocity of the wind during the time of the course by an anemometer, and gives a table for use in applying his principles of allowance. We do not think that yachtsmen are likely to favor any allowance for wind, that being an element of chance which gives zest to the sport. Beside this, it would be very difficult to measure by an anemometer the velocity of the wind *where the yachts were sailing*, while at a station on shore it might be altogether different. We may remark, in reference to this part of the question, that the Association has had the time-scale under consideration, and has determined upon an alteration with a view to decrease the allowance somewhat between large yachts and increase the allowance between small yachts.

ON THE ELECTROMOTIVE ACTION OF ILLUMINATED SELENIUM, DISCOVERED BY MR. FRITTS, OF NEW YORK.*

By WERNER SIEMENS.

From "The Electrical World."

Mr. Ch. E. Fritts, of New York, sent me, early last year, a description of his method of constructing light-sensitive selenium plates, differing from mine in essential features, and accompanied the same by a number of the plates prepared by him. These do not consist of parallel platinum wires embedded in a thin body of selenium, like mine,† but of a thin, homogeneous sheet of selenium, which is spread upon a metal plate, and after a subsequent heating—for the conversion of the amorphous into crystalline selenium—is covered over with a fine gold leaf. Mr. Fritts has found that the green light which penetrates through the gold, by the further passage through the selenium heightens its electrical light-capacity. In fact the sensitiveness to light (electrical conductivity) of the selenium plate between the gold leaf and the metallic base-plate is increased by the lighting of the gold leaf with direct sunlight falling perpendicularly upon it, in some of the constructions sent here, from 20 to 200 times! The effect of the illumination by diffused daylight is also greater in the Fritts cells than in mine.

One of the plates sent over to me showed absolutely no sensitiveness to light, but in place of that it has a different and most noteworthy property (*hochst merkwurdige Eigenschaft*), namely, that when a galvanometer is inserted between the gold leaf and the base-plate it shows an electrical current in the direction of the light action through the selenium, which continues as long as the gold leaf is illuminated. I conjectured at first that this current was not lasting, but had the character of a polarization current, which continued only until the molecular modification of the selenium by the illumination was completed, and a first experiment seemed to confirm this supposition.

* Read by the author before the Berlin Academy of Sciences on Feb. 12, 1885.

† *Monatsbericht der Berl. Akad. d. W. vom* 18. *Mai* 1875 *und* 7. *Juni* 1877.

But further trials convinced me that this idea was erroneous. In reality, we have here to do with an entirely new physical phenomenon, which is scientifically of the most far-reaching importance (*die von grosster wissenschaftlicher Tragweite ist*).

My experiments show that by the illumination of the gold leaf a difference of potential is established between it and the base-plate, which, to all appearance, is proportional to the intensity of the light, and which stands forth unchanged so long as the illumination remains constant. Obscure heat radiations do not produce electromotive force, and therefore the supposition of a thermo-electric action as the explanation of the phenomenon is excluded. Mr. Fritts holds that the light-waves penetrating the selenium are converted directly into electrical current, and therefore states as the fact the proportionality of the strength of the current to the strength of the light. They showed themselves nearly so, in the collated series of trials given in the following table:

Strength of the light in standard candles	6.4	9.9	12.8	16.8
Strength of the current	18.0	30.0	40.0	48.0
Quotient	2.8	3.0	3.1	2.8

The strength of the light was measured with a Bunsen's photometer; that of the current by the deflections of a sensitive mirror galvanometer.

With the gold leaf exposed to the light from the south-eastern portion of a cloudless sky, while the sun itself was hidden by the neigboring high buildings, the series of measurements in the following table were obtained:

Time of observation	9.37	10.05	10.30	11.00	11.35	12.00	12.30
Deflections of the galvanometer.	190	196	209	223	250	250	244
Time of observation	1.00	1.30	2.00	2.30	3.00	3.30	4.00
Deflections of the galvanometer.	245	349	228	188	173	172	108

It is seen from the foregoing that the electromotive force of the selenium pretty uniformly increased from 9.30 A. M. until 11.35 A. M., the current then remained almost constant during two hours, and thereafter again quite uniformly decreased till 3.00 P. M.

As to the reason why some of his selenium plates become more conductive during illumination, while others produce electromotive force, Mr. Fritts has no explanation to give. He complains of the uncertainty in the preparation of the plates, of whose property one can foresee nothing, and gives various manipulations by which one can often render unserviceable plates useful. There is yet required thorough investigation to determine upon what the electromotive light-action of many selenium plates depends. Nevertheless, the existence already of a selenium plate with the property described is a fact of the greatest scientific significance, since there is here presented to us, for the first time, the direct conversion of the energy of light into electrical energy (*eine Thatsache von grosster wissenschaftlicher Bedeutung, da uns hier zum ersten Male die directe Umwandlung der Energie des Lichtes in elektrische Energie entgegentritt*).

Note.—The uncertainties, etc., mentioned by Dr. Siemens as existing at the time I wrote the communication referred to (in February, 1884) have since been largely overcome. I have conclusively determined the conditions upon which the electromotive light action depends, and with more complete and perfect apparatus for preparing the plates, I believe that I can even now effect the conversion of more than 50 per cent. of the energy of light into electrical energy. If my theory proves to be complete, when fully carried out and tested in all its variations, we may ere long see the photo-electric plate competing with the dynamo-electric machine itself in high percentage of electrical conversion. In compactness, it is not designed to compete with it, being adapted and intended principally for what is known as "isolated" working—*i. e.*, for each building to have its own plant.

I am now able to foresee the general properties of the plates, and to give them such of the properties as I wish. I can cause a plate to show either an electromotive action or a higher conductivity when exposed to light, or to show both properties. In explanation of the slight irregularity in the action of the electromotive plate measured by Dr. Siemens, it should be stated that it was not prepared for that purpose, but was sent to him as exhibiting another and entirely different property. During the development of that property it had received some of the manipulations adapted for giving the electromotive action, and in consequence thereof it could generate a slight current upon exposure to light, although not with

the uniformity of a specially prepared electromotive plate. It will also be noted that "the south-eastern portion of a sky without clouds" (direct sunlight being excluded) is not a very intense source of illumination. But this fact renders the trial more satisfactory.

At the time of sending him the samples of my different constructions, I had not a very large number of the electro-motive plates, and they were in almost daily use in experiments from which to deduce the philosophy of the action, so that they could not be spared, and I contented myself with pointing out that the plate in question could generate a current, sufficient, at least, to prove its possession of that property. After having my papers, data and plates under consideration nearly a year, Dr. Siemens, than whom there is no higher or better authority, has accorded to me the honor of being the first one to produce electricity directly from light.

But I do not explain the action exactly as he supposes—that "the light-waves are converted directly into electrical current"—as is shown by the following statement concerning it, in my paper on "Selenium," read before the American Association for the Advancement of Science, at Philadelphia, Sept. 5, 1884: "The light * * passes through the gold and acts upon its junction with the selenium, developing an electromotive force which results in a current," when the circuit is completed. "The current thus produced is radiant energy converted into electrical energy directly and without chemical action." The final result is the same, of course, but the *rationale* is different.

In conclusion, I would say that however great the scientific importance of this discovery may be, its practical value will be no less obvious when we reflect that the supply of solar energy is both without limit and without cost, and that it will continue to pour down upon us for countless ages after all the coal deposits of the earth have been exhausted and forgotten.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers—Annual Convention of 1885.—The annual convention of this Society for the year 1885 will be held at Deer Park, Md., on the line of the Baltimore and Ohio Railroad, June 24th, 25th, 26th and 27th, 1885. Sessions for professional discussion and one for the transaction of business will be held.

By invitation of the officers of the Baltimore and Ohio Railroad Company, a steamer excursion will be made by members and their families on Monday afternoon, June 22d, to the Water Front of the Harbor of Baltimore, the marine terminals of the B. & O. R. R., Locust Point, Canton, Fort Carroll, Fort McHenry, and Curtis Bay, returning to Baltimore early in the evening.

Members and their families are invited to visit the summer evening concert, at the Baltimore Academy of Music, on Monday evening, under the escort of the Local Committee of Members of the Society and officers of the Baltimore and Ohio Railroad. This committee will have charge of all arrangements at Baltimore.

The sessions will be held during Wednesday, Thursday, Friday and Saturday, except that on one of those days, to be specially announced hereafter, an excursion will be made by invitation of the B. and O. R. R. to the Cheat River Grade, Kingwood Tunnel, Tray Run Viaduct, and other interesting points on the Mountain Division of the road.

Engineers' Club of Philadelphia—Record of Meeting, April 18th.—The following resolution, offered by Prof. L. M. Haupt, was unanimously adopted: That a committee be appointed to draft a memorial to the Legislature in behalf of the proposed topographical survey of this State, and to report at the next meeting.

A handsome framed engraving of the old Wernwag Bridge was presented to the Club.

This structure was designed and built by Louis Wernwag across the Schuylkill River, at Fairmount. The corner-stone was laid with Masonic ceremonies, April 28th, 1812. It was a wooden structure, with the bottom member arched—340 feet span, 20 feet rise. There were 5 trusses. The span was 98 feet greater than any other in the world at that time. Each truss was an open-built beam, formed of a bottom-curved solid-built beam and of a single top beam, which were connected by radial pieces, diagonal braces, and inclined iron stays. The bottom curved beam was composed of three concentric solid-built beams, slightly separated from each other, each of which had seven courses of curved scantling in it, each course 6 inches thick by 13 inches in breadth, the courses, as well as the concentric beams, being firmly united by iron bolts, etc. A roadway that rested upon the bottom curved ribs was left on each side of the center truss, and a foot-path between each of the two exterior trusses.

Mr. James F. Wood described the sewerage work now in progress at Atlantic City, N. J. The sewers are being laid, under contract, by Messrs. Robinson & Wallace, of New York, who expect to lay this year 600 feet of 24-inch pipe, 1,260 feet of 20-inch, and 4,000 feet of 15-inch, with their 6-inch connections.

Mr. Jones Wister presented a description of the Beaumont Rock Drill, which is said to bore

a 7-foot 2-inch hole at the rate of from 42 to 120 feet per week; to be suited to all rocks but the very hardest, and to avoid the leakage and foul air incident to the use of dynamite.

The workings of the drill were discussed by several members.

RECORD OF MEETING MAY 2D, 1885.—Prof. L. M. Haupt, chairman of committee appointed at last meeting to prepare a memorial to the Legislature of Pennsylvania, upon the continuation of the second geological survey of the State, presented the following report from that committee:

To the Honorable Lieutenant-Governor Chauncey F. Black, President, and the Honorable Amos H. Mylin, President, *pro tem*, of the Senate, and the Hon. James L. Graham, Speaker of the House of Representatives of Pennsylvania:

Whereas, The Engineers' Club of Philadelphia recognize the gross errors and discrepancies which exist in all of the best maps which have been constructed of Pennsylvania and of individual counties in the State, which have been published since 1816, and which are based upon Melish's State Map, constructed under authority of the Legislature of that year; and

Whereas, There is a great demand among those citizens of the State interested in all surface improvements and in the development of the natural resources of the State, for an accurate map, which shall establish the true geographical position of every point on its surface, north and south and east and west from every other point, and the elevation of all points above tide, so that the citizens of the commonwealth shall have correct maps of their respective townships and counties on which surface improvements may be outlined, and upon which surveyors can place land lines, and mining engineers and geologists can trace mineral outcrops and areas; and

Whereas, The Board of Commissioners of the Geological Survey have entered into negotiations with the United States Geological Survey, and the United States Coast and Geodetic Survey, and a plan of co-operation has been proposed by which the United States Surveys will expend in the State, for a geographical and topographical survey, $35,000 a year, if the State will expend $10,000 a year, and which co-operation now depends upon the granting of a State appropriation by the Legislature in accordance with the recommendations of the Board of Commissioners; and

Whereas, Such State maps are now being constructed by New York, Massachusetts and New Jersey, it is, therefore,

Resolved, By the Engineers' Club of Philadelphia, that they respectfully petition your Honorable bodies to speedily pass House Bill 497, entitled "An Act to provide for the continuance of the Geological Survey of the State," as originally introduced and committed to the Appropriation Committee of the House of Representatives, wherein an appropriation of $90,000 is asked, $20,000 of which provides for the continuance of topographical work and the commencement of the construction of an accurate map of the State, and your petitioners will ever pray.

Upon motion of the Secretary the report and resolution were unanimously adopted, and the chairman of the committee empowered to forward the same, in proper form, to the Senate and House and to the individual members thereof.

Mr. T. Everett Austin presented a paper on Fast Passenger Locomotives.

Mr. Kenneth Allen presented extracts from a paper by Henry J. Barnes, M. D., on Purification of Sewage by Application to Land, with especial reference to the City of Boston. Efficacy of system is endorsed by numerous authorities.

Mr. W. F. Newell, member of the Civil Engineer's Society of St. Paul, introduced by Prof. Haupt, presented a description of the Borup Coal Chute, which is designed to prevent accidents to pedestrians in street openings, for convenience in receiving or removing material, and for ventilation of cellars and vaults.

PROCEEDINGS OF THE ENGINEERS' CLUB OF ST. LOUIS.—ST. LOUIS, APRIL 29, 1885.—The Club was called to order by Pres. Moore, at the Mercantile Library.

Mr. M. L. Holman read a paper, "House to House Inspection to Prevent Water Waste."

The paper gave a recount of the inspection made in this city from December, 1883, to March, 1884.

A few trials with the Bell Waterphone were made, but it was found that the ordinary iron key used for turning on and off the water at the stop boxes was all that was needed. A portion of the end of the key-rod was flattened out so that the ear could be pressed tightly against it. After some practice this was used with great success.

In order to arrive at the good produced by house to house. combined with night inspection, one district of 118 houses inspected was selected, and 59 houses were all right, while 59 houses were wasting water. Previous to this inspection, all of the houses of this district had been inspected twice by regular day men, and twice by the night gang, followed each time by the day inspection of cases reported for wasting.

The writer concludes that night inspection is a very good method of house to house inspection, as it only puts those who are wasting water to the inconvenience of a day inspection.

The paper was generally discussed.

Mr. C. F. White read a paper on "Dynamometers."

The increase in the practice of selling power, and the growing need of exact information as to the power required for various classes of work, has made a demand for power-measuring machines.

AMERICAN INSTITUTE OF MINING ENGINEERS.—For the annual meeting at Chattanooga, the following papers are announced:

The Relative value of Coals to the Consumer, by H. M. Chance, Philadelphia, Pa.

Quicksilver Condensation at New Almaden, by S. B. Christy, Berkeley, Cal.

Notes on American Blast Furnaces, by John Birkenbine, Philadelphia, Pa.

Supplementary Paper on the No-Bosh Furnace, by W. J. Taylor, Chester, Pa.

The Durham Blast Furnace, by B. Fackenthal, Jr., Riegelsville, Pa.

The New Mining Code of Mexico, by R. E. Chism, Saltillo, Coahuila.

The Manufacture of Steel Castings, by Pedro G. Salom, Thurlow, Pa.

The Flow of Air and Other Gases, by Fred. W. Gordon, Philadelphia, Pa.

A Fire-brick Hot-blast Stove, by Victor O. Strobel. Philadelphia. Pa.

Minerals of the Sequachee Valley. Tenn., by W. M. Bowron, South Pittsburgh, Tenn.

Notes on some American Ore Deposits, by C. M. Rolker, New York City.

The Basic Bessemer Process, by Thomas Egleston, New York City.

ENGINEERING NOTES.

Qualitative Tests for Steel Rails—By Mr. L. Tetmajer.—This memoir is the first of a series upon the unification of nomenclature and classification of building materials undertaken by the author at the request of the Swiss Engineers' and Architects' Union. For its preparation numerous mechanical tests have been made upon steel rails, both good and bad, taken from the Swiss railways, while the corresponding chemical analyses have been made by Dr. Treadwell in the Polytechnic Laboratory, at Zurich. The results are given for twenty-two examples, about one-half of which have stood well, while the remainder have either broken, split, or suffered considerable abrasion in wear; but in many instances the mechanical test of tensile strength, elongation, and contraction, and the figures of quality (Wohler's sum and Tetmajer's coefficient) deduced from these have varied very considerably for the results obtained in practice. The best wearing rails, which often give contradictory results with the tensile test, were comparatively pure manganese steels, low in silicon, only exceptionally up to 0.2 per cent., but generally below 0.1 per cent., and with less than 0.1 per cent. of phosphorus and sulphur. On the other hand, rails with a tendency to break or split are low in carbon, with variable proportions of manganese, but contain much silicon, 0.3 to 0.9 per cent., and often above 0.1 per cent. of phosphorus. Another series of experiments upon rails for the Finland mines, made by the author in 1879–80, shows the high quality of manganese steel. These are essentially highly carburized (0.3–0.4 per cent. carbon) with 0.7 to 1.4 per cent. manganese, and have stood three and a-half years' wear without a single one being broken; while those of silicon steel with 0.106–0.144 per cent. carbon, 0.592–0.828 manganese, and 0.423–0.435 silicon, have failed in many cases, showing a great tendency to split. In both of the latter instances, however, the figures deduced from tensile tests of both good and bad specimens were substantially the same.

The causes of the difference between the two kinds of steel the author attributes to differences in the structure of the ingot due to the agent used in "chemical consolidation," which may be either manganese or silicon, which structures are illustrated by photographs of ingot fractures. When silicon is used there is a tendency to unsoundness about the exterior of the ingot, which is surrounded by a honeycomb-like cellular casing of greater or less depth; while with manganese the vesicular cavities are more or less dispersed through the whole substance, or concentrated towards the interior of the ingot. Rails made from the former are, therefore, more likely to contain unsound portions near the outer wearing surface, and to give unsatisfactory results in wear, than those from the latter; but as the test pieces are usually cut from the center of the rail-head, the tensile resistance of the inferior may be equal to or surpass that of the superior material.

In summing up his observations the author concludes that the method of tensile testing is mainly of value in determining the quality of the material, but that for the finished product properly arranged falling weight tests are necessary. He also considers that the test pieces should be flat bars of 2.5 to 3.5 centimeters in area, cut as near as possible to the outer surface of both head and foot of the rail. He reprobates especially the research for microscopic imperfections (mikrobensucherei) upon the fractured surfaces, as an annoyance to the producer, and perfectly useless to the consumer.—*Stahl und Eisen.*

IRON AND STEEL NOTES.

Final Report Bearing upon the Question of the Condition in which Carbon Exists in Steel.—The following are the conclusions which Sir Frederick Abel bases on the reports to the Mechanical Engineers:

"The results of the experimental work described appear to warrant the following conclusions in regard to characteristics, recognizable by chemical examination, which are exhibited by different portions of one and the same sample of steel presenting marked physical differences consequent upon their exposure to the hardening, annealing, or tempering processes.

"(1) In annealed steel the carbon exists entirely, or nearly so, in the form of a carbide of iron, of uniform composition (Fe_3C, or a multiple thereof), uniformly diffused through the mass of metallic iron.

"(2) The cold-rolled samples of steel examined were closely similar in this respect to the annealed steel, doubtless because of their having been annealed between the rollings.

"(3) In hardened steel the sudden lowering of the temperature from a high, red heat appears to have the effect of preventing or arresting the separation of the carbon, as a definite carbide, from the mass of the iron in which it exists in combination, its condition in the metal being, at any rate mainly, the same as when the steel is in a fused state. The presence of a small and variable proportion of Fe_3C in hardened steel is probably due to the una-

voidable and variable extent of imperfection, or want of suddenness, of the hardening operation, so that, in some slight and variable degree, the change due to annealing takes place prior to the fixing of the carbon by the hardening processes.

"(4) In tempered steel the condition of the carbon is intermediate between that of hardened and of annealed steel. The maintenance of hardened steel in a moderately heated state causes a gradual separation (within the mass) of the carbide molecules, the extent of which is regulated by the degree of heating, so that the metal gradually approaches in character to the annealed condition; but, even in the best result obtained with blue-tempered steel, that approach, as indicated by the proportion of separated carbide, is not more than about half-way towards the condition of annealed steel.

"(5) The carbide separated by chemical treatment from blue and straw-tempered steel has the same composition as that obtained from annealed steel.

"It does not appear that this inquiry can be further extended with the prospect of obtaining any additional facts—elucidating the condition of the carbon in steel exhibiting various physical characteristics—the value of which would bear any proportion to the very laborious nature of the necessary experimental work which has to be conducted with small quantities of material on account of the necessity of carying out the annealing, hardening and tempering processes with very thin pieces of steel.

"I believe it will be admitted that, although the data obtained have not led to the discovery of a ready chemical method of differentiating between different degrees of temper in steel (a method of examination which Prof. Hughes's interesting results have almost rendered unnecessary), they have at any rate contributed to the advancement of our knowledge of the nature of steel."

The Clapp-Griffiths Steel Process.—At the last meeting of the American Institute of Mining Engineers held at New York City, the contributions of Messrs. J. P. Witherow, of Pittsburgh, and Robert W. Hunt, of Troy, upon the Clapp-Griffiths process of steel manufacture were of great interest. An experimental plant was put up by Messrs. Oliver Bros. & Phillips at Pittsburgh, on the plan of one at Wales. Two thousand tons have been made at these works and sold to the trade with the most favorable results. Mr. Hunt stated that a complete plant, including buildings, with a capacity of 80 gross tons per twenty-four hours, can be erected for $55,000, and that the cost of steel produced by this method varies from $21.95 to $23.10 per ton, according to the quality of pig iron used. With an additional expenditure of $30.000 a blast furnace plant could be equipped with a steelmaking plant, which would take the molten iron from the blast furnace and convert it into steel at an expenditure of $4 per ton, making a product of nearly double the value of pig iron. Such work would leave the puddlers decidedly in the cold, as it costs $12.50 at Pittsburgh to convert a ton of pig iron into muck bar. The most important phase of this new process is the new relation which phosphorus bears to this new product, for this steel with high phosphorus and low silicon gave satisfactory results. A sample containing the following elements:

Carbon	0.08	per cent.
Silicon	0.01	"
Phosphorus	0.50	"
Manganese	0.48	"
Sulphur	0 09	"

gave the following physical results:

Tensile strength	80,940 lbs.
Elastic limit	58,570 lbs.
Elongation	24 per cent.
Reduction of area	$36\frac{4}{10}$ "

This question assumes great importance in view of the fact of the increasing demand for low steels for nails, wire, bolts, agricultural implements, and many other purposes.—*Engineering.*

Clemandot's Method of Tempering Steel by Compression.—By A. Carnot.—Mr. Clemandot's method consists in heating the metal till it acquires sufficient ductility, and applying great pressure while it cools. This operation modifies the structure of the metal, and imparts to it qualities similar to, though not identical with, those developed by tempering. The resulting metal differs very sensibly from that which is cooled naturally, the grain being much finer, and the hardness and resistance to rupture much greater. Two different and almost simultaneous effects are produced—first, energetic and continuous compression; second, rapid cooling. The latter is caused by contact with the plates of the hydraulic press. Its effect is similar to that produced by cooling in water, while that of the compression resembles the effect of hammering. It is sufficient if the pressure is applied to two faces only. The steel should be heated to a cherry red, and the pressure be brought to the required point (which may be 6, 12, or 19 tons per square inch) as rapidly as possible. In tempering by immersion the volume of the steel is increased and its density diminished, but this is not the case in the new process. The metal becomes very hard without losing its ductility, especially in steels with a large percentage of carbon.

Hitherto there has appeared a slight inferiority in the magnetic properties of the compressed steel, but this is compensated for by other advantages. It is well known that water-tempered steel when re-heated to redness returns almost to its natural state, and loses its magnetic properties. This is not the case with that tempered by compression, which can be re-heated, and even forged, without becoming demagnetized. This steel also retains the fineness of its grain after re-heating. Thus a bar of steel bent into a U-shape and then compressed, and a bar of the same weight compressed when straight, then re-heated and bent at the forge to the same shape as the first, would each support about twenty times its own weight, after having been magnetized to saturation.

It is thought that the magnetic force might be increased by submitting a bar already compressed to a second compression after forging;

and bars that have been compressed may afterwards be tempered in water, and be converted into powerful magnets.

RAILWAY NOTES.

RAILWAY ACCIDENTS COMPARED.—In an article on "Modern Railroad Facilities," contributed by Mr. Barnett Le Van to the *Journal* of the Franklin Institute, the author states that, from railway mortality statistics of England and some parts of the Continent of Europe recently compiled and published in France, it appears that the greatest fatality occurs on the French railways. French railways annually kill one in every 2,000,000; English railways, one in every 5,250,000; Belgian railways, one in every 9,000,000; and Prussian railways, one in every 21,500,000. With reference to injuries, it is stated that French railways wound annually one in every 500,000; English railways, one in every 750,000; Belgian railways, one in every 9,000,000; Prussian railways, one in every 4,000,000. In round numbers, French railways kill five times as many as English, English not quite twice as many as Belgian, and Belgian nearer three times than twice as many as Prussian, which are much the least fatal of the four.

THE ARGENTINE RAILWAYS.—Great progress is being made with the railways of the Argentine Republic, the work of construction being pushed forward with all possible speed. A new section of the Andine line from Mendoza to San Juan, a distance of 95 miles, was completed at the end of March, and was to be opened at once. The entire distance from Rosario to San Juan, 625 miles, is now done in a three-day journey. A few years ago the journey took from twenty to twenty-five days, and the freight by bullock carts was so costly that the products of the Andine province could not be taken to the coast. The railway from Mercedes to Rio Quinto—360 miles—is being pushed forward, and is expected to be ready for traffic this month (May). The Great Northern Railway is rapidly advancing to Salta, a rich agricultural province, distant from Rosario 775 miles. The wealth of that province is stated to have been doubled within the last ten years.

THE work of building the new "island" railway station at Rugby, which is to cost some £70,000, and to be the biggest thing of its kind in Europe, continues to be rapidly pushed forward. Although it is expected that quite another year must elapse before the alterations are completed, yet it is hoped that the down side will be ready for use by about June next. The line has been widened 150 ft. southwards, so as to give room for nine sets altogether of through metals instead of four as now. For the greater safety at the junctions of the new Northampton line of the Stamford branch important works, presenting considerable engineering difficulties have been effected. The traffic will be regulated by large signal boxes at each end of the platform, that at the south end having 180 levers.

MR. P. A. PLEWE, well-known on the Continent as the originator of the large and perfect tramway network, Die Grosse Berliner Pferde-Eisenbahn, and who is also the sole concessionaire of the Brush system of electric lighting for Germany, is now introducing Reckenzaun's system of propulsion on the above-mentioned tramways, and the first of a series of cars is now in course of construction. Mr. Plewe has been in London recently for the purpose of examining the car which is now on the South London lines, and on the strength of favorable reports from independent scientific experts, the permission of the Municipality and the Berliner Pferde-Eisenbahn Gesellschaft was obtained. The first car, which will be fitted internally with unusual elegance, is to be ready for the opening of the Japanese Exhibition next month, and it will be running between the Spittel Market and Bauer's Ausstellungs Park —Alt-Moabit—a distance of about four kilometers, from the city to the west end of the city of Berlin.

ORDNANCE AND NAVAL.

HIGH EXPLOSIVES IN SHELLS.—Some recent experiments upon the use of shells charged with high explosives were recently made by officers of the United States Ordnance Department before the military committee of the United States Senate and a number of members of the foreign legations. Four shots were fired from a range of 1,000 ft. against a precipitous ledge of trap rock. These 6-in shells contained an 11-lb. charge of nitro-gelatine which consisted of 95 per cent. nitro-glycerine and 5 per cent. gelatine, and were exploded by the impact of their concussion against the ledge. The explosions of these shells excavated a cavity in the face of the ledge, throwing the fragments back half a mile in each direction. In view of the effects of this work the local authorities are not willing to permit experiments with shells containing 35-lb. charges, and the experiments will probably be continued at Fortress Monroe. Other army officers have been experimenting on a projectile charged with dynamite, and thrown by condensed air. One of the old fortifications near to New York City is shortly to be dismantled and taken down, and the proposition has been made to test these projectiles upon the old fort. Some of the officers at the torpedo station at Newport, R. I., exploded torpedoes charged with 34 lbs. of gun-cotton, which were placed on the ice of a pond. The explosion is said to have blown holes in the ice 40 ft. across, and thrown ice and water 200 feet into the air.—*Engineering.*

THE Japanese war-ship, Naniwa-Kan, a cruiser recently built by Messrs. Sir W. G. Armstrong, Mitchell, & Co., for the Japanese Government, was designed by Mr. W. H. White, of Elswick, and, like the Esmeralda, has been constructed so as to combine great speed with great offensive power. The Naniwa-Kan and a sister ship, which is nearing completion, are the largest vessels ever built by the Elswick firm, and when delivered to the Japanese Government will be the swiftest and

most heavily armed cruisers afloat. In dimensions the new cruisers are almost identical with the Iris and Mercury, despatch vessels of the Royal Navy, and the Leander class of partially-protected cruisers. They are 300ft. in length, 46ft. in breadth, draw 18½ft. of water, and are of about 3,600 tons displacement. They have twin-screw engines, which are to develop 7,500 horse-power at least, and their estimated speed is from 18 to 18½ knots. The armament includes two 28-ton 26-centimeter guns, mounted on center-pivot automatic carriages, as bow and stern chasers. These heavy guns are worked and loaded by means of hydraulic mechanism, which is an improvement on that fitted in the Esmeralda. On each broadside there are three 15-centimeter guns of 5 tons each, also on center-pivot automatic carriages of Elswick design, and along the broadsides there are also placed no less than ten 1in. machine guns, and two rapid fire-guns. There are two military masts, in the tops of which four of the improved Gatling guns made at Elswick will be mounted. All the guns except those in the tops are carried on the upper deck, and all of them have strong steel shields protecting the guns and crews from rifle and machine gun fire. Besides the gun armament, each vessel will have a complete armament of locomotive torpedoes ejected from four stations—two on each broadside, situated at a small height above water.

Sheffield Steel for Heavy Ordnance.—The Surveyor-General of Ordnance—Mr. Brand—made a statement in Parliament on Monday night which is pleasant reading for Sheffield manufacturers. He said the Government having determined to encourage the production of large steel forging in Sheffield, were now doing as much as possible to give the great Sheffield firms an opportunity of recouping themselves in some degree for their heavy outlay. He assured the House that it was not a question of months, but of weeks, before the Sheffield firms would be in a position to supply large steel forgings for ordnance. Towards this end Messrs. Thomas Firth and Co., Messrs. Charles Cammell and Co., and Messrs. Vickers, Sons, and Co., were making energetic efforts, and in a very short time they would be able to execute any orders with which the Government might favor them. Mr. Brand added that similar efforts had been made by Sir W. G. Armstrong and Co., at Elswick, "so that at the present time forgings of this heavy character would be produced at home entirely to the satisfaction of the Government." Mr. Brand's references to Sheffield are scarcely complete. Messrs. John Brown and Co. are also exhibiting similar enterprise, and are well forward with their work. The particular direction in which the extensions are being made is not in the putting down of heavier hammers for the manipulation of large masses of steel, but in the erection of forging presses, which are believed to be more advantageous in dealing with great ingots. The four Sheffield firms mentioned are at present expending at least £200,000 to £250,000 on new forging presses, with the necessary adjuncts of powerful cranes, furnaces, &c. Messrs. Vickers, Sons, & Co., are about ready for operations. Messrs. Cammell and Co. are erecting a special building to hold their press. Messrs. John Brown and Co. are able to utilize their rail mill. There is not the slightest doubt of the Sheffield manufacturers being able to do all that is required of them in the future as they have done in the past. According to Sir Thomas Brassey, the Government is preparing to spend on shipbuilding £3,000,000, or double the amount placed at the disposal of any other naval administration, their scheme for the strengthening of the Navy including four ironclads, five belted cruisers, torpedo ram, eight scouts, five gun vessels, and fifteen torpedo boats, of which ten were to be ordered at once. This list implies a good deal of work in steel, but it is altogether exclusive of the heavy ordnance needed, not only for arming ships, but for land defences. It is chiefly for these monster guns that the Sheffield firms are at present expending money so freely. It should not be forgotten that Mr. John Haswell, formerly locomotive superintendent of the Austrian State Railways, was the originator of the forging press, and for many years he has used his presses on a large scale and with perfect success in Vienna.—*Engineer*.

Dynamite Shells.—The *San Francisco Chronicle* gives an account of the recent experiments with dynamite shells at Port Lobos. It quotes General Kelton as saying of them: "The experiments were made under my charge, and with the authority of the chief of ordnance. The piece of ordnance used was a condemned 3-inch rifle gun, made of wrought iron; the gun was a sound one, save that it had become honeycombed by use and exposure to weather; it was a good gun for the experiments. I was ably assisted by Mr. Quinan, officer of the 4th United States Artillery, who resigned to undertake the hazardous business of improving the methods of manufacture of high explosives, for which task his scientific attainments eminently fitted him. Experiments of the kind in question need the supervision of an expert in high explosives, and Mr. Quinan's knowledge of dynamite came into great service. Mr. Quinan in person loaded the shells, each shell, an elongated 3-inch rifle projectile, being charged with 7 oz. of dynamite. The selected place of experiment was Lobos Beach, with the ocean on one side and a precipitous cliff on the other, the place being selected so that no possible danger could occur to any one. When the gun was fired, our party was over 100 yards from the piece, and under protection. The gun was placed in position 150 yards in front of a huge rock. The first projecting charge was a ¼ lb of cannon powder. The rock was struck by the shell, the dynamite ignited by percussion, and the shell broken into innumerable fragments, whereas by ordinary powder it would only have broken into a few large fragments. The second charge was ½ lb. of cannon powder, and the experiment was attended with equally good results. It did just what was expected; the shell was expelled, and did not ignite until it struck the rock. The

third charge was a pound of powder, service charge. When the gun was fired, the explosion of the charge, the bursting of the shell, and the shattering of the gun, appeared to be simultaneous. The gun was torn into fragments. One fragment, including the breech, and weighing about 200 lbs., was hurled to the rear fully 20 feet; the muzzle part hung to the carriage by a trunnion, the carriage being only slightly injured; the third fragment of the gun, weighing several hundred pounds, flew high in the air in a nearly vertical course and over the cliff; the immense piece of iron went up fully 90 feet. Then, as a matter of course, our experiments for the day ceased." The results of the experiments were, in the opinion of General Kelton, "exceedingly satisfactory, for they conclusively showed that shell loaded with dynamite can be used in warfare. Seven ounces of dynamite rent the gun as a charge of 100 lbs. powder could not have done. Powder would have opened a fissure in the iron, thus permitting the gas generated by its combustion to escape; but while the combustion of powder, while rapid, is progressive, the combustion of dynamite is so instantaneous that the enormous volume of gas thereby generated seems to want to escape at once. This fact was shown by the sudden rending of the gun into fragments. If the dynamite shell should strike the side of the vessel and explode without penetrating the armor, the destructive effect would be greatly in excess of the damage worked by the ordinary shell made of gunpowder. But the dynamite shell must penetrate to some extent to produce its full effect. I am satisfied that experiments will show that it can easily be managed to give the shell the power to thus penetrate before it explodes. The necessary penetration—about one-half the length of the shell, would be effected in the thousandth part of a second after it had reached the ship. Then the exploding dynamite would instantaneously rend asunder the entire side of an ironclad. In defending a fort against a land attack these dynamite shells would be very effective. One of these shells exploding in the midst of a body of attacking troops would produce as much consternation as a thunderbolt; its explosion would be like unto the explosion of a powder magazine in their very midst. No troops in the world, however brave, could stand more than a few of such shells. So destructive, in fact, would be these shells that their introduction in active warfare would vastly diminish the duration of wars, if it did not make wars an impossibility." In conclusion, General Kelton expressed satisfaction that the experiments had been so successful. While experiments had been made by others, he did not think that any had gone so far or succeeded so well. Captain Daniel M. Taylor, of the ordnance department, and an aide-de-camp on General Pope's staff, said: "The experiments conducted so successfully by General Kelton show that a compound many times more destructive than gunpowder will add to the havoc of the battlefield in future wars. One peculiar property of dynamite may somewhat interfere with its usefulness as a destroying and rending agent, and that is the fact, authenticated by experiments, that its destroying power operates vertically and with its main effect in a downward direction; in other words, a dynamite-charged shell would not scatter death and destruction in every direction, as a gunpowder-charged shell so frequently does." Captain James Chester, of the 3d Artillery, has paid great attention to the subject of dynamite in its connection with the art of war. He maintains that dynamite can be used with great success in active warfare if rockets are employed to throw the death-bearing material into the ranks of the enemy. He holds that dynamite shells can be thrown by means of the rocket with fair accuracy and to very long ranges. He calls these rocket-propelled shells aerial torpedoes, in contradistinction to submarine torpedoes, and holds that, with the submarine torpedo defence of the navy, and the aerial defence in the hands of the army, the country would be safe against any attack.

BOOK NOTICES

Publications Received.

Monthly Weather Review for March. 1885. Washington: Signal Office.

The Popular Science Monthly—June. New York: D. Appleton & Co.

Transactions of the American Institute of Mining Engineers (advanced sheets).

Oxygen and Ozone. By Prof. Anton Stamm.

The Copper-Bearing Rocks of Lake Superior. By Roland Duer Irving. Washington: Government Printing Office.

This is one of the component parts of the general report of the United States Geological Survey under Clarence King.

The separate chapters of this elegant quarto volume present, with fine illustrations and maps, the following:

Extent and General Nature of the Keweenaw Series, Lithology of the Keweenaw Series, Structural Features of the Three Classes of Rocks of the Series, General Stratigraphy of the Keweenaw Series, The Keweenawan Rocks of the South Shore of Lake Superior, The Keweenawan Rocks of the North and East Shores of Lake Superior, Relations of the Keweenawan Rocks to the Associated Formations, Structure of the Lake Superior Basin, The Copper Deposits.

Colored lithographs of Microscopic rock sections form a conspicuous feature of the book. The folding maps and geological sections are also beautifully colored.

Report of the Commissioner of Navigation for 1884. Washington: Government Printing Office.

The present report contains for a preface the law establishing the Bureau of Navigation.

Then follows the report proper, including a historical retrospect of the birth and growth of the maritime commerce of our country. A brief account of the merchant marine of France, of Norway, and of Italy then follows. Then, in order, are presented a statistical account of the lake and ocean commerce of the United States, the English Law (regarding seamen), the French Law, the German Law, the Norwegian

Law, the Italian Law, the Belgian Law, the Netherlands Law, Pilotage in United States Ports, the Laws Governing Coasting Trade of the above-mentioned countries, and the Law Governing Collisions at Sea.

A tabulated statement of tonnage of the United States Merchant Marine closes the report.

APPLIED MECHANICS. By GAETANO LANZA, S. B., C. E. New York: John Wiley & Sons.

As Prof. Lanza explains in his preface, "the work is essentially a treatise on strength and stability; but inasmuch as it contains some other matter, it was thought best to call it Applied Mechanics, notwithstanding the fact that a number of subjects usually included in treatises on Applied Mechanics, are left out."

What the work does contain may be substantially inferred from the list of topics, to each of which a chapter is devoted:

I. Composition and Resolution of Forces. II. Dynamics. III. Roof Trusses. IV. Bridge Trusses. V. Center of Gravity. VI. Strength of Materials. VII. Strength of Materials as determined by Experiment. VIII. Continuous Girders. IX. Equilibrium Curves; Arches and Domes. X. Theory of Elasticity and Applications.

When it is added that the text of these ten chapters covers seven hundred and twenty pages octavo, it will seem to the reader that the topics are treated with something more than ordinary fullness In short, we believe the work is admirably adapted for use as a text-book for students in engineering. Of course the omissions referred to by the author must be supplied from other sources before the pupil can complete Applied Mechanics, but within the range of topics specified above, we know of no book better adapted to the wants of American students than this.

BALLOONING. By G. MAY. London: Symans & Co. New York: D. Van Nostrand.

This neatly-printed little volume presents a concise sketch of the history of ballooning, and discusses the obstacles that inventors have sought to overcome.

The scientific researches that have been conducted by aid of balloons receive a fair share of attention under the heading of Practical Applications of Aeronautics. This would presumably include the military use, but the author has devoted a separate chapter to this phase of practical use.

More space, however, is devoted to the prospective service of balloons when adequate steering power shall have been devised.

The author concludes, "It would be venturesome to assert that aerial navigation has no future, looking to what it has accomplished during the past century. Nevertheless, considering the wonderful strides art and scientific progress have made in later years, it is quite within the scope of discovery to contrive some appliance partly to effect a more practical solution of the difficult problem of balloon steerng."

COMSTOCK MINING AND MINERS. By ELIOT LORD. Washington: Government Printing Office.

This report forms Monograph No. 4 of the United States Geological Survey. "It is," says the preface, "the record of a struggle which has materially affected the mining interests of the world. It is the story of the birth of the silver mining industry in this country, and it portrays, as well, the most vigorous growth of that industry. The simple narrative is, in truth, not less marvelous than an Arabian tale, recounting, as it does, how a handful of earth tossed away carelessly by a poor immigrant became the loadstone which drew a swarm of men to a desert avoided even by beasts, and how from this clue a thread of gold was traced to its hidden source, and treasures rivaling the fancied store of Aladdin were unveiled.

The history, though encumbered here and there with dry details of legal and commercial transactions, fulfills the promise implied in the preface.

Three well-engraved maps embellish the work.

MISCELLANEOUS.

THE New South Wales shale yields, on an average, about 150 gallons of crude oil per ton, which contains over 60 per cent. of refined kerosene oil, and the remaining products consist of gasoline, benzine, spongaline, paraffine, wood-preserving composition, and lubricating oil. Its gas-producing capabilities amount to the large yield of over 18,000 cubic feet of gas, with an illuminating power of 38 to 40 candles. On this account it has been found advantageous for mixing with ordinary coal in the manufacture of gas.

ENGLISH COAL IN RUSSIA.—A short time ago the managing engineer of the South-West Russian Railroad sent in a report to the directors of the line, stating that the English coal supplied for the use of the locomotives was of a very inferior quality, and recommending the adoption of certain measures; among others, the dispatch of an agent to England, to investigate matters, in order to put an end to the evil. In reply, according to the Odessa papers, the directors have declared the adoption of any special measures unnecessary, since they intend shortly to suspend the use of English coal on the line. Whether Russian coal, or petroleum refuse, is to be used instead, the directors do not say; thus leaving the matter open for the newspapers to fight about. Some assert that Russian coal from the Donetz Valley is to be used on all the lines stretching from the Black Sea to the Austrian frontier; others, that the South-West Railway Company dislike the native fuel even more than the English, and are determined to adopt the practice of the Volga lines, and burn petroleum refuse.

AT a recent meeting of the Cambridge Philosophical Society, a communication was made on the measurement of electric currents, by Lord Rayleigh. The author referred to the method of measuring currents by the silver

voltameter as suitable for currents of from .05 amperes to 4 amperes, and stated that the electro-chemical equivalent of silver, as determined at the Cavendish Laboratory, was 1.119×10^{-3}. A second method was described, suitable for larger currents. It consists in balancing the difference of potential between two points in the circuit through which the current is running against the effects of a standard cell working through a large resistance, such as 10,000 ohms. The author suggested, as a third method, the use of the rotation of the plane of polarization of light passing through a piece of heavy glass, around which the current circulates in a coil of thick wire. A current of 40 amperes will produce a rotation of 15 deg. if the coil have one hundred turns.

At a recent meeting of the Paris Academy of Sciences, a note on "The Universal Hour Proposed by the Conference in Rome," was communicated by M. Faye. The author urged several objections against the adoption of Greenwich astronomical time and meridian, calculating the longitude from 0 to 24 h. east, which might be convenient for navigation and astronomical purposes, but unsuitable for railways, telegraph, government offices, and the public generally. For the formula, universal time=local time−(L+12 h.), where L indicates the longitude calculated east from Greenwich, he proposes to substitute, universal time=local time−L. The formula would thus be simplified by the suppression of the last term, and, instead of Greenwich astronomical time, the civil hour would be adopted as the universal hour. Thus would be avoided the inconvenience of disagreement between local and universal time, which would otherwise be felt precisely in the most densely-peopled regions of the globe.

At a recent meeting of the Chemical Society, a paper was read on "Toughened Filter-paper," by E. E. H. Francis. Filter-paper which has been immersed in nitric acid, rel. den. 1.42, and washed with water, is remarkably toughened, the product being pervious to liquids, and quite different from parchment paper made with sulphuric acid. Such paper can be washed and rubbed without damage, like a piece of linen. The paper contracts in size under the treatment, and the ash is diminished; it undergoes a slight decrease in weight, and contains no nitrogen. Whereas a loop formed from a strip 25 mm. wide of ordinary Swedish paper gave way when weighted with 100—150 grams, a similar loop of toughened paper bore a weight of about 1.5 kilogs. The toughened paper can be used with the vacuum pump in ordinary funnels without extra support, and fits sufficiently close to prevent undue access of air, which is not the case with parchment paper. An admirable way of preparing filters for the pump is to dip only the apex of the folded paper into nitric acid, and then wash with water; the weak part is thus effectually toughened.

Mr. Thomas Kay, of Stockport, lately read a paper before the Manchester Literary and Philosophical Society in which he suggested a method of making sea-water potable by precipitation. He suggests that every ship's boat should be supplied with a quantity of citrate of silver, which should be used for precipitating the chlorides, leaving the sodium, potassium, magnesium, and other constituents in solution as citrates. The solution would be similar to ordinary effervescing draughts after the gas has escaped; it would be slightly aperient or slightly diuretic if taken in too large quantities, but still suitable for moistening the parched mouth. The expense of the silver would be but a small addition to the capital sunk in a ship, and the interest on it would be a small insurance premium against thirst in case of disaster. The value of the silver would not decrease, and could always be realized if disaster did not occur. The scheme seems practicable if the solution of citrates is sufficiently weak to be potable; only experience can prove this. The silver, being very portable, not easily identified, and easily reduced to metallic silver, would offer great temptations to petty larceny.

"Improvements in the Manufacture of Cement" forms the subject of a good deal of recent patent literature. Mr. R. Stone's improvement in the process of manufacture consists essentially in grinding the materials in a red-hot state, giving a better prepared material with less consumption of power. The burnt clinkers pass red-hot through steel-crushing rolls of special construction, and thence to the grinding rollers, the bed-plate of which is made concave and adapted to the lower roll, so that the crushed material after passing between the rollers is further ground between the lower roll and the bed-plate. Mr F. W. Gerhard adds lime to certain impure silicates of alumina and iron, and claims to make cement from the rotch bat or bovin, and what is known as black lime in the Wolverhampton district. Mr. E. W. Harding claims the application of gaseous fuel, in kilns of special construction, to the burning of Portland cement, together with the utilization of the waste heat, for the carbonization of coal and the heating of the drying floors; also the mixing of a certain proportion of resin with the cement mixtures to assist the calcination. Messrs. R. W. Lesley and J. M. Wilcox seek to mould cement powder into forms suitable for the kilns, as respects size, adaptability to free draught, and the like, while dispensing nearly, if not altogether, with the water ordinarily required to bring it to the pasty condition for this purpose. They compress the powdered materials, damped by heavy pressure between rollers having cells or cavities wherein the powder is molded into blocks of the requisite size. Messrs. J. W. Matteson, W. J. Chapman, and T. G. Matteson seek to improve the quality of the mixture of chalk, clay, and water in cement making, technically known as slurry, and obtain at a particular stage of the manufacture a more complete admixture and homogeneity of the materials, with perfect disintegration, and free from small particles of chalk hitherto met with in all known processes of manufacture. The apparatus consists of a rotary sieve or tempse, preferably of conical shape, revolving in a well provided with an exit pipe at one end.